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The general circulation of the atmosphere: an evolving problem

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ABSTRACT

We trace the development of the prevailing ideas about the general circulation of the atmosphere from the middle 17th century up to 1970. During this time, a quantity U, representing the extent to which we have not yet explained those features of the circulation of which we are aware, appears to have gone through four distinct cycles. Since 1970, no additional cycles are apparent, and the outstanding feature of the new work seems to be diversification. We propose that the study of the general circulation is a chaotic process, so that, at any time, the coming developments cannot be predicted with any assurance, even though many possibilities can be eliminated.

1. Introduction

It was some 40 years ago that I acquired the status of a post-doc, and began to work under the guidance of Victor Starr on a project devoted to the general circulation of the atmosphere. Early in our work, we encountered a new volume of articles about various aspects of the atmosphere, written by various authors, and naturally I was curious to see who had written the general-circulation article. It turned out to be someone named Bert Bolin.

From that time on I took careful note of Bert's work, but it was not until we both attended a now historic conference in Princeton in 1955 that I had the pleasure of meeting him. By that time he was also well established in the fields of numerical weather prediction and atmospheric chemistry. So, 35 years later, it seems entirely fitting not simply that we should be holding a symposium in his honor, but also that the general circulation should occupy the opening session.

There is a famous old American tale entitled *Rip Van Winkle*, written by Washington Irving in one of his lighter moments. It tells about a likable but lazy fellow who lived in a small village by the Hudson River, at the foot of the Catskill Mountains. It relates how he went walking into the mountains with his dog one afternoon, met a strange company of men, drank deeply from one of their flagons, and slept for twenty years, and how, when he walked back to his village on what he thought was the next morning, he was baffled by the changes that he encountered. We are not told the exact years of his sleep, but they spanned the American Revolution, and might have been from 1770 to 1790.

When I recently took a look at what was going on in the general-circulation community, I felt a certain kinship with Rip Van Winkle. I don't mean that I was actually sleeping from 1970 to 1990, but most of may work during that period was addressed to other subjects, and my few papers related to the general circulation dealt with rather specialized topics, somewhat removed from the mainstream of research. Thus, when I did attend a session on the general circulation a while ago, and found that many of the experts in that area were using linear or quasi-linear methods to study a circulation that I had always considered strongly nonlinear, I began to feel almost as perplexed as Rip Van Winkle, when he found that the portrait of His Majesty George the Third in front of his favorite tavern had been transformed into one of George Washington.

What I want to do in this talk is to trace the history of thoughts about the general circulation from the early days up to 1970, and then see whether the more recent developments are what one might have expected. My own speculations (Lorenz, 1969) had been that there would be more interest in something other than the time-averaged or time-and-longitude-averaged circulation, and that the role of water would become increasingly prominent. I even speculated that the pressure systems of an earlier generation, which by 1970 were more often called circulation systems, might some day be called water systems. For the sake of brevity, I must limit the topics to be considered, and I shall concentrate on studies that have a bearing on observable patterns, such as zonal westerly currents or superposed large-scale eddies, rather than amounts of specific quantities, such as total energy.

Let me digress for a moment. The atmosphere is in a state of chaos. Here I am using the term "chaos" in a sense that it has acquired since 1970, to mean that, as a result of its sensitive dependence on its present state, the atmosphere may appear upon casual inspection to be fluctuating randomly, but upon closer examination may be seen to possess considerable regularity, without, however, exactly repeating its behavior at regular intervals. As a consequence, there are quite a few things that the atmosphere might do in the near future, even though there are far more things that it is certain not to do. In a time series of some atmospheric quantity, then, we may observe what appears to be cyclic behavior, but with detectable differences from cycle to cycle, until, at some point, something entirely different sets in. Fig. 1, although produced by a low-order atmospheric model (Lorenz, 1984) rather than real atmospheric data, illustrates the situation.

In a like manner, human behavior tends to be sensitively dependent upon what is presently happening, and is therefore chaotic. This is true not only of the totality of human activity, but also of many of the individual parts that make up the total, including such a minute portion as the study of the general circulation of the atmosphere. Indeed, as I have recounted elsewhere in greater detail (Lorenz, 1983), our ideas about the circulation appear to have undergone several similar but not identical cycles. Each cycle ends at a time when a dynamically consistent explanation for the observed circulation has finally attained general acceptance, but, almost concurrently, new observations are contradicting the explanation, and the next cycle begins. There follows a period when the new observations are rejected or ignored, and then one when they are accepted and new explanations are sought. Following some unsatisfactory explanations, a plausible one is found; for a while it is rejected or ignored, but ultimately it is accepted, and the cycle is completed. If we can define a time-dependent scalar quantity U (standing for unexplained), measuring the extent to which we have not yet logically explained those features of the general circulation of wich we are aware, a graph of U ending in 1970 might look somewhat like the early and middle parts of Fig. 1. In what follows we shall examine the four cycles of

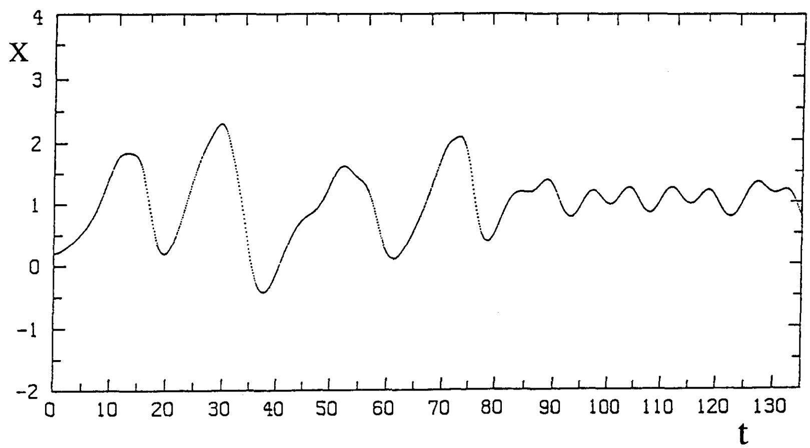

Fig. 1. Variations of the variable X in a 135-day segment of a particular solution of the equations of a 3-variable general-circulation model (Lorenz, 1984), with $a = 0.25$, $b = 4.0$, $F = 8.0$, and $G = 1.0$. Time t is in days.

U, and then see whether an extension of the graph beyond 1970 would exhibit similar cycles or would look more like the final part of Fig. 1.

It might be supposed that if every important new observation has demanded a changed explanation, none of the previous studies, except possibly the most recent one, has had any value. We should note, then, that not only the scope of our observations but also our idea as to what constitutes the general circulation is continually evolving. Successive explanations, no one of which is correct in its entirety, may very well contain successively more correct ingredients. We see no reason to start from scratch again and again.

2. The four cycles of U

Our account begins over 300 years ago, with $U = 0$, not because anything had been explained, but because it was not generally realized that there was a global circulation to be explained. Before the end of the seventeenth century, however, it was recognized that the trade winds exhibited rather similar behavior over the Atlantic, Pacific, and Indian Oceans. Halley (1686), best remembered today for his comet, is generally credited with having given the first rational explanation for some of this behavior. A very readable account of many of the early ideas appears in the Bakerian Lecture of Thomson (1892).

Halley maintained, as we do today, that differential solar heating provided the driving force for the atmosphere, and he assumed that this would produce rising motion at low latitudes and sinking farther poleward, thus necessitating equatorward flow at low levels. It is not certain, however, that he visualized a global circulation; his account simply suggests three separate oceans subjected to similar influences and consequently exhibiting similar responses. He noted the need for poleward return flow aloft, but did not suggest that air would pass from one ocean to another.

Halley attempted to account for the westward component of the trades as an additional tendency for the air to follow the sun, but here he failed to distinguish between motion toward the sun and motion in the direction in which the sun moved. Because of this shortcoming, the seventeenth century ended with $U > 0$.

The first rational and ostensibly complete explanation for the trades was given by Hadley (1735). In this famous paper Hadley attributed the westward flow in the trades to the dynamic influence of the earth's rotation. Effectively he had discovered the east-west component of the Coriolis force, although he underestimated its magnitude by a factor of two. He also recognized that if the surface easterlies in low latitudes were not to produce a continual slowing down of the earth's rotation, by means of their frictional drag, they would have to be accompanied by surface westerlies farther poleward; the latter he assumed were also produced by the Coriolis force, in the return flow aloft, after which the air would sink. He thus visualized a truly global circulation; this is shown schematically in Fig. 2. A single cell, now called a Hadley cell, occupies virtually the whole of either hemisphere. He evidently regarded any variations with longitude as extraneous details, and in this respect he set the tone for the investigations that were to follow more than a century later.

A minimum requirement for any picture of the general circulation to be dynamically consistent is that it contain a means for transporting absolute angular momentum, energy, and water from the latitudes where the atmosphere receives them to those where it gives them up. Hadley's theory

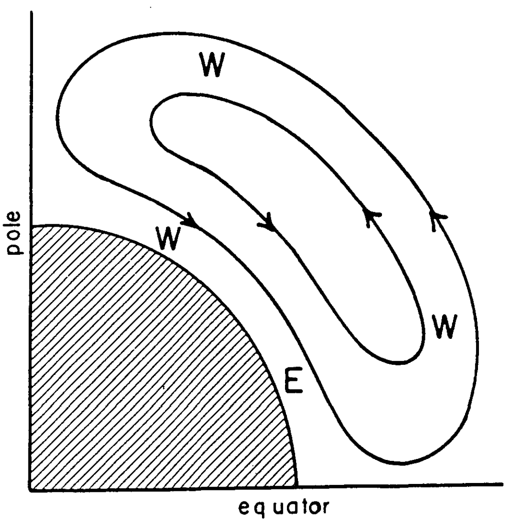

Fig. 2. A schematic view of a meridional cross-section of the general circulation as visualized by Hadley (1735). Streamlines indicate the meridional and vertical flow, while letters E and W indicate regions of easterly and westerly flow.

satisfies the requirements for angular momentum and energy, since the poleward-moving air aloft carries more angular momentum and energy than the equatorward-moving air below. The water requirement was more or less irrelevant in Hadley's day, since neither the sources and sinks of water nor the amounts in the assumed contrasting currents were known.

Hadley's ideas went almost unnoticed for half a century or more, and were even rediscovered on several occasions, but by the early nineteenth century they had replaced Halley's as the ones generally accepted. For practical purposes, U vanished again.

As observations of the atmosphere increased in scope, more facts needing explanation were discovered, and the general-circulation problem evolved into a more complex one. Even as Hadley's theory was acquiring general acceptance, observations (in the northern hemisphere) were revealing that the air in the low-level westerlies was drifting slowly poleward, rather than equatorward as Hadley's picture demanded, so that, again, U became positive.

There followed various attempts to revise the theory; the most rational were those of Thomson (1857) and Ferrel (1859), who invoked the north-south component of the Coriolis force, of which Hadley had presumably been unaware. They noted that, in high latitudes, friction should lead to a rapid downward decrease in the westerly wind speed, with no comparable decrease in the poleward pressure gradient. As a result of the unbalance, low-level poleward flow, if not present, would have to develop, until it became balanced by friction. The circulation that they envisioned appears schematically in Fig. 3; below part of the Hadley cell a second cell, now called a Ferrel cell, circulates in the opposing direction.

Things progressed more rapidly than they had a century earlier, and, before the end of the nineteenth century, Thomson's and Ferrel's ideas became generally accepted. Once more, and for the last time, U vanished.

The collapse of these ideas resulted from an extension of atmospheric observations to higher elevations. Following an international study of cloud motions (see Bigelow, 1900; Hildebrandsson and Teisserenc de Bort, 1900), a compilation of upper-level winds, deduced from these motions, indicated that the high-level poleward flow, so

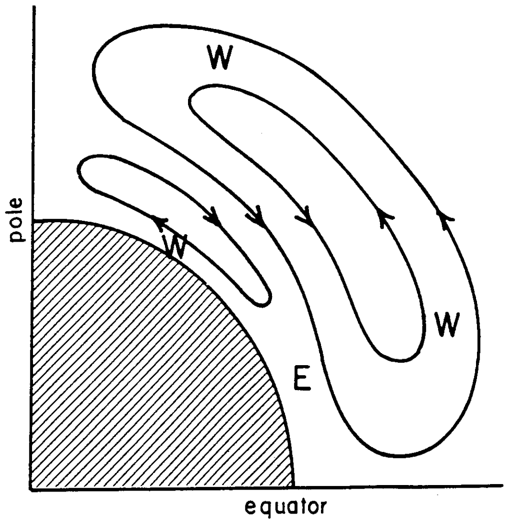

Fig. 3. The same as Fig. 2, but for the general circulation as visualized by Thomson (1857) and Ferrel (1859).

prominent in Figs. 2 and 3, did not extend from equatorial to polar regions, but terminated in lower middle latitudes. This forced the Ferrel cell to extend to a much greater height than indicated in Fig. 3. There was even a suggestion of an additional cell nearer the pole.

With the disappearance of the upper-level poleward current, the means for transporting angular momentum and energy from low to high latitudes was also lost. There followed numerous attempts to introduce new arrangements of cells, while still keeping everything longitudinally uniform. One after another these proved to be dynamically deficient.

Meanwhile a developing school of thought was maintaining that the circulation could not be explained without including the role of variations with longitude. Bigelow (1902), for example, proposed that northward and southward currents with different temperatures at different longitudes could account for the net poleward transport of heat. This idea encountered some skepticism, but tended to be better received following the work of Defant (1921), who identified the cyclones and anticyclones and other asymmetries with respect to the polar axis as a form of large-scale turbulence, and showed that the amount of heat that they carried poleward was consistent with a mixing-length theory.

It is remarkable that well before the work of Thomson and Ferrel, Dove (1837) has proposed a scheme much like Bigelow's, but his advanced ideas had been more or less ignored. The reasons are not altogether certain, but it appears that whereas Bigelow had maintained that the northward and southward currents influenced the general circulation, Dove had claimed that they were the general circulation. Such an idