9. The Scandinavian Tag-Team: Providers of atmospheric reality to numerical weather prediction efforts in the United States (1948-1955)

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Introduction

In late 1945, the distinguished mathematician John von Neumann needed a suitably difficult scientific problem amenable to a numerical solution to showcase the capabilities of his proposed computer. Although there were numerous candidates from the physical sciences[1], von Neumann settled on the weather prediction problem. In their brief accounts of the development of numerical weather prediction, William Aspray's *John von Neumann and the Origins of Modern Computing* and Frederik Nebeker's *Calculating the Weather: Meteorology in the 20th Century* give von Neumann primary credit for starting and leading the Meteorology Project at the Institute for Advanced Study. Given significantly less credit are Carl-Gustav Rossby, Jule Charney, and a series of Scandinavian meteorologists who significantly influenced the entire project.[2] I will argue that the Scandinavian "tag-team", invited by Charney and supported by Rossby, was not only critical to its ultimate success, but that differences in the cultures of American and Scandinavian meteorology made the Scandinavians better suited to accomplish the work at hand than their American counterparts. That the Scandinavians possessed both practical forecasting skills as well as superb analysis and theoretical knowledge enabled the answering of this question: Is the computer predicted representation of the atmosphere a valid one?

The Beginnings of the Meteorology Project

John von Neumann's quest to prove the worth of an electronic computing device needed a sufficiently complex problem to fully exploit its power and be practical enough to attract funding and public attention. By the end of 1945, the Navy was interested in funding von Neumann's efforts without restriction.[3] World War II's war fighting and countermeasures efforts had been heavily based on applied physics. The advantages presented by a computer for problem solving as well as strategic and tactical decision making could not have been lost on Navy officials.

However, after discussing the issue with Vladimir Zworykin of the Radio Corporation of America's Princeton laboratories, the weather prediction problem started to look appealing. Von Neumann had no meteorology background, but he had worked on hydrodynamical problems. Weather prediction was a problem whose solution was potentially useful to everyone. A non-linear mathematical description of a moving ocean of air surrounding a rotating body was ideal because it could only be attacked numerically, not analytically.

Having met several times with Weather Bureau personnel, in early February 1946, Harry Wexler, Director of Research, encouraged von Neumann to present his ideas to Carl-Gustav Rossby.[4] Rossby, of the University of Chicago's Department of Meteorology, was probably the most influential academic meteorologist in the United States. A naturalized U.S. citizen from Sweden, Rossby had spearheaded the training of several thousand military personnel in meteorology during World War II. A strong proponent of theory-based meteorology, he had many ideas about moving the art of weather prediction into a solid scientific endeavor.

Von Neumann proposed to Rossby that the general circulation of the atmosphere be modeled in its most "simplified and schematic form" – a homogeneous rotating earth that allowed for variations in incoming solar radiation by latitude, but assumed that all data were zonal, i.e., longitudinal placement had no effect.[5] Rossby subsequently met with von Neumann and reported the substance of the meeting to Francis W. Reichelderfer, Chief of the Weather Bureau. Von Neumann's interest in the pursuit of basic meteorological problems by reducing the dynamic equations to a computer solvable version was a potentially great asset to the progress of the science. Rossby wrote, "To stimulate him further, and to lay the foundation for a computational approach to theoretical meteorology, it would be necessary to organize a small and versatile group of competent theoretical meteorologists to work with Professor von Neumann".[6]

Von Neumann's requirement for additional stimulation is debatable. The need for a theoretical foundation to allow for a computational solution to meteorological problems is not. The mathematical approach to meteorology had been weak before the war. The existing hydrodynamical equations had to be simplified to be numerically solvable. The resulting simplification had to produce a realistic representation of the atmosphere.

Therein lay the primary problem outside of building the new computer: who would create such a simplified mathematical system? Rossby created a list of those who might be both available and capable of addressing this problem. He suggested to von Neumann that he propose a research project to the Navy's Office of Research and Inventions (later the Office of Naval Research – ONR) to be based at IAS. Due to discussions with the Navy's LCDR Daniel F. Rex, he knew the Navy would make a renewable annual funding commitment. The project would "examine the foundation of our ideas concerning the general circulation of the atmosphere with the intention of determining the steady state of the general circulation of the atmosphere and its response to arbitrarily applied external influences". Having arranged the contract, he provided von Neumann with a complete budget proposal, personnel suggestions and a proposed starting date.[7] As von Neumann himself later wrote to Rossby in 1949, "[you] more than anyone else were responsible for getting theoretical work started at IAS under the auspices of the contract with ONR".[8]

With the addition of technical information on the proposed computer, the contract proposal echoing Rossby's discussion went to the Navy. The proposed personnel would be younger meteorologists led by Harry Wexler and prominent consultants including meteorologists and oceanographers C.-G. Rossby, Harald Sverdrup and Jacob Bjerknes – all Scandinavians.[9]

With the contract successfully negotiated, the Meteorology Project became a reality on July 1, 1946. A meeting of some 20 prominent meteorologists in late August established the working assignments. However, for a variety of reasons the personnel did not materialize. After consulting Rossby and Reichelderfer, von Neumann reorganized the Project into a smaller group. Only two people worked full-time (Paul Queney of France, and Albert Cahn, University of Chicago) and they left in December. Israeli Chaim Pekeris and New York University's Hans Panofsky worked1/3 time and Army Captain Gilbert Hunt, a mathematician, participated in some discussions. Army-Air Force Lieutenant Philip D. Thompson took over Cahn's work on the hydrodynamical equations. As 1947 dawned, only one person worked full-time for the Meteorology "group" – Thompson.[10] His meteorology background was due to wartime training at the University of Chicago, he was largely self-taught beyond the basics, and he lacked familiarity with the existing literature. However, he did have some very good ideas. Jule Charney, who had attended the August 1946 meeting in Princeton, visited the group in March 1947 just before departing for the University of Oslo. Writing to Rossby, he noted that the meteorological aspect was the "weak sister" of the Meteorology Project which did not exactly bode well for its success.[11]

Von Neumann focused on numerical solutions to the hydrodynamical equations, but of necessity his efforts were divided between the short-handed meteorology group and the Computer Project itself. Since little could be accomplished by the isolated Thompson, the Project was extremely fortunate that Charney was developing ways to numerically treat the dynamic equations of the atmosphere. By the end of 1947, he had found a "filtering" method which would remove "noise" – energy waves that did not contribute to the solution, but which complicated the calculations.[12] Von Neumann was pleased when he found out that Charney was interested in the Meteorology Project. He also inquired as to the availability of Arnt Eliassen, a Norwegian meteorologist who had been collaborating with Charney.[13]

Charney, who had been offered a position at UCLA by Jacob Bjerknes,[14] accepted von Neumann's offer in part because he was concerned about the Project's future if it continued on its existing path. In writing to Bjerknes, Charney made clear his view, "unless some physical ideas are brought to bear, the project will die out through mathematical sterility".[15] Charney also recognized that the addition of physics would not be enough. The Meteorology Project needed people who knew enough about meteorology to know "when and how to make approximations" that would inevitably be required to reduce the applicable equations to a computer solvable version. To this end Charney recommended bringing in Arnt Eliassen from the Norwegian Meteorological Institute.[16]

The Scandinavian Tag-Team Begins

Arnt Eliassen thus unwittingly became the first in a line of Scandinavians to join the Project. Eliassen possessed the ideal combination of skills: experienced in synoptic and theoretical meteorology, he was also interested in numerical solutions. As Charney saw it, the Project needed to be combining theoretical work with empirical data. That meant that some members of the group had to have "intimate experience with actual weather processes".[17] Eliassen was the only member of the group who met that requirement.

Charney and Eliassen arrived in Princeton in the summer of 1948 joining Thompson and Hunt.[18] Not on scene, but a major player nonetheless, was its primary meteorological supporter – the always forward looking Rossby.

If this group was focused on the development of meteorological theory, why the need for personnel with synoptic experience? Synoptic meteorology relied on data collected worldwide and analyzed locally to make predictions. A very subjective endeavor, it was considered by theory-based dynamicists to be more an art than a science. However, Rossby recognized that any theory used as a basis for a computational solution had to first include those factors which were either consciously or unconsciously used by the forecaster. After all, they were adding significant skill to turn raw data into a representation of the atmosphere from which they could make a prediction. Any additional variables could be added once the approximations and assumptions of the forecasters had been included.[19]

Therein lay a potential problem for the fledging group. If the team members were looking strictly at elegant numerical solutions to the hydrodynamical equations, then they could develop internally consistent models. Such models could produce forecasts for conditions at multiple atmospheric levels correctly correlating with each other, but not necessarily having any relation to reality. As Rossby noted, the equations needed to be viewed as tools to studying problems suggested by the atmosphere, not as an end in themselves.[20] Without solid synoptic support, Charney's fear of the group becoming mathematically sterile would become a reality.

Charney and Eliassen developed a very close collaborative effort producing a major article entitled "A numerical method for predicting the perturbations of middle latitude westerlies".[21] It was important because it demonstrated that an objective method could successfully replace more subjective methods. Further, the heuristic nature of their approach enabled them to try a model, see how it worked, and modify it as required.[22] Of course, how would they know whether it worked or not? The only way to do that was to compare the computer output to analyses produced by synopticians. When the output appeared meteorological and gave results similar to those anticipated by an outstanding analyst then it could be considered valid. Eliassen's value was not just his theoretical and mathematical knowledge, but the synoptic experience that allowed him to analyze the data with a significant degree of expertise.

Eliassen, however, had accepted a position at UCLA with Jacob Bjerknes and would not be in Princeton forever. Charney needed a replacement. He settled on another Norwegian, Ragnar Fjörtoft, because of his ability to mix into the general nature of the problems being explored by the Project and of course, because he had an excellent synoptic background through his work with the Norwegian Weather Service.[23] He arrived in September 1949.[24]

Fjörtoft slid easily into Eliassen's spot and was on hand for the first ENIAC (the Aberdeen Proving Grounds computer) "expedition" in March 1950. With Charney and von Neumann, he wrote an article describing the results of their barotropic model runs.[25] However, he needed to return to Scandinavia in June 1950. Charney once again needed a replacement – preferably someone like Eliassen or Fjörtoft.[26]

Rossby had a new prospect for Charney's group – the Swede Bert Bolin. Bolin was an excellent analyst and had had enough theoretical training that he would be able to carry out the needed synoptic studies. The barotropic model had failed in some regions and they needed to determine the non-computational error sources.[27] Charney thought baroclinicity might be the problem.[28] Rossby thought it might have been a "rather hidden appearance of cyclonic vorticity aloft". It would take careful analysis of the atmospheric situation to determine the error source. That would be Bolin's task.

As it turned out, Fjörtoft returned for another six months stay. During that period, with Bolin, Fjörtoft and Davies on board in addition to Joseph and Margaret Smagorinsky, Charney had sufficient personnel to attempt a baroclinic model.[29] Unfortunately, this abundance of personnel riches came to an abrupt halt in July1951 when for a period of time Smagorinsky was the only one left with the group. In the fall of 1951, only Charney, Norman Phillips and Joseph Smagorinsky were working full time. In the absence of experienced analysts, members of Rossby's International Meteorological Research Institute in Stockholm performed a large part of the preliminary work needed for evaluating the new models.[30] However, Charney needed people on site in Princeton and the staffing situation was rather desperate. Once again Rossby arranged a personnel transfer to the beleaguered Charney. By the middle of 1952, Ernst Hovmöller of the Swedish Meteorological and Hydrological Institute (SMHI) arrived to undertake the synoptic duties associated with the switch from the barotropic to the baroclinic models.[31] Still short of people, Charney continued to look abroad. One name on his list, Eady, was from England, but the other two were the Norwegians Eliassen and Fjörtoft. Unfortunately, they were all unavailable until 1953.[32]

These meteorologists would provide relief, but it was too distant. Von Neumann made a rather desperate plea to Hovmöller's boss, Anders Angström, to let him stay beyond the end of 1952. Von Neumann wrote that "His association with us is perhaps the first example of the kind of cooperating that will ultimately have to take place between theoretical and synoptic meteorologists if and when numerical forecasting is integrated into the governmental weather services".[33]

Angström deflected von Neumann's plea and instead offered Roy Berggren. Von Neumann was happy to take Berggren, but still wanted Hovmöller. Angström firmly closed that door, but undeterred von Neumann tried again. He needed an outstanding synoptic meteorologist and needed him right then. "To obtain such a person we are willing to bring him over from Europe…" Berggren arrived in March 1953 and stayed until the end of the year.[34]

In early 1953, the Princeton computer was working well and the Project members were kept very busy running models and analyzing output. Phillips in Princeton and Bolin in Stockholm changed places in the summer. Bolin stayed with the Princeton group for nine months where he was joined by Eady and Fjörtoft.[35] Fjörtoft remained until the end of June 1955. The last of the tag-team at the Meteorology Project was fittingly the first - Eliassen – and he stayed for the six months ending April 1956.

By the mid-Fifties, the Princeton group's emphasis had shifted to general circulation versus weather prediction due to the opening of the Joint Numerical Weather Prediction Unit. With von Neumann's departure for the Atomic Energy Commission and support from IAS fading, Charney left for MIT and the Meteorology Project ceased to exist.

Conclusion

So why the Scandinavians? Were there no Americans capable of performing the detailed analyses needed to validate the models and locate sources of error? Prior to World War II there were very few university trained meteorologists in the United States. Most operational forecasters received their training on the job. When the war required many more meteorologists, Rossby's University Meteorology Committee only accepted for training those who had extensive backgrounds in either physics or mathematics. Consequently, those who remained in meteorology after the war often had a different approach to the science than those already in the field. All of the Americans working in the Meteorology Project came into meteorology because of the war and they did not have the practical weather experience which develops a solid feel for the atmosphere.

Charney realized that it was absolutely imperative that at least some of the people working in numerical modeling have a more subjective feel for the atmosphere and the weather parameters necessary to making a valid prediction if they were going to overcome the skepticism that greeted them from the meteorological community. The "older" meteorologists possessed the necessary backgrounds, but many were either teaching the returning servicemen crowding their campuses or conducting research that had been postponed by the war. Some, perhaps, were just not convinced of the need for numerical methods. In any case, those active in the field in the United States were not clamoring to join the highly respected von Neumann.

Charney needed meteorologists with weather experience coupled with a solid theoretical background; people who were excellent analysts and interpreters of data and who could also communicate physical concepts in terms of complex equations. Charney already knew people who fit the requirements because of his stay in Norway. He also knew Rossby could provide the people he needed to keep the project moving.

In the United States, academic meteorologists were not people who were interested in actually predicting the weather. On the other hand the people making the predictions were not considered "meteorologists", but forecasters; a lower caste of non-professional, non-scientific workers. In Scandinavia these two groups were often one and the same. Therefore, their meteorologists were multi-faceted scientists ready to be tapped for the development which would change the face of meteorology in the middle of the Twentieth Century – numerical weather prediction – and thus provided a dose of atmospheric reality to a tremendous achievement of applied mathematics.

Endnotes

[1] Among them fluid dynamics, elasticity and plasticity theory, electrodynamics and quantum theory.

[2] William Aspray, *John von Neumann and the Origins of Modern Computing,* Cambridge, Mass., MIT Press, 1990. Chapter 6 addresses the early period in numerical weather prediction. Frederik Nebeker, *Calculating the Weather: Meteorology in the 20th Century,* San Diego, Academic Press, 1995 addresses the growth of calculations as a tool of weather prediction from the 19th century to the introduction and acceptance of the computers in the advance of numerical weather prediction. He mentions the development of meteorological instrumentation by RCA as a possible reason for Zworykin's interest in weather prediction.

[3] Memo to John von Neumann from Frank Aydelotte of November 9, 1945 (von Neumann Papers, Library of Congress, Box 12, Folder 1) (Hereafter: von Neumann Papers)

[4] Letter to C.-G. Rossby from John von Neumann of February 6, 1946 (von Neumann Papers, Box 15, F 7)

[5] Ibid.

[6] Letter to F. W. Reichelderfer from C.-G. Rossby of April 16, 1946 (von Neumann Papers, Box 15, F 7)

[7] Letter to John von Neumann from C.-G. Rossby of April 23, 1946 (von Neumann Papers, Box 15, F 7)

[8] Letter to C.-G. Rossby from John von Neumann of June 13, 1949 (Charney Papers, MIT Archive, MC 184, Box 14, F 459) (Hereafter: Charney Papers)

[9] Letter to CDR D. F. Rex from Frank Aydelotte of May 8, 1946 (von Neumann Papers, Box 15, F 6)

[10] Progress Report, Meteorology Project, July 1, 1946 to November 15, 1946 (von Neumann Papers, Box 15, F 6)

[11] Letter to C.-G. Rossby from Jule Charney of March 19, 1947 (Charney Papers, Box 14, F 460)

[12] Letter to P. D. Thompson from Jule Charney of November 12, 1947 (Philip D. Thompson Papers, National Center for Atmospheric Research, Boulder, Colorado, Folder Correspondence Jule Charney, 1947-1950) (Hereafter: Thompson Papers)

[13] Letter to Jule Charney from John von Neumann of November 19, 1947 (von Neumann Papers, Box 15, F 1)

[14] Letter to Jule Charney from Jacob Bjerknes of November 4, 1947 (Charney Papers, Box 4, F 120)

[15] Letter to Jacob Bjerknes from Jule Charney of January 14, 1948 (Charney Papers, Box 4, F 120)

[16] Letter to John von Neumann from Jule Charney of January 2, 1948 (von Neumann Papers, Box 15, F 1)

[17] Letter to John von Neumann from Jule Charney of January 2, 1948 (von Neumann papers, Box 15, F 1)

[18] Letter to Jule Charney from John von Neumann of February 6, 1948 (von Neumann papers, Box 15, F 1)

[19] Letter to J. Charney from C.-G. Rossby of October 28, 1948 (Charney Papers, Box 14, F 460)

[20] Letter to Jule Charney from C.-G. Rossby of January 9, 1949 (Charney Papers, Box 14, F 460)

[21] J.G. Charney and A. Eliassen, "A numerical method for predicting the perturbations of middle latitude westerlies", *Tellus,* 1 (1949), 2: 38-54.

[22] Letter to J. Charney from C.-G. Rossby of April 23, 1949 (Charney Papers, Box 14, F 459)

[23] Letter to C.-G. Rossby from Jule Charney of May 24, 1949 (Charney Papers, Box 14, F 459)

[24] Progress Report, Meteorology Project, July 1, 1949 to June 30, 1950 (Charney Papers, Box 14, F 459)

[25] J.G. Charney, R. Fjörtoft and J. von Neumann, "Numerical integration of the barotropic vorticity equation", *Tellus*, 2 (1950), 237-254. In this barotropic model, density is assumed constant on a pressure surface.

[26] Letter to C.-G. Rossby from Jule Charney of June 5, 1950 (Charney Papers, Box 14, F 459)

[27] Letter to Jule Charney from C.-G. Rossby of June 13, 1950 (Charney Papers, Box 14, F 459)

[28] Letter to George Platzman from J. Charney of June 22, 1950 (Charney Papers, Box 14, F 451)

[29] Letter to George Platzman from Jule Charney of June 22, 1950 (Charney Papers, Box 14 F 451). The baroclinic model assumes the advection of both the circulation and thermal fields.

[30] Progress Report, Meteorology Group, July 1, 1951 to September 30, 1951 (Charney Papers, Box 9, F 304)

[31] Letter to Jule Charney from C.-G. Rossby of April 10, 1952 (Charney Papers, Box 14, F 459)

[32] Letter to John von Neumann from Jule Charney of November 17, 1952 (Charney Papers, Box 14, F 517)

[33] Letter to Anders Angström from J. von Neumann of November 14, 1952 (Charney Papers, Box 16, F 517)

[34] Letter to Anders Angström from J. von Neumann of February 3, 1953 (Charney Papers, Box 16, F 517)

[35] Letter to Bert Bolin from Jule Charney of February 3, 1953 (Charney Papers, Box 4, F 121)