

A. The early history of NWP

Notes written from memory by Anders Persson (ECMWF) on 16 September 1999. The reader is encouraged to read P. D. Thompson's paper "Charney and the Revival of Numerical Weather Prediction", reproduced, together with Charney's letters to Thompson in Lindzen et al., (1990).

A.1. History of NWP

In late 1945 Vladimir Zworykin, the "Father of Television", who worked at RCA, joined with John von Neumann, the "Father of the Computer", to suggest the use of the computer in meteorology. Zworykin's interest was in weather modification, and von Neumann's was in fluid dynamics. They also had the dream of connecting the TV and the computer into something we today know as a PC or Workstation. Their dream came partially true in Sweden in around 1955 when for the first time a forecast map that was made directly and automatically without any human intervention was produced on a screen (oscilloscope) (see Bergthorsson and Doös, (1955), Bergthorsson et al., (1955), also the Rossby Memorial Volume).

In early 1946 von Neumann contacted Rossby's group. They told von Neumann why a zonally averaged dynamical model would not work, and instead suggested a barotropic model which had been manually tested by Victor Starr in his 1941 book on weather forecasting for a 72-h forecast at 700 hPa. Von Neumann was not satisfied with the simple barotropic approach and in speeches in the spring of 1946 presented more ambitious plans. Von Neumann and Zworykin also appeared at the annual meeting of the AMS (see Bulletin of AMS (1946)).

In the summer of 1946 the Princeton meeting took place. Few if any had any idea of what should be done. Not even the normally optimistic Rossby could see a solution to the problem. A working group was set up with Albert Cahn and Phil Thompson, with Hans Panofsky and Bernhard Haurwitz acting as advisors. By the autumn of 1946, there was still no clear idea of what to do. Cahn left meteorology to become a successful real estate agent in California, leaving Phil Thompson in despair. It was at this crucial state that Jule Charney moved to Chicago (on his way to Norway). Charney had attended the Princeton meeting, where he had offered some obscure ideas about having the whole atmosphere represented by a few singular levels.

In early 1947, Charney, now in Oslo, wrote to Phil Thompson that he indeed saw light at the end of the tunnel taking a completely new approach. It is important to realize that the practical (political/psychological) impact of L.F. Richardson's 1922 book was essentially to convince the meteorological community that NWP was impossible.

This was further supported by the experience Phil Thompson and others had while trying to make use of Jack Bjerknes “tendency equation” (which was as much in vogue then as potential vorticity is today!). For the 1948–50 events, I refer to the well-known literature.

A.2. Why Sweden?

The first real-time, operational NWP was run in Sweden in September 1954 (to 72 h at 500 hPa), half a year before the USA.

Two Reasons:

1. For a short period in 1954 the Swedes were in possession of the world’s most powerful computer, BESK. In 1950 they had already constructed a more basic one, BARK. One must again realize the thinking at that time: even among the most radical, it was felt that having just one computer in Sweden for the coming 20–30 years was sufficient. Even in the USA they thought that four or five computers would be more than enough for the foreseeable future. The “explosion” only came in 1955 when IBM launched their first machine.
2. Rossby moved to Sweden and wanted to repeat the ENIAC success of 1950 in his homeland. In this endeavor he was supported by: (a) the Swedish Air force and other national institutions (but not the Meteorological Service!); (b) young enthusiastic scientists who worked at or visited his institutions, both Swedish and foreign; (c) the US Air Force and Woods Hole.

(See articles by Wiin-Nielsen in *Tellus*, 1991, and Bolin in *Tellus*, 1999).

The Swedish project was hampered or complicated by an internal political conflict. In 1954 a new Director of SMHI (the Swedish Meteorological Office) was to be elected by the government. Rossby would have been the obvious choice, but he was seen as a troublemaker. The “official” candidate was Alf Nyberg, who had taken a very skeptical attitude towards Rossby’s project. Against him, Rossby lobbied Herrlin, head of the Military Meteorological Service. Unfortunately the run-up to the selection of new Director coincided with the launch of the first real-time operational NWP, 29 Sep–2 Oct 1954. Those who supported Nyberg took a negative attitude; those who supported Rossby took a positive one. In the end, the government chose Nyberg. SMHI began slowly to support NWP 5 day/week barotropic forecasts to 72 h at 500 hPa started in early December 1954. The US operational NWP started in May 1955, but it was not until 1958 that they reached the same quality standard as the Swedish. Japan started in 1959 along the same lines as Sweden.

A.3. More provocative ideas

Between 1950 (the first ENIAC run) and 1955 (the start of operational NWP) there was a long lapse of 5 years. Why? To what extent was the delay due to computer resources?

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To what degree to skepticism about NWP? Charney's presentation in his 1954 National Academy of Sciences paper is very political. My feeling from this paper and other sources is that he and the meteorological community were under strong pressure to present results, in particular with respect to the Thanksgiving Storm of 1950. The set up of the committee for NWP with George Cressman as Head seems to have been done in great haste. The Swedes were known to be progressing towards operational NWP. During the "Dark Years" 1956–57 some influential persons relying on Norbert Wiener suggested that computers should be used for statistical forecasting of weather patterns. Ed Lorenz at MIT was given the task of finding out if nonlinear dynamic evolutions could be reproduced or simulated by statistical means. His report, of which I have a copy, was guardedly optimistic! If Phillips and Cressman in 1957–58 had not managed to develop a functioning NWP system, things might really have developed along other lines. . . . It was during this or related work that Lorenz discovered the Butterfly Effect. As mentioned by Phillips in his 1990 monograph about energy dispersion (Phillips 1990a), if Charney et al. had run the ENIAC forecasts on a small area, the whole experiment would have had a severe setback, similar to Richardson's 1922 work. It is not commonly known that the UKMO lost 15years (1950–65) by trying to run a (good) baroclinic model on an area that was too small.