Clarifying the Dynamics of the General Circulation: Phillips's 1956 Experiment

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ABSTRACT

In the mid-1950s, amid heated debate over the physical mechanisms that controlled the known features of the atmosphere's general circulation, Norman Phillips simulated hemispheric motion on the high-speed computer at the Institute for Advanced Study. A simple energetically consistent model was integrated for a simulated time of approximately 1 month. Analysis of the model results clarified the respective roles of the synoptic-scale eddies (cyclones–anticyclones) and mean meridional circulation in the maintenance of the upper-level westerlies and the surface wind regimes. Furthermore, the modeled cyclones clearly linked surface frontogenesis with the upper-level Charney–Eady wave. In addition to discussing the model results in light of the controversy and ferment that surrounded general circulation theory in the 1940s–1950s, an effort is made to follow Phillips's scientific path to the experiment.

1. Introduction

One thousand years ago, the Viking colonizer Erik the Red knew of the stiff westerly winds that resided over the North Atlantic. These persistent winds hindered his passage from Iceland to Greenland in 990 A.D. Fourteen out of the 25 ships under his command failed to make the pilgrimage because of the gales and associated rough seas (Collinder 1954). Christopher Columbus was more fortunate, finding the northeast trades on his first voyage to the west. By the time Queen Elizabeth I founded the East India Trade Company in 1600, ocean traders knew full well that once their ships reached the mouth of the Mediterranean, sails could be continuously set and yards braced for a following wind (see Fig. 1).

When these surface observations over the Atlantic were coupled with Newton's system of dynamics (available by the 1680s), the stage was set for a rational study of the atmosphere's general circulation. Astronomer Edmund Halley (1656–1742), knowledgeable of Newtonian mechanics before the publication of *Principia* in 1687, attempted a systematic study of the low-latitude wind systems, namely, the trades and the monsoon (Halley 1686). In Louis More's biography of Issac Newton (1642–1727), written correspondence between Halley and Newton is presented (More 1934). Based on the information in these letters, it is clear that Halley was familiar with the material in Newton's monumental treatise, *The Mathematical Principles of Natural Philosophy* (Newton 1687), or simply *Principia,* prior to its publication. In fact, Halley was a driving force behind the publication of *Principia.*

Nearly 50 years passed before the first conceptual model of the atmosphere's circulation emerged, and the honor of discovery fell to a relatively unknown English scientist, George Hadley (1685–1768). In his essay of 1300 words, free of equations, Hadley used arguments based on the conservation of angular momentum to explain the trades:

> From which it follows, that the air, as it moves from the tropics towards the equator, having a less velocity than the parts of the earth it arrives at, will have a relative motion contrary to that of the diurnal motion of the earth in those parts,

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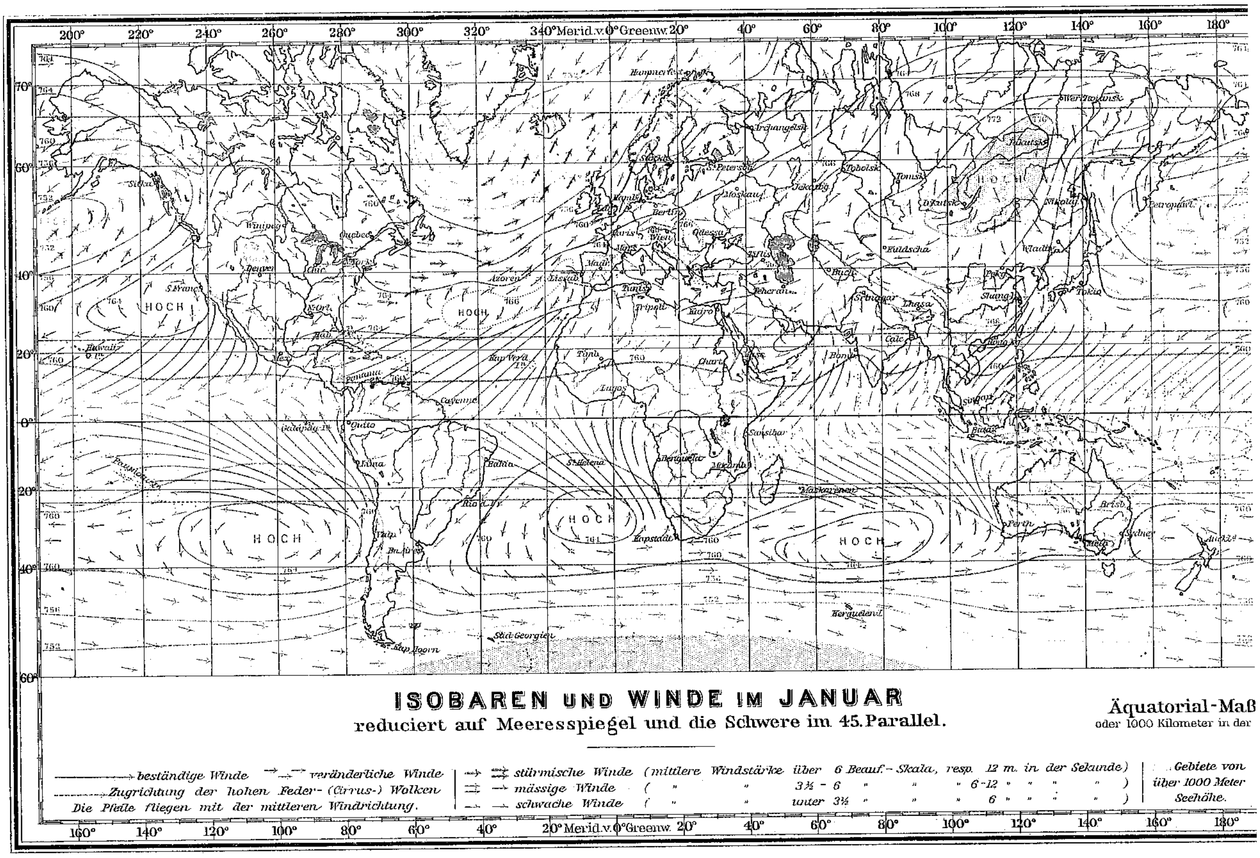


FIG. 1. Global wind and pressure patterns derived from late nineteenth-century charts constructed at Seewarte ("Sea Watch"). Isobars are labeled in mm (Hg) and the wind vectors combine both steadiness and strength. The steadier the wind, the longer the vector, and the stronger the wind (Beaufort scale), the thicker the shaft. Key words in the legend are *beständige* (steady), *veränderliche* (variable), *stürmische* (stormy), *mässige* (moderate), *schwäche* (weak), and *windstärke* (wind force/Beaufort scale). (Courtesy of Deutsche Seewarte, Hamburg, Germany.)

> which being combined with the motion towards the equator, a NE. wind will be produced on this side of the equator and a SE. on the other.
>
> (Hadley 1735, 59)

Lorenz (1967) has carefully traced the development of ideas associated with the atmosphere's general circulation from the time of Halley and Hadley to the mid-twentieth century. His historical research shows that advances appeared to fall into time blocks of approximately a half-century. Typically, an idea gained credibility and was published in the leading texts of the day, only to be challenged by the avant garde. New theoretical ideas emerged, often concurrent with observational facts, only to suffer the same fate as the precedent theory.

By the 1930s–1940s, conceptual models began relying on an ever increasing set of upper-air observations—pilot balloon observations from the early century later complemented by observations from radiosondes and instrumented aircraft. The picture was nevertheless incomplete, suffering from a lack of simultaneous measurements over latitudinal swaths commensurate with the pole-to-equator distance. The hint and hope for a global observational view, however, came with the heroic study by Scandinavian meteorologists Jacob Bjerknes and Erik Palmén (Bjerknes and Palmén 1937). Bjerknes coordinated the simultaneous release of radiosondes ("swarm ascents") from 11 European countries to study the evolution of a midlatitude depression (extratropical cyclone). Data from 120 radiosondes were used to analyze the storm. As recalled by Palmén, "It was most exciting to see that we were able to construct maps for different isobaric levels over practically the whole of Europe for a period of about two days" (Palmén 1980, 28). The cross sections in this paper spanned 3500 km and featured a pronounced sloping frontal zone as well as a bifurcation in the tropopause height that was linked to the front. The wind structure normal to the sections could be inferred from the isotherm pattern in conjunction with the thermal wind relation.

Coupled with these improvements in the atmospheric observation system, the vicissitudes of World War II spurred the development of high speed computation. In 1946–47, this computational power was brought to bear on two challenging problems in physics, both formulated by scientists at the Los Alamos Scientific Laboratory. The first was the numerical solution to a hydrodynamics–radiative transfer problem associated with the explosive release of energy from thermonuclear reaction; the second was the simulation of neutron diffusion in fissionable materials (Ulam 1964). Both experiments used the ENIAC (Electronic Numerical Integrator and Computer), a computer ostensibly designed for the computation of artillery firing tables but rewired for the physics experiments. John von Neumann was a central figure in these experiments, and in spring of 1946 he contemplated a numerical weather prediction (NWP) experiment. This project, labeled the Meteorology Project at Princeton's Institute for Advanced Study (IAS), officially started on 1 July 1946. Three years later, after a fitful start linked to staffing problems, a team led by Jule Charney made the celebrated short-range forecasts on the ENIAC (Charney et al. 1950). Nebeker (1995) has carefully examined events associated with the Meteorology Project, and eyewitness accounts are also available (Platzman 1979; Thompson 1983; Smagorinsky 1983).

Steady improvements to short-range NWP accrued during the early 1950s, in large part due to more realistic models that accounted for energy conversion in extratropical cyclones. Encouraged by the success of these forecasts, IAS team member Norman Phillips began to contemplate longer-range prediction using the IAS computer. His work took the form of a numerical simulation of the atmosphere's general circulation for a period of 1 month. The work was completed in 1955 and Phillips communicated the results to von Neumann, who immediately recognized their significance. Von Neumann hastily arranged a conference in October 1955, *Application of Numerical Integration Techniques to the Problem of the General Circulation*, held at Princeton University. In his opening statement at the conference, von Neumann said

> I should like to make a few general remarks concerning the problem of forecasting climate fluctuations and the various aspects of the general circulation that cause such fluctuations. Specifically, I wish to point out that the hydrodynamical and computational efforts which have been made in connection with the problem of short-range forecasting serve as a natural introduction to an effort in this direction . . . With this philosophy in mind, we held our first meeting nine years ago at the Institute for Advanced Study to discuss the problem of short-range weather prediction. Since that time, a great deal of progress has been made in the subject, and we feel that we are now prepared to enter into the problem of forecasting the longer-period fluctuations of the general circulation.
> (von Neumann 1955, 9–10)

Following this conference, which highlighted his numerical experiment, Phillips entered the research into competition for the first Napier Shaw Memorial Prize, a prize honoring England's venerated leader of meteorology, Sir Napier Shaw (1854–1945), on the occasion of the centenary of his birth (the competition was announced in April 1954). The subject for the first competition was "the energetics of the atmosphere." On 20 June 1956, "the adjudicators recommended that the prize be given to Norman A. Phillips of the Institute of Advanced Study, Princeton, U.S.A. for his essay 'The general circulation of the atmosphere: a numerical experiment,' which had been published in the *Quarterly Journal [of the Royal Meteorological Society]* (82, p. 1230) [April 1956] ..." (*Quarterly Journal of the Royal Meteorological Society* 1956b).[1]

This numerical experiment is retrospectively examined; furthermore, an effort is made to trace the steps that led Phillips to undertake the research. We begin by reviewing the state of knowledge concerning atmospheric general circulation in the 1940s–early 1950s, with some attention to the underlying controversies.

2. General circulation—Ideas and controversies: 1940s–early 1950s

To appreciate the momentous changes that took place in general circulation theory between 1940 and

[1]The adjudicators also commended the excellence of the entry "On the dynamics of the general circulation" by Robert Fleagle (1957).

1955, one has only to read Brunt's classic text (Brunt 1944, chap. 19) and follow this with a reading of Eady's contribution 13 years later, "The General Circulation of the Atmosphere and Oceans" (Eady 1957). From Brunt, the reader is left feeling that a consistent theory of the atmosphere's general circulation is out of reach: "It has been pointed out by many writers that it is impossible to derive a theory of the general circulation based on the known value of the solar constant, the constitution of the atmosphere, and the distribution of land and sea . . . It is only possible to begin by assuming the known temperature distribution, then deriving the corresponding pressure distribution, and finally the corresponding wind circulation" (Brunt 1944, 405).

Eady's discussion, on the other hand, promotes a sense of confidence that the general circulation problem, albeit complicated, was yielding to new theoretical developments in concert with upper-air observations. His final paragraph begins, "If from this incomplete survey, the reader has gained the impression that general circulation problems are complicated, this is as it should be. The point is that mere complication does not prevent their being solved. Much of the complication shows itself when we attempt to give precise answers instead of vague ones . . . To answer problems in any branch of geophysics we need vast quantities of observations but we also need precise, consistent, mathematical theory to make proper use of them" (Eady 1957, 151).

Certainly the 10-year period prior to Phillips's numerical experiment was one of ferment as far as general circulation was concerned. A brief review of the major issues and themes during this period follow.

a. Rossby: Lateral diffusion

Rossby's interest in the general circulation problem can be traced to his review paper on atmospheric turbulence (Rossby 1927). In this paper, the work of Austrian meteorologists Wilhelm Schmidt and Albert Defant was highlighted. Defant (1921) had suggested that traveling midlatitude cyclones and anticyclones could be viewed as turbulent elements in a quasi-horizontal process of heat exchange between air masses, and he quantified the process by calculating an *austausch* or exchange coefficient following Schmidt (1917). Rossby was attracted by this concept (especially in the context of momentum transfer), and he applied it to the Gulf Stream and tropospheric westerlies (Rossby 1936, 1937, 1938a,b).

Rossby summarized his ideas in a wide-ranging review article in *Climate and Man* (*Yearbook of Agriculture*), a compendium of meteorology that was shaped by a diverse committee headed by the chief of the U.S. Weather Bureau, Francis Reichelderfer (Rossby 1941). Rossby relied on the three-cell model of circulation that emanated from the work of nineteenth-century scientists William Ferrel and James Coffin (Ferrel 1859; Coffin 1875). This conceptual model, as it appeared in Rossby's article, is shown in Fig. 2. Here we see two direct cells: the equatorial cell (called the "Hadley cell") and the polar cell. The indirect cell in the midlatitudes is called the "Ferrel cell."

Regarding the westerlies, Rossby argued

> In the two direct circulation cells to the north and to the south, strong westerly winds are continuously being created at high levels. Along their boundaries with the middle cell, these strong westerly winds generate eddies with approximately vertical axes. Through the action of these eddies the momentum of the westerlies in the upper branches of the two direct cells is diffused toward middle latitudes, and

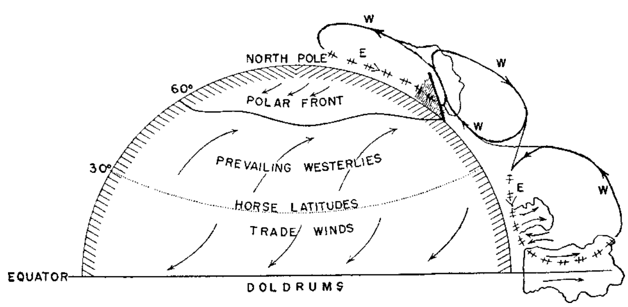


FIG. 2. Three-cell conceptual model of global circulation [extracted from Rossby (1941, Fig. 4)]. Deep cumulus cloud is indicated in the equatorial zone, clear sky is associated with descending air in the subtropics (~30°N), and precipitation occurs in association with ascent of air over the polar front zone. Westerly/easterly winds are indicated along the meridional circulation circuits by the solid lines/"hatched symbols." In the panel to the right, Rossby is shown sitting at his desk in the U.S. Weather Bureau building in Washington, D.C. (ca. 1940). (Rossby photo courtesy of K. Howard and the Library of Congress.)

> the upper air in these regions is dragged along eastward. The westerlies observed in middle latitudes are thus frictionally driven by the surrounding direct cells . . . the air which sinks in the horse latitudes spreads both polewards and equatorwards. The poleward branch must obviously appear as a west wind. . . .
>
> (Rossby 1941, 611)

Rossby modified his ideas by the late 1940s, vorticity becoming the transferable property rather than momentum (Rossby 1947).

b. Jeffreys–Starr–Bjerknes–Priestley–Fultz: Asymmetric eddies

Tucked away near the end of a paper that explored atmospheric circulation by analogy with tidal theory, Harold Jeffreys argued that asymmetric eddies (cyclones/anticyclones) "not unlike that described by Bjerknes . . ." were an essential component of the atmosphere's general circulation (Jeffreys 1926). Quantitative arguments based on the conservation of angular momentum led him to state that a steady meridional (axially symmetric) circulation could not be maintained. Balance could only be achieved when the frictional torque was balanced by angular momentum transport due to asymmetric eddies. The governing equation for this transport is the integral (around a latitude circle) of the product of horizontal wind components. Quoting Jeffreys, "Considering any interchange of air across a parallel of latitude, then $u\,v$ [the product of horizontal wind components] must be negative both for the air moving north and for that moving south. This corresponds to the observed preponderance of south-westerly and north-easterly winds over those in the other two quadrants." (Jeffreys chose a coordinate system where u was directed southward and v eastward. Thus, the sign of $u\,v$ in Jeffreys's coordinate system is opposite to that found in the more conventional system where u points eastward and v northward.)

Jeffreys came to this conclusion after grappling with the frictional formulation in his theory. The paper conceals this battle, but his reminiscence exposes it:

> the point was that you could solve the [atmospheric] problem when you had adopted the hydrodynamical equations to a compressible fluid . . . you could solve that for a disturbance of temperature of the right sort, and you could solve it in just the same way as you did for the tides—and it just wouldn't work! At least it worked all right when you didn't put in any friction. When you put friction in, it turned out that the friction in the result would stop the circulation in about a fortnight, and I had to start again, and I found that the only way to do it was to have a strong correlation between the easterly and northerly components of wind.
>
> (Jeffreys 1986, 4)

Jeffreys's theory laid dormant for 20 years. It was rejuvenated in the late 1940s by Victor Starr (1948), Bjerknes (1948), and Charles Priestley (1949). In the second paragraph of Starr's paper, he says "In reality, this essay may be construed as a further extension of the approach to the problem initiated by Jeffreys." Starr, who had exhibited his prowess with mathematical physics applied to the geophysical system (e.g., see Starr 1939, 1945), displayed another aspect of his skill as a researcher in this essay—namely, a clarity of expression and an expansive research vision. In essence, the essay became the blueprint for Starr's research plan at the Massachusetts Institute of Technology (MIT) during the next decade.[2] The upper-air observations collected in the postwar period made it clear that there was a decidedly NE–SW tilt to the horizontal streamlines, "so common on meteorological maps, [it] is a necessary automatic adjustment to provide for the poleward transfer of atmospheric angular momentum" (Starr 1948, 41). Dave Fultz's hydrodynamical laboratory experiments confirmed the tilted streamline patterns and became an independent source of support for Jeffreys's theory [photographs from Fultz's experiment are shown in Starr (1956)].

The initial investigations by Starr and Bjerknes led to independent, long-term efforts [at MIT and the University of California, Los Angeles (UCLA), respectively] to collect and archive upper-air data on a global scale. These assidious efforts led to sets of general circulation "statistics," measures of the temporally and/or spatially averaged terms in the heat and angular momentum budget equations [see the contributions by Starr and White (1951) and Mintz (1951, 1975)]. Priestley's work is notable, however, because his calculations relied on observed winds rather than geostrophic approximations to the wind. Priestley

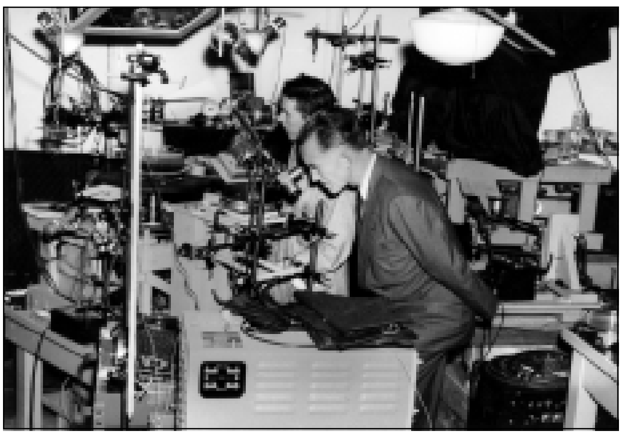
[2]Starr was the second recipient of the Ph.D. in meteorology from the University of Chicago (summer 1946). (The first recipient was Morris Neiberger, autumn 1945). Starr accepted a faculty position at MIT in 1947.

continued his work on these problems until the early 1950s "before yielding to the greater resources of the two American pairs, Bjerknes–Mintz and Starr–[Robert] White . . ." (Priestley 1988, 104).

Photographs of the scientists who were instrumental in studying the asymmetric aspects of the general circulation are shown in Fig. 3.

c. Palmén and Riehl: Jet streams

The existence of the strong and narrow band of upper-level westerlies, labeled the jet stream, was established by forecasters in Germany (late 1930s) and the United States (early 1940s) (see Seilkopf 1939; Flohn 1992; Riehl et al. 1954; Plumley 1994).



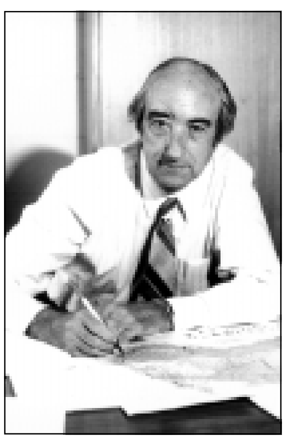


FIG. 3. Top: J. Bjerknes (in the foreground) and D. Fultz at the University of Chicago's Hydrodynamics Laboratory (1953). Middle-left: H. Jeffreys sits in his office at Cambridge (ca. 1928). Middle-right: C. H. B. Priestley (ca. 1980). Bottom: V. Starr (ca. 1965). (Courtesy of Lady Jeffreys, Dave Fultz, Constance Priestley, and the MIT archives.)

Following World War II, Rossby obtained funding from the Office of Naval Research (ONR) for a comprehensive study of atmospheric general circulation (including the dynamics of the jet stream). He invited Erik Palmén to assume a leadership role in this research. Palmén had spent his early career at Finland's Institute for Marine Research and was named director of the institute in October 1939, just two months before Russia invaded Finland. Throughout the remainder of World War II, Palmén's scientific work was severely curtailed. "He [Palmén] was born again in the setting of the general circulation project at the U of C [University of Chicago]" (C. Newton 1990, personal communication). He remained at Chicago for 2 years (1946–48), returning to Finland in late 1948 as chair professor of meteorology at the University of Helsinki. His frequent long-term visits to Chicago during the next decade, however, made him a fixture at the University of Chicago Institute of Meteorology. A photo of Palmén in the company of other meteorologists at the 1947 Aerology Commission meeting is shown in Fig. 4.

In June 1947, the expansive report on the ONR project appeared under the authorship of staff members of the department of meteorology (Staff Members 1947). Salient features of the jet stream were enumerated in the "summary" section of the paper. Notable were the following: 1) the jet is located in or just south of a zone in which a large fraction of the middle and upper troposphere temperature contrast between polar and equatorial regions is concentrated; and 2) below the jet stream, it is possible to identify a well-defined frontal zone, intersecting the ground south of the jet stream.

Palmén became convinced that the concept of a single circumpolar jet was questionable, and he proposed the existence of a second jet, which he called the subtropical jet. "He [Palmén] thought that the great mass of air convected to the upper troposphere in the tropics could not all then descend in the subtropics. As evidence kept mounting, one began to speak of the 'subtropical jet stream' found mainly above 500 mb and not undergoing the many violent north–south oscillations of the northern, soon called 'polar jet stream'" (Riehl 1988).

Following Palmén's return to Finland in 1948, Herbert Riehl became the scientific leader of the jet stream project. Through the continued sponsorship of ONR, research flights across the circumpolar jet stream were initiated in 1953 (H. Riehl 1994, personal communication).

d. Controversies

Amid such rapid advancement in meteorology, along with the slate of competing ideas, there is little wonder that this period had its share of controversies. A considerable amount of heated debate occurred at the daily map briefings at University of Chicago in the late 1940s. George Cressman offered daily discussions and forecasts with all the available maps (from mid-Pacific Ocean to the Ural Mountains in Russia—240° of longitude in the Northern Hemisphere). There was no end to the arguments about general and cyclone circulations that followed Cressman's briefings. The "reverse cell" of midlatitudes created fuel for the verbal exchanges. The abrupt transition from equatorward westerlies at high level in this middle cell to the neighboring easterlies in the equatorward or Hadley cell was conceptually difficult to understand [see Palmén and Newton (1969, chap. 1) for a summary of research that established the existence of the upper-level easterlies]. In Riehl's words, "[why should] the equatorward westerlies, virtually friction-free in high atmosphere, . . . quickly diminish and go over into easterlies, just where the maximum west wind is observed" (Riehl 1988).

FIG. 4. Members of the International Meteorological Organization's Aerology Commission at their meeting in Toronto, Ontario, Canada, in 1947. Identification of members follow, where the position in the rows (front, middle, and back) is counted from left to right: W. Bleeker (front, 1); E. Palmén (front, 3); Sverre Petterssen (front, 5); C. Penner (middle, 2); A. Nyberg (middle, 3); J. Bellamy (middle, 4); H. Wexler (back, 3); W. Hewson (back, 4); and Z. Sekera (back, 5). A photo of Herbert Riehl is shown in the inset (ca. 1970). (Courtesy of the Library of Congress; inset courtesy of the World Meteorological Organization.)

One of the most celebrated scientific exchanges occurred in the "correspondence" section of the *Journal of Meteorology.* Starr and Rossby (1949) wrote a short article reconciling their differences on the role of angular momentum conservation in the atmosphere's general circulation. Their "differences" were minor, essentially related to the interpretation of terms in the equation of angular momentum conservation. One of the statements in the article, however, created an uproar. This cardinal statement reads, "*Most of the classic theories for the general circulation were based upon the assumption that it is this effect of meridional circulations which maintains the angular momentum of the zonal motions in the atmosphere.* It is this assumption that *both of us* call into question for reasons enumerated by Rossby [1941]." They go on to say that, in their opinion, it is the advective transport of relative angular momentum—the $u\ v$ term in Jeffreys's formulation—that is of prime importance in the mechanics of the general circulation.

Four months after the appearance of the Starr–Rossby article, Palmén wrote a letter to the editor that adamantly questioned the conclusion stated above (Palmén 1949). He argued that the mean meridional circulation term could not be discounted; furthermore, Palmén made order of magnitude estimates of the meridonal transport and found them comparable to the eddy transport term. The verbiage was strong and it elicited an ordered yet acerbic response from Starr (1949). Quoting Starr, "Apparently Palmén suspects me of highest heresy lest I suggest that the energy production process may also be accomplished without the aid of meridional circulations. This I have indeed proposed . . . the hypothesis that meridional cells are of small importance seems to be bearing fruit. Indeed if such are the fruits of heresy, then I say let us have more heresy."

Although more stimulating than controversial, the general circulation statistics generated by the research teams at UCLA and MIT were demanding explanation. For example, the work of Bjerknes (and Mintz) at UCLA showed that the poleward eddy heat flux had its maximum at 50° latitude and was strongest near the ground. On the other hand, the poleward eddy angular momentum flux had its maximum near 30° and was strongest near the tropopause (Bjerknes 1955).

Thus, by the mid-1950s, major questions related to the atmosphere's general circulation begged for answers. Among the issues were respective roles of the mean meridional circulation and transient eddies in the momentum and energy budgets, mechanism for the maintainance of the westerlies (jet streams), and the dynamical basis for alternating wind regimes at the surface.

3. The experiment

Norman Phillips had been exposed to much of the controversy on general circulation theory while a graduate student at the University of Chicago in the late 1940s–early 1950s. During this same period, Phillips's interest in dynamic meteorology was awakened through a careful reading of Charney's paper on the scale of atmospheric motions (Charney 1948). He became convinced that simple baroclinic models (in particular, models that stratified the troposphere into two or three layers) could faithfully depict the principal features of cyclogenesis. His early work with these models, both theoretically and numerically, proved to be fundamentally important for the subsequent work on numerical simulation of the atmospheric general circulation. Because of the importance of these various professional experiences, an appendix has been attached to this paper that carefully follows his path to the experiment.

Although Phillips's doctoral and postdoctoral research concentrated on the short-range prediction problem, he had an abiding interest in the general circulation problem that came in part from his exposure to the debates at Chicago, but also from his own practical experience as a research assistant on the ONR general circulation research project (see appendix). These two research themes or components, the theoretical investigation of baroclinic motions and the phenomenological view of global circulation, came together for Phillips in early 1954. He was employed by the Institute for Advanced Study at this time but was on leave at the International Institute of Meteorology in Sweden. As he recalls,

> From graduate school days at Chicago we had a pretty good idea of what the leading theoreticians and synopticians thought about how the general circulation worked. So it was not too difficult for me to first do this study in the '54 paper [Phillips 1954] to see what baroclinic unstable waves might do—force an indirect circulation and then . . . the lateral north and south boundary conditions would require direct circulation further out towards the pole and equator. And that this indirect circulation, in middle latitudes was the process, turbulent process that Rossby always referred to vaguely as giving rise to the surface westerlies. The explanation of surface westerlies had been the main challenge in the general circulation for centuries. They all knew that a direct circulation with the equator flow would not produce westerlies. So they had to put in little extra wheels, to end up creating polar flow in midlatitudes. This seemed to all fit together so it encouraged me to go back to Princeton [in April 1954] and convince Jule [Charney] with that paper that yeah, that should be a logical thing to spend my time on. He was my boss.
>
> (Phillips 1989, 25)

Another key factor or ingredient in Phillips's strategy for designing the general circulation experiment was the success of the laboratory simulations of hemispheric flow by Fultz and English geophysicist Raymond Hide. Phillips writes:

> In spite of the unavoidable dissimilarities between the laboratory experiments and the atmosphere, certain experimental flow patterns are remarkably like those to be seen on weather maps. Thus, one is almost forced to the conclusion that at least the gross features of the general circulation of the atmosphere can be predicted without having to specify the heating and cooling in great detail.
>
> (Phillips 1955, 18)

[See Hide (1969) for a comprehensive review of research on laboratory simulations of the atmosphere's general circulation.]

a. The model and computational constraints

Phillips adopted a set of dynamical constraints not unlike those used in short-range forecasting of the large-scale tropospheric flow—a two-level quasi-geostrophic model with horizontal winds specified at the 750- and 250-mb levels, and mean temperature defined at 500 mb. Net radiation and latent heat processes were empirically parameterized by a heating function, a linear asymmetric function of the north–south distance (denoted by coordinate y, $-W \le y \le +W$), vanishing at $y = 0$.

The salient features of the model follow: quasi-geostrophic and hydrostatic constraints on the beta-plane,[3] where lateral diffusion of vorticity is included at both levels and frictional dissipation is parameterized at the lower level. Following Phillips, subscripts are used as follows: 1 = 250 mb, 2 = 500 mb, 3 = 750 mb, and 4 = 1000 mb. The vorticity (ζ) at 1000 mb is found by linear (in pressure) extrapolation of vorticity from the 750- and 250-mb levels, that is, $\zeta_4 = (3\,\zeta_3 - \zeta_1)/2$. Streamfunction, geopotential, and wind components are found by using an equivalent extrapolation formula.

To model a "hemispheric" region, the north–south dimension (y direction) of the domain was set to 10^4 km (~equator-to-pole distance on the earth's surface). The east–west dimension (x direction) was chosen to accommodate one large baroclinic disturbance [~(5 − 6)10^3 km]. Phillips cleverly allowed for the life cycle of the eddies by postulating periodic boundary conditions in the x direction; thus the disturbances typically moved out of the domain on the eastern boundary and entered along the western boundary.

The discretized arrays of variables shared computer memory with the stored program, and this was the limiting factor on the dimensionality of the problem. The IAS computer had 1024 words of internal memory and 2048 words of slower magnetic drum memory. This dictated arrays of (17 × 16) in y and x directions, respectively. The associated grid intervals were $\Delta x = 375$ km and $\Delta y = 625$ km. Since the mean temperature (level 2, 500 mb) is proportional to the difference in geopotential between levels 1 and 3, the dependent variables for the problem are the geopotential arrays (streamfunctions); thus, the instantaneous state of the modeled atmosphere is determined by roughly 500 numbers. The horizontal domain (with the grid spacing shown on the inset) is displayed in Fig. 5.

As might be expected in those early days of computer modeling, execution time for the model run was long and the associated coding was tedious and laborious. Using nominal time steps of 1 h, the 31-day forecast required 11–12 h on the IAS machine. As recalled by Phillips (N. Phillips 1997, personal communication):

> Code was written in what would now be called "machine language" except that it was one step lower—the 40 bits of an instruction word (two instructions) were written by us in a 16-character (hexadecimal) alphabet 0, 1, . . ., 9, A, B, C, D, E, F instead of writing a series of 0's and 1's, for example, "C" represented the four bits "1100".
>
> There was no automatic indexing—what we now call a "DO-LOOP" was programmed explicitly with actual counting. Subroutines were used, but calls to them had to be programmed using explicitly stored return addresses. In the first year or so of the IAS machine, code and data were fed in by paper tape. Von Neumann

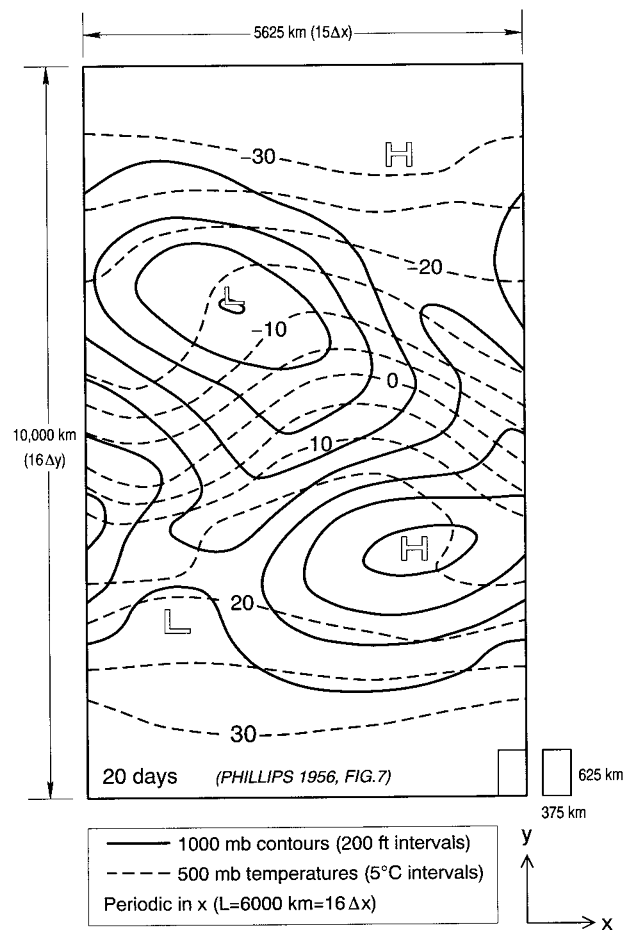


FIG. 5. On day 20 of the simulation, the synoptic-scale disturbance exhibits the characteristics of a developing cyclone with attendant frontogenesis. The mesh size is shown beside the model's horizontal domain.

[3]The beta-plane was introduced by Rossby et al. (1939) to simplify the equations of motion on the sphere. In this formulation, the kinematic effects of the earth's curvature are ignored but the dynamical effects are retained through the inclusion of the variation of the Coriolis parameter. Phillips assumed the beta-plane's point of tangency was 45°N.

> eventually got IBM to allow one of their card readers to be modified so that punched cards could be used for input and output.

b. The basic state

In accord with studies of baroclinic instability via analytical dynamics, Phillips established a basic-state solution upon which perturbations could be superimposed. To derive this basic state, he started with an isothermal atmosphere at rest and used the model constraints to incrementally march forward in units of 1 day. The net heating gradually built up a latitudinal temperature gradient and associated zonal wind structure. The empirical heating/cooling rate of 0.23°C day^{-1} (at $y = \pm W$) led to a latitudinal temperature gradient of 60.2°C/10^4 km after 130 days. At this stage of the integration, the meridional circulation consisted of a single weak direct cell (as Hadley had envisioned)—superimposed on a zonal circulation that was independent of x. The latitudinal temperature gradient gave rise to a vertical wind shear of ~2 m s^{-1} km^{-1}, sufficient for the growth of small amplitude perturbations in the zonal flow.

Charney (1959), and more recently A. Wiin-Nielsen (1997, personal communication), have investigated steady-state solutions to Phillips's model. It is clear from their investigations that Phillips's basic state was not the steady-state solution. Quoting Wiin-Nielsen: "From the values of the zonal velocities in [Phillips's basic state] it is obvious that the model at this stage did not make a good approximation to the steady state derived here. His [Phillips's] purpose was only to obtain a zonal state where the vertical wind shear (or equivalently, the horizontal temperature gradient) was sufficiently large to be a state which was unstable for small perturbations. It is, however, of interest to see what the spin-up time is for the model to approximate the derived steady zonal state. . . . It is seen that the asympotic level is almost reached after t = 4.32 10^8 seconds, which is equivalent to 5000 days (13.7 yr)" (A. Wiin-Nielsen 1997, personal communication).

c. The disturbed state

A random number generating process was used to introduce perturbations into the geopotential field, where the perturbations were identical at levels 1 and 3. Incremental steps of 1 h were used to march forward in time and the following events took place.

1) A disturbance developed with wavelength of ~6000 km [similar to the disturbance shown in Fig. 5], and the flow pattern tilted westward with height; the wave moved eastward at 21 m s^{-1} (1800 km day^{-1}).
2) Transport of zonal momentum into the center of the region by horizontal eddies created a jet of 80 m s^{-1} at 250 mb, and at the same time a pattern of easterly–westerly–easterly zonal winds were established at the 1000-mb level.

d. Zonal-mean winds

The time evolutions of the zonal-mean fields are displayed in Fig. 6. [zonal-mean implies an average over the x coordinate and is denoted by the overbar, $\overline{(\,.\,.)}$]. Time (in days) is shown along the abscissa, where t = 0 (days = 0) is the time when disturbances were introduced into the simulation and the total period of simulation is 31 days.

The zonal-mean wind components at 250 mb are shown in the top panels of Fig. 6: $\overline{V}_1$ (meridional component in cm s^{-1}) and $\overline{u}_1$ (zonal component in m s^{-1}). The extrapolated zonal wind at 1000 mb, $\overline{u}_4$, is shown in the lower-left panel. The $\overline{V}_1$ field shows two sign reversals as one moves along the y axis (j index) on days 10–25. The magnitude of this meridional component is greatest in the middle zone, reaching values of ~60–80 cm s^{-1}.

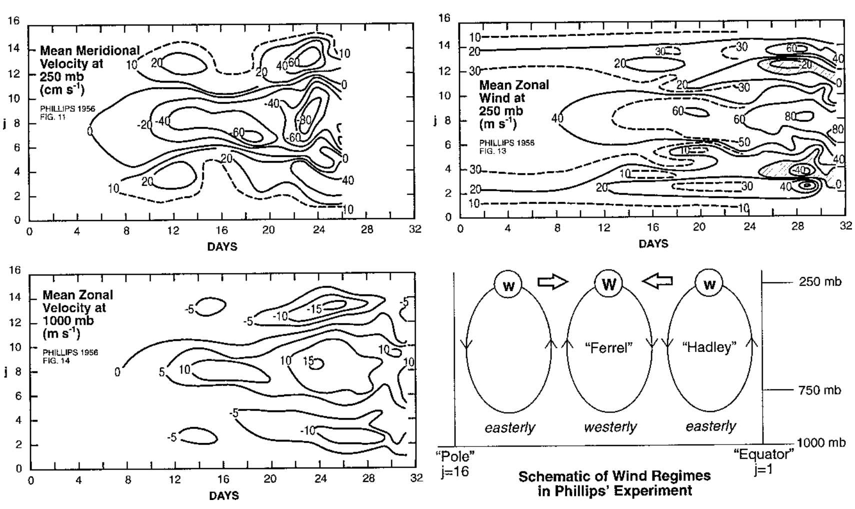


FIG. 6. Latitudinal distribution of the mean meridional and zonal winds over the 31-day period of simulation.

The $\overline{u}_1$ pattern shows westerly winds at all latitudes for approximately the first 25 days of simulation. The strongest winds are in the middle zone where speeds are 40–60 m s^{-1} (days 10–25). At the 1000-mb level, the zonal winds ($\overline{u}_4$) exhibit an alternating pattern of easterly, westerly, and easterly winds.

The summary of the mean-zonal flow has been depicted in the lower-right panel of Fig. 6, where the extremities of the *y* coordinate have been labeled "equator" and "pole" (to indicate that the north and south limits of the beta-plane have been chosen commensurate with the pole-to-equator distance). Here "W" and "w" indicate the strongest and weaker westerly flow at the upper level, respectively; these westerlies overlie the alternating pattern of easterlies and westerlies at the 1000-mb level. The arrows at the upper level, directed toward the strongest midlatitude westerlies (the jet stream), are indicative of the flux of eddy momentum into the jet (to be discussed in the next section).

Since the zonal-mean meridional flow at 750 mb is equal and opposite to that at 250 mb, a three-cell pattern can be inferred. Because of the similarity between this three-cell structure and that postulated from earlier studies, the labels "Ferrel" and "Hadley" have been added. Phillips, however, did not use these terms in the discussion of his results, only "we see the appearance of a definite three-cell circulation, with an indirect cell in middle latitudes and two somewhat weaker cells to the north and south. This is a characteristic feature of the unstable baroclinic waves in the two-level model, as has been shown previously by the writer (Phillips 1954). After 26 days, the field of $\overline{V}$ became very irregular owing to large truncation errors, and is therefore not shown" (Phillips 1956, 144–45).

e. Momentum budget

To clarify the processes that give rise to the jet, Phillips tabulated the momentum budget based on statistics over the 11-day period, days 10–20 inclusive. Information found in Phillips (1956, Tables 4 and 5) has been graphically represented in Fig. 7. At the upper level, the tendency ($\partial \overline{u}_1/\partial t$) in midlatitudes is mainly determined by the meridional circulation ($\propto \overline{V_1}$) and the eddy transport $[(-\partial/\partial y)(\overline{u_1'v_1'})]$, the latter being the larger. The contribution from the meridional circulation is in general opposite to the observed changes in $\overline{u_1}$, so as to reduce the effect of the eddy term at 250 mb. As stated by Phillips, "The resulting picture is thus very much like that postulated by Rossby as existing during the building up of zonal wind maximum (Staff Members 1947)" (Phillips 1956, 152).

The profiles at level 3 indicate that the midlatitude westerlies form in response to the meridional circulation, the ($f_0\overline{V_3}$) term. Thus, the meridional circulation tends to balance both the large values of $[(-\partial/\partial y)(\overline{u_1'v_1'})]$ in the upper atmosphere and the effect of surface friction on the lower atmosphere. As retrospectively examined by Phillips,

> Thus Palmén and Starr had missing features in their respective views, Starr could not explain the low-level westerlies without the indirect meridional circulation, and Palmén could not explain the upper-level westerlies without the eddies.
>
> (N. Phillips 1997, personal communication)

f. Thermodynamic budget

As a complement to the zonally averaged momentum budget, Phillips tabulated the terms in the ther-

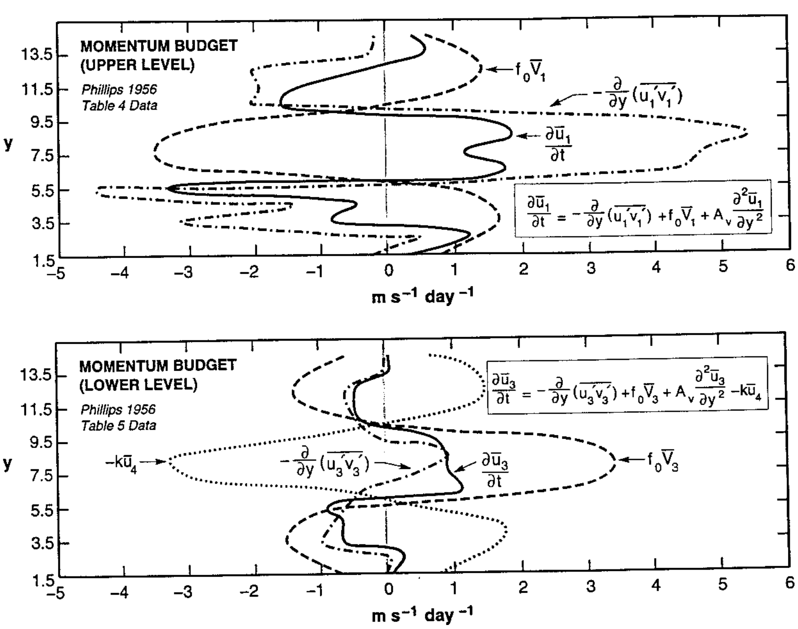


FIG. 7. Latitudinal distribution of the various terms in the momentum budget equations at the upper and lower levels. The equations were averaged over the 11-day period, days 10–20 inclusive. Parameterized coefficients of lateral diffusion and friction are denoted by A_v and *k*, respectively. The diffusion terms at both levels were neglibly small and have not been plotted.

modynamic energy equation. These results are displayed in Fig. 8. Here, the net radiation term heats the atmosphere in low latitudes and cools it at high latitudes. The convergence of eddy heat transport, $[(-\partial/\partial y)(\overline{v_1'T_2'})]$, opposes the net radiation, tending to destroy the latitudinal temperature gradient, especially in midlatitudes. The meridional circulation term, $(\propto \overline{\omega}_2)$, on the other hand, tends to increase the latitudinal temperature gradient $(\overline{u_1'T_2'})$ due to the reverse circulation of the Ferrel cell.

g. Energetics

Since the heating function is a linear and asymmetric function about $y = 0$ (45° latitude), the total amount of energy added/subtracted from the system is zero. However, there is a positive correlation between the heating and mean meridional temperature; that is, the heating is positive/negative in the region of higher/lower mean temperature. This generates *available potential energy.* In Phillips's model, this energy is expressed as the spatial integral of the squared deviation of the 500-mb temperature (a deviation from the standard atmosphere). It is derivable from the governing equations of the two-level quasigeostrophic model, first appearing in Phillips (1954). Lorenz's (1955) systematic treatment of available potential energy is acknowledged by Phillips, "in a beautiful attempt to reconcile the synoptic meteorologist's intuitive association of *available* potential energy with temperature gradients, [Lorenz] has recently shown how a similar expression can be approximated from the usual definition of the potential plus internal energy . . ." (Phillips 1956, 135). It is clear from information in Phillips's oral history interview that he was unaware of Lorenz's contribution until the general circulation experiment was completed (Phillips 1989, 27).

The energy, both kinetic (K) and available potential (P), are partitioned into zonal-mean components ($\overline{K}$ and $\overline{P}$) and perturbations about this mean, referred to as eddy components (K' and P'). At each step of the model integration, the various energy components are calculated along with the energy transformations. Phillips then found the *temporal* average of these quantities over a 22-day period of simulation (days 5–26).

The results are presented in Fig. 9 [patterned after the diagram found in Oort (1964)]. The generation of mean meridional available potential energy is represented by the symbol $\overline{G}$, and it is shown in the upper-left corner of the schematic diagram. This generation term is theoretically balanced by the dissipation of energy [D], which takes the form of lateral diffusion and surface friction in Phillips's model. As indicated by Phillips's results and subsequent studies, the energy cycle generally proceeds from $\overline{P}$ to P' and K' and finally to $\overline{K}$, a counterclockwise movement around the diagram (Wiin-Nielsen and Chen 1993, chap. 7). The transformation rates are indicated along lines connecting the various energy reservoirs, where a positive value indicates transfer in the direction of the arrow.

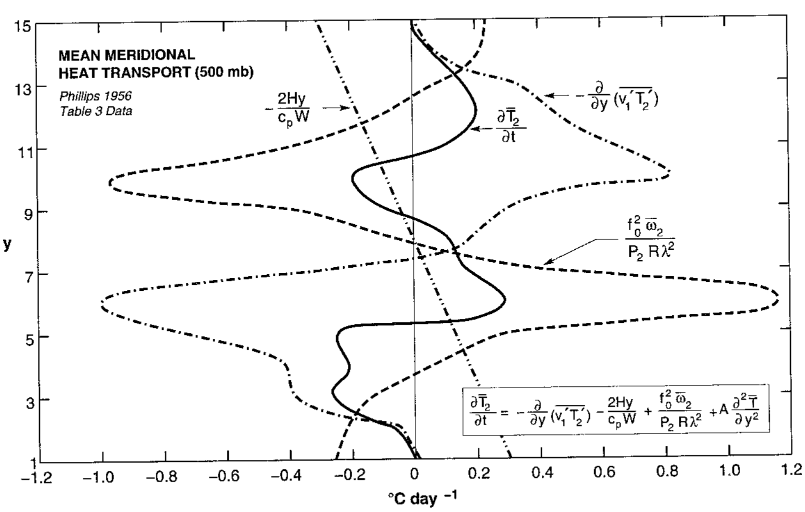


FIG. 8. Latitudinal distribution of the various terms in the thermodynamic equation, averaged over the 11-day period, days 1–20 inclusive. The lateral diffusion coefficient is denoted by A, P_2 is 500 mb, R is the gas constant, and λ^2 is a positive parameter related to the static stability (assumed constant). The diffusion term is of negligible magnitude and has not been plotted.

Phillips, of course, had little basis for validation of his calculated energy exchanges (the top set of values at the various junctions in the energy diagram). He nevertheless appeared to be pleased that the generation and dissipation almost balanced and that the generation term was "about half of the estimated rate of energy dissipation in the real atmosphere (Brunt 1944) . . . the model is undoubtedly too simple to expect any closer agreement" (Phillips 1956, 154). He is circumspect when he writes, "It is of course not possible to state definitively that this diagram is a complete representation of the principal energy changes occurring in the atmosphere, since our equations are so simplified, but the verisimilitude of the forecast flow patterns suggests quite strongly that it contains a fair element of truth. Further computations with more exact equations will presumably refine the picture considerably, as will an extension of observational studies using real data" (Phillips 1956, 154).

When the first comprehensive set of general circulation statistics became available in the next decade (Oort 1964), Phillips's cautious optimism was rewarded. Oort had judiciously combined results from various observational studies [with limited information from Phillips (1956) and Smagorinsky (1963)] to make mean annual estimates of the terms in the energy budget of the Northern Hemisphere. Oort's mean annual statistics are displayed in the rectangular boxes of Fig. 9. Phillips did not account for the generation of eddy available potential energy (a very difficult component of the system to determine because it depends on the heating in the atmospheric waves). On the other hand, Oort's study made no attempt to calculate the dissipation associated with the available potential energy (a modeled term that tends to smooth out the temperature gradients). The sense of Phillips's energy transformations, as well as their magnitudes, are quite consistent with Oort's. The absolute values of the energy components in the reservoirs, however, are significantly different. The variability of these statistics on seasonal, let alone monthly, timescales could account for part of the difference, but the simplified model dynamics also shared responsibility for this discrepancy. It would be nearly 10 years before more complete models of the general circulation would begin to faithfully represent this aspect of the energetics (Smagorinsky et al. 1965).

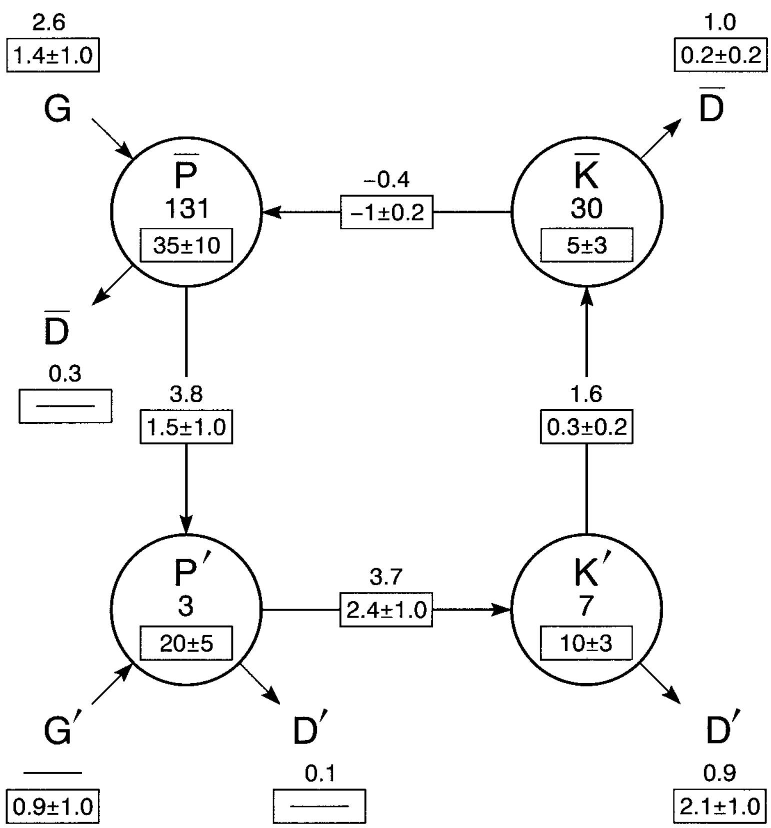


FIG. 9. Energy diagram showing the reservoirs of kinetic (K) and available potential energy (P), where zonal-mean and eddy components are denoted by $\overline{(\ldots)}$ and $(\ldots)'$, respectively. The transformation rates between the various components are indicated along the lines connecting the reservoirs; if positive, the energy is transferred in the direction indicated. Energy generation/dissipation is denoted by *G/D,* respectively. Oort's observationally based statistics are shown in the rectangular boxes, and Phillips's simulated statistics are written above these boxes. The energy units are 1) reservoirs—J m^{-2} 10^5, and 2) transformation rates –W m^{-2}.

4. Reaction to the experiment

Fortunately, some of the discussion that followed Phillips's oral presentation of his work has been preserved. Excerpts from these discussions are presented, and they are followed by vignettes that feature retrospective viewpoints from several prominent scientists who worked on the general circulation problem in the 1950s.

a. Sir Napier Shaw lecture

As the recipient of the first Napier Shaw prize in 1956, Phillips was invited to deliver a seminar on his paper to the Royal Meteorological Society. The state of affairs in the English meteorological establishment at this time was not far removed from that in the United States. Both countries were in the throes of initiating operational numerical weather prediction, and both had strong traditions in synoptic meteorology. Reginald Sutcliffe, director of research at the British Meteorological Office (BMO), had a strong team of researchers at the BMO, including John Sawyer, and England possessed a bonafide world-class theoretician in Eric Eady of Imperial College. These scientists, along with other members of England's meteorological elite, were in attendence at

Phillips's presentation. A series of questions (and replies) that followed the talk are recorded in the *Quarterly Journal of the Royal Meteorological Society* (1956b). Broad issues and questions that arose are paraphrased as follows, where the author(s) of the questions are identified in parentheses:

1) Unrealistic initial condition, that is, starting the simulation from a state of rest (P. Sheppard and R. Sutcliffe);
2) excessive strength of the indirect cell (P. Sheppard);
3) absence of condensation processes that could possibly explain the "slow rate of baroclinic development" (B. Mason and R. Sutcliffe);
4) questionable physical significance of the transformation of energy between K' and $\overline{K}$ (G. Robinson); and
5) question regarding the secondary jets to the north and south of the main jet. Can these jets be considered similar to the subtropical jet and can we deduce that these are established by different mechanisms than the main jet stream (J. Sawyer)?

Phillips's responses to these questions (and others) are detailed in the *Quarterly Journal of the Royal Meteorological Society*. He seemed to be particularly stimulated by the question posed by Sheppard on the indirect circulation and Sawyer's question related to the subtropical jet. He sided with Sheppard and agreed that the indirect circulation of the middle cell was probably overestimated (citing evidence from observational studies at UCLA); furthermore, he was reluctant to claim that the secondary jets in the simulation were manifestations of the subtropical jet (as postulated and studied by Palmén).

The most encouraging remark came from Eady:

> I think Dr. Phillips has presented a really brilliant paper which deserves detailed study from many different aspects. I am in complete agreement with the point of view he has taken and can find no fault with his arguments, either in the paper or in the presentation. With regard to the statement by Prof. Sheppard and Dr. Sutcliffe, I think Dr. Phillips' experiment was well designed. Numerical integrations of the kind Dr. Phillips has carried out give us a unique opportunity to study large-scale meteorology as an experimental science. By using a simple model and initial conditions which never occur in the real atmosphere he has been able to isolate, and study separately, certain fundamental properties of atmospheric motion—the kind of procedure adopted by all good experimenters . . . An experiment which merely attempted to ape the real atmosphere would have been very poorly designed and very much less informative.

b. Princeton conference

The issue that received the most attention at the *Symposium on the Dynamics of Climate* at Princeton University in October 1955 was truncation error in the numerical experiment (see "Discussions" in Pfeffer 1960). During the last 10 days of the 31-day period of simulation, there was a steady deterioration of the flow pattern. There appeared to be little doubt in Phillips's mind and in the opinion of the others at the symposium that the culprit was truncation error, that is, numerical error that accrued from finite-difference approximations to the governing equations. Quoting Phillips (1956, 157), "It was thought initially that the introduction of a lateral eddy-viscosity into the equations would eliminate some of the bad effects of truncation errors, by smoothing out the small-scale motions. To some extent this was true . . . but evidently a still more fundamental modification of the equations is required." Phillips (1959) would later identify nonlinear computational instability as a contributor to this noise problem.

c. Vignettes

Norman Phillips visited Stockholm in early 1956 and presented his research results at the International Meteorological Institute. Rossby, director of the institute, was especially interested in Phillips's experiment because it addressed issues related to cyclogenesis (and associated frontogenesis). The Bergen School model of cyclone development had placed emphasis on instabilities that formed on existing fronts (e.g., see Solberg 1928; Kochin 1932; Eliassen 1962), whereas the work of Charney (1947) and Eady (1949) discussed cyclogenesis in terms of the upper-level tropospheric wave. (Figure 5 shows an upper-level Charney–Eady wave and the associated surface pressure pattern.) Following the seminar, Rossby held forth and had an elongated discussion with Phillips on the numerical simulation of the cyclogenesis process (A. Wiin-Nielsen 1993, personal communication). Wiin-Nielsen reconstructs this discussion where Rossby's and Phillips's statements are denoted by R and P, respectively.

R: Norman, do you really think there are fronts there?
P: Yes, look at the temperature fields packed up very nicely.
R: But Norman, what's the process that creates these fronts? Where do they come from?
P: Well, they come out of a very simple dynamics.
R: And what is that?
P: I have very simple linear heating between the equator and pole, simple dissipation, but of course there is no water vapor or no precipitation, no clouds, totally dry model.
R: Yes, Norman, and it should be that! Because here we are getting this front—and it has nothing to do with clouds/rising motion, it is a sheer dynamic effect that comes as a result of the development.

Charney discussed this same issue in a paper commemorating the work of Jacob Bjerknes. Quoting Charney,

> His [Phillips's] experiment also helped to resolve an apparent inconsistency that I had found in my own and Bjerknes' work on the cyclone. I had been struck by the fact that while there was a one-to-one correspondence between long upper air waves and the primary surface cyclones—which always seemed to form fronts—there was no such correspondence between the upper wave and the secondary and tertiary frontal waves in a cyclone family . . . In Phillips' experiment . . . the dominantly unstable baroclinic wave mode appeared and, in its nascent stage, very much resembled the theoretical prediction from small perturbation analysis; but when the wave developed to finite amplitude, it exhibited the typical concentration of isotherms of a frontal wave. Thus the deformation field in the developing baroclinic wave produce frontogenesis in the form of the frontal wave, so that the primary cyclone wave does not form on a preexisting front, rather it forms at the same time as the front and appears as the surface manifestation of the upper wave . . . once the front has formed, it may permit frontal instabilities of the type analyzed by Solberg (1928) and Kochin (1932) and later more completely by Eliassen (1962) and Orlanski (1968). It would seem that the latter type is the "cyclone wave" of Bjerknes and Solberg (1922), whereas the former is the "cyclone wave" of Bjerknes, [and] Holmboe (1944), Eady (1949), and Charney (1947).
>
> (Charney 1975)

Phillips's experiment had a profound effect outside the United States, especially in the strongholds of dynamic meteorology such as the International Meteorological Institute at Stockholm and in Tokyo, Japan, at the university's Geophysical Institute and at the Japan Meteorological Agency (JMA). Akio Arakawa, a scientist at JMA in the mid-1950s, recalls his reaction to Phillips's work:

> I myself was also extremely inspired by Phillips' work. My interest around the mid-50s was in general circulation of the atmosphere, mainly those aspects as revealed by observational studies on the statistics of eddy transports by Starr and White at MIT and Bjerknes and Mintz at UCLA, and laboratory experiments by Fultz [at University of Chicago] and Hide at MIT. At the same time, I was also interested in numerical weather prediction, through which dynamical meteorologists began to be directly involved in actual forecasts. Phillips' work highlighted the fact, which people began to recognize around that time, that the dynamics of cyclones and that of general circulation are closely related. I was so excited about these findings that I published a monograph through Japan Meteorological Society (Arakawa 1958) . . . to let Japanese meteorologists recognize the important ongoing progress in our understanding of general circulation of the atmosphere.
>
> (A. Arakawa 1997, personal communication)[4]

5. Epilogue

George Hadley presented the first consistent theory of the general circulation of the atmosphere. A little over 200 years later, with the advent of high speed computation, Norman Phillips would blend theory and observations in the design of a numerical experiment, an experiment that he hoped would clarify the inter-

[4]In collaboration with Donald Johnson, Arakawa has coauthored a tribute to Yale Mintz (Johnson and Arakawa 1996). The influence of Phillips's work on Mintz has been carefully documented in their paper.

action between synoptic-scale eddies and the general circulation.

The experiment design was bold. The simplicity of the model dynamics exhibited an almost irreverent disregard for the complexities of the real atmosphere—the governing equations were quasigeostrophic, there were no mountains, no land/sea contrast, and water vapor was only indirectly accounted in the empirically derived heating function. The strength of the contribution rested on 1) the reasonable simulation of the energy transformation rates, and 2) the explanation of interacting physical processes (the mean meridional circulation and the eddy transport) that gave rise to the midlatitude westerlies and the alternating surface wind regimes. The experiment also demonstated the linkage between surface frontogenesis and evolution of the planetary wave—in essence, it showed that fronts grow on the broad north–south temperature gradient field according to the Charney–Eady theory. This result inspired a cadre of young theoreticians to analytically and numerically examine the dynamics of frontogenesis in the next decade [see the review by Orlanski et al. (1985)].

From a politico–scientific viewpoint, Phillips's work led to the establishment of an institutional approach to deterministic extended-range forecasting. Von Neumann was the champion of this effort. As recalled by Joseph Smagorinsky,

> Phillips had completed, in the mid-1950s, his monumental general circulation experiment. As he pointed out in his paper, it was a natural extension of the work of Charney on numerical prediction, but Phillips' modesty could not obscure his own important contributions to NWP. The enabling innovation by Phillips was to construct an energetically complete and self-sufficient two-level quasi-geostrophic model which could sustain a stable integration for the order of a month of simulated time . . . A new era had been opened . . . von Neumann quickly recognized the great significance of Phillips' paper and immediately moved along two simultaneous lines . . . One was to call a conference on "The Application of Numerical Integration Techniques to the Problem of General Circulation" in Princeton during October 26–28, 1955 . . . [and] the other initiative by von Neumann was stimulated by his realization that the exploitation of Phillips' breakthrough would require a new, large, separate, and dedicated undertaking . . . [he] drafted a proposal to the Weather Bureau, Air Force, and Navy justifying a joint project on the dynamics of the general circulation . . . The proposal, dated August 1, 1955, was more or less accepted the following month as a joint Weather Bureau–Air Force–Navy venture. I was asked to lead the new General Circulation Research Section, and reported for duty on October 23, 1955.
>
> (Smagorinsky 1983, 25–29)

This research unit, initially a companion project alongside the short-range numerical forecasting unit in Washington, D.C., soon attained a separate identity with the help of U.S. Weather Bureau Chief Robert White, and became known as the Geophysical Fluid Dynamics Laboratory in 1965. And within the 10-year period, 1955–65, major institutional efforts in global numerical simulation were started at the National Center for Atmospheric Research, Lawrence Livermore Laboratory, and UCLA (in the United States), and at the Meteorological Office, United Kingdom (abroad).

The experiment helped resolve the Starr–Palmén controversy, but it accomplished this goal in concert with a series of diagnostic studies of the general circulation that accrued from the late 1950s through the 1960s [see Palmén and Newton (1969, chaps. 1–2)]. Palmén, Riehl, and disciples eventually came to accept Starr's thesis regarding the primacy of the eddies in transporting momentum poleward, while Starr, Rossby, and company accepted the fact that mean meridional circulations are essential ingredients in the global balance requirements.

In his oral history interview, Phillips makes it clear that he greatly benefited from Rossby, Palmén, Platzman, and Charney; these scientists stimulated and challenged him at pivotal junctures on his path to the experiment. As he said, "I began to learn more about how fortunate I was to have Platzman as a thesis advisor . . . George, as you know, has a characteristic of being accurate as well as being right. And I think I've, I hope I've learned some of that from him . . ." (Phillips 1989, 13). The experiment certainly contained that element of verity that we all search for in our research, and by example, Phillips inspired his contemporaries and a host of us in the succeeding generations.

Acknowledgments. I am grateful for a series of letters from Norman Phillips over the past several years. In this correspondence, he clearly presented his scientific experiences at Chicago, Princeton, and Stockholm. I gained perspective on the experiment

by meshing these personal reminiscences with his scientific contributions and the wealth of information contained in Phillips's oral history interview. Tony Hollingsworth and colleagues (Akira Kasahara, Joseph Tribbia, and Warren Washington) are congratulated for their superlative work in the collection of this oral history. Hollingsworth's knowledge of Phillips's oeuvre was encyclopedic.

Bulletin-appointed reviewers offered valuable suggestions for revision that were followed, and the result was a significantly improved manuscript. Throughout, Aksel Wiin-Nielsen shared his knowledge of general circulation theory with me. Additionally, his unpublished notes, *Lectures in Dynamic Meteorology* (University of Michigan, ca. 1965) served as a pedagogical guide as I worked my way through the literature on atmospheric general circulation.

Eyewitness accounts concerning early developments in numerical simulation of atmospheric motion have been provided by the following scientists, where "O"/"L" denote oral history/letter-of-reminiscence, respectively, and where the date of the communication is noted within parentheses:

Akio Arakawa L (14 April 1997)
Fred Bushby L (29 October 1997)
Larry Gates L (15 April 1997)
Brian Hoskins O (25 October 1995)
Akira Kasahara L (20 August 1993)
Syukuro Manabe L (14 April 1997)
Joseph Smagorinsky L (28 May 1997)
Phil Thompson O (18 May 1997)
Warren Washington L (17 April 1997)
Aksel Wiin-Nielsen O (22 April 1993)
Terry Williams L (3 September 1993)

Photo acquisition credit goes to the following people: Dave Fultz, Kenneth Howard, Bertha Jeffreys, Constance Priestley, Joseph Smagorinsky, Athelstan Spilhaus, Monika Stutzback-Michelsen, Judith Torres, and Monique Yabi; and the following institutions: Cambridge University, Library of Congress, Massachusetts Institute of Technology, Seewarte (Bundesamt für Seeschiffahrt und Hydrographie, Hamburg, Germany), and the World Meteorological Organization. Finally, I want to thank Joan O'Bannon, graphics specialist at the National Severe Storms Laboratory, for her faithful reproduction of data from Phillips's 1956 paper.

Appendix: Path to the experiment

Phillips was only 30 years old when he began his research on the general circulation experiment in April 1954. Nevertheless, he had a wide range of experiences in meteorology by this time, from practical forecasting experience during the war to graduate research in synoptic meteorology and numerical weather prediction at the University of Chicago, followed by postdoctoral research at the Institute of Advanced Study and the International Meteorological Institute in Stockholm. These experiences represented important stages on his path to the experiment, and while briefly referenced in the main body of the text, they are elaboratoted upon in this appendix.

a. Weather school and forecasting in the Atlantic region (1942–46)

When the United States entered World War II (8 December 1941), Norman Phillips was an undergraduate majoring in chemistry at the University of Chicago. Induction into the armed forces was likely, and rather than leave his duty assignment to chance, he enlisted in the U.S. Army with the understanding that he would be assigned to the meteorology program. He started premeteorology training (the "B" school) at the University of Michigan on 15 March 1942, along with 356 other recruits (Walters 1952, 103). Upon completion of this 6-month program, he entered the cadet program (the "A" school) at Chanute Field, Illinois. Nine months later, on 5 June 1944 (the day before D-Day), Phillips graduated and was commissioned second lieutenant, U.S. Army Air Force. He was assigned to headquarters, 8th Weather Region of the Air Force, Grenier Field, just south of Manchester, New Hampshire [see Fig. A1 for the location of stations in the 8th Weather Region]. As recalled by Phillips:

> I was fortunate that the head of that weather squadron was Arthur Merewether who had been head of meteorology for American Airlines . . . I was sent to a station in the Azores where we had to consider the weather over the entire Atlantic Region in order to service the flights that came to and left the Azores.[5]
>
> (Phillips 1989, 4)

It is clear upon reading Phillips's oral history that he was fascinated with interpretation of the synoptic weather, especially trying to fit the observed data to the Bergen School models. "[You were not] quite that much impressed by the usual fronts as they passed you on the ground, . . . [but] when you're traveling through a frontal zone at, I suppose 150 miles an hour [in a reconnaissance aircraft], things are compressed a lot more and you could see what was a discontinuity—showers, wind shift, you could see the winds by the foam on the waves" (Phillips 1989, 5). There were tense moments: "I remember once one of the planes for which I gave a wind forecast got blown over the

[5]See Fuller (1991) and Brown (1994) for further information on Merewether and weather-related activities in the Atlantic region during World War II.

Normandy peninsula at the time the Germans were still in charge of St. Lo and they were shot at—a passenger plane! Another time I sent a flight to Newfoundland, and Gander zeroed in and I wasn't sure myself whether the plane had enough fuel to go on to Stevensville [Stephenville] on the other side of the island" (Phillips 1989, 6). (See Fig. A1 where Stephenville is the 8th Weather Region station on the west side of Newfoundland.)

Phillips's forecasting experiences in the Azores inclined him toward a career in meteorology, and when he was discharged in August 1946, he returned to the University of Chicago as an undergraduate majoring in meteorology instead of chemistry.

b. Graduate education, University of Chicago (1947–51)

Phillips received his bachelor's degree in spring 1947 and immediately entered the university's graduate program in meteorology. He had the luxury of working under Erik Palmén for his master's degree, where the thesis was a synoptic study describing the subsidence of a cold dome. He also had the benefit of immersing himself in the milieu created by Rossby, a steady stream of world-class meteorologists visiting for both short and longer periods of time, the scientific debates at the daily map briefings mentioned earlier, and, quite important for Phillips, the chance to pursue some of his interests in numerical methods. As Phillips recalls: "Then [after completion of the master's degree] I worked for a year and one-half to two years with [Hsiao-Lan] Kuo on a navy research project [ONR] that Rossby had. Mostly under my own—I was doing my own things, as was Kuo at that time. I was . . . learning about [numerical] relaxation and Southwell's technique'' (Phillips 1989, 9; Southwell 1946). R. V. Southwell was an English mathematician who pioneered the use of numerical methods ("relaxation methods") in the solution of elliptic partial differential equations.

FIG. A1. Stations in the 8th Weather Region during World War II. Colonel Arthur Merewether, shown above the location chart, commanded this weather region from 1942 to 1945. (Courtesy of Athelstan Spilhaus.)

Phillips was tasked with preparing a set of cross sections for the final ONR report, and remembers Rossby's reaction to his work:

> [Rossby] had to produce something for the navy for paying us our salary and had me publish, pretty up and write a paper on some of the cross sections that I had been drawing on the 80th meridian. I think I drew a whole winters' worth of cross sections. Many of them in a rather sketchy fashion and I remember Rossby's shudder at the appearance of some of the cross sections. So I quickly learned to draw them nicer. The jets were more complicated than

> the simple picture that had previously existed. Palmén, together with his student, [Chester] Newton, (and so forth),usually waited for the most clear-cut case to draw because they were after all proselytizing a new concept that there was this jet, in Palmén's view,connected and indistinguishable from the main polar front.
>
> (Phillips 1989, 9)

In 1950, Phillips began to close in on his dissertation topic, the development of a two-layer (three-dimensional) model suitable for studying extratropical cyclones. He chose "the simplest model which contains some of the essential features of the baroclinic atmosphere . . ."—two superimposed layers of incompressible fluid (different densities), constrained to move between two rigid horizontal plates and governed by the conservation of potential vorticity (Phillips 1951, 3). In the spirit of the Chicago school of meteorology, Phillips was solely responsible for the choice of this dissertation topic. He was stimulated by Charney's paper on the scale of atmospheric motions (Charney 1948), "Charney presented this recipe for how to predict large-scale motions," and Rossby's classroom lectures paved the way for model design: "I had seen Rossby use such kind of models in his lectures" (Phillips 1989, 10). The research had two primary goals (as described in the dissertation abstract): 1) "The possibility of using the two-layer model to represent motions of a continuously stratified baroclinic troposphere . . .," and 2) "forecast of the atmospheric flow patterns . . . [via] a forecast of the two-layer flow patterns." Phillips rested his hopes for success on the fact that the "aerological evidence [Fleagle (1947) and Palmén and Newton (1951)] shows that the variation with height of the large-scale flow pattern of interest to us is rather simple. Thus, approximately, the vertical velocity component associated with these motions is zero at the ground, reaches a maximum value between the ground and tropopause, and becomes small again just above the tropopause" (Phillips 1951, 7).

Phillips's dissertation succeeded on two counts: he was able to theoretically derive the criteria for baroclinic instability in the two-layer model [and it compared favorably with the result of Eady's (1949) for the continuous stratification],[6] and he used the two-layer model to forecast the instantaneous tendencies for the sea level pressure and vertical velocity for a case of extratropical cyclogenesis. The tedious calculations were carried out by hand—iterative solutions to elliptic equations via Southwell's relaxation techniques. This was the first numerical weather forecast using a baroclinic model of the atmosphere.

c. Postgraduate experience (1951–54)

Charney visited the University of Chicago in late 1950 or early 1951, and it was during this visit that Phillips discussed his dissertation topic in the company of Rossby, Charney, and Platzman [N. Phillips 1997, personal communication; see also Lindzen et al. (1990, 54)].

Charney appears to have been impressed with Phillips' work:

> Phillips just outlined very briefly on the blackboard what he had done. And what he had done, of course, was to develop a two-layer model. It was somewhat inspired by the geostrophic approximation [Charney's 1948 paper mentioned above], but I think it would be historically of considerable interest to know what led him to do that. . . .
>
> In any case, I recognized that right away as the next step. I hadn't proposed to do it in that way, but it turned out that the finite difference formulation in the simplest way, by vertical differencing of the quasi-geostrophic equations, one arrives at the same equations as one gets for a two-layer model.
>
> (Lindzen et al. 1990, 54)

Following completion of his dissertation, Phillips accepted a position with the Meteorology Project at IAS in August 1951. Figure A2 shows Phillips in the company of Platzman, Charney, and other staff members at the Institute.

In late 1951 the design and construction of the IAS computer had progressed far enough that Phillips was assigned to write the program for a barotropic model. This was then extended to a two-level program following the formulation by Charney [reported in Charney and Phillips (1953)]. Both models were tested on the Thanksgiving Day storm of 1950 that Phillips had used in his dissertation. Nebeker (1995, 148–151) has presented verification statistics for this case study from information in the institute archives and states that "the baroclinic forecasts [two-level model] were widely

[6]Phillips derived his instability formula before reading Eady's paper, and he expressed delight on seeing it confirmed (Phillips 1989, 10–11). He gives credit to his advisor George Platzman, along with Charney and Rossby, for helping him overcome mathematical difficulties inherent in the derivation.

FIG. A2. Some of the members of the Meteorology Project at the Institute for Advanced Study in 1952. From left to right: J. Charney, N. Phillips, G. Lewis, N. Gilbarg, and G. Platzman. The IAS computer, MANIAC I, is in the background. This picture was taken by Joseph Smagorinsky, another member of the Meteorology Project. (Courtesy of J. Smagorinsky.)

seen as evidence that numerical forecasting could outperform subjective forecasting." The two-level model did not capture the cyclogenesis, however, and Charney and Phillips started development of a three-level model. As recalled by Charney, "And we went to a three-level model, that is a two-and-two-thirds-dimensional model, and we did catch the cyclogenesis. It wasn't terribly accurate, but there was no question that [we did]. . . . And I always thought this was a terribly important thing" (Lindzen et al. 1990, 54).

Phillips would later revisit the November 1950 storm using a revised two-level model at IAS [reported in the special issue of *Geophysica*, commemorating Palmén's 60th birthday; see Phillips (1958)]. As he recalls, "I showed that Charney's three-level computations did not have the magic he thought they did, but that the essence of the cyclogenesis was already present in the two-level model, hidden by error due to the artificially high geostrophic wind in the initial upper-level trough" (N. Phillips 1997, personal communication).

In 1953, Rossby sent Phillips an invitation to visit the International Meteorological Institute at Stockholm University. The invitation carried the responsibility of serving as a consultant on issues related to operational weather prediction. Phillips accepted the invitation, and as he recalls:

> I came to Stockholm in August 1953 to assist with the first barotropic forecasts to be made on the Swedish computer BESK (modeled after von Neumann's Princeton system). I think that the construction of the BESK was initiated by someone in Stockholm other than Rossby, but it certainly must have been he who arranged for the cooperation with his Institute to use it for weather prediction.
>
> (N. Phillips 1991, personal communication)

Aksel Wiin-Nielsen, one of the participants in the forecasting experiments at Stockholm, has written an informative account of the events that took place in 1953–54 (Wiin-Nielsen 1991). It is interesting to note that operational forecasts were made as early as March 1954. Quoting Wiin-Nielsen (1991, 46), "It is believed that these two cases [23–24 March 1954] represent the first forecasts finished in time to be of use in operation." Wiin-Nielsen lists the participants in this first operational forecast: G. Arnason, Iceland; B. Bolin, Sweden; G. Dahlquist, Sweden; B. Döös, Sweden; N. Phillips, United States.

In the midst of work on the barotropic model in Stockholm Phillips started to think about the general circulation experiment. Following his six-month stay in Stockholm (and two months at the Geophysical Institute in Oslo, Norway), he returned to IAS in April 1954 and work on the experiment began.

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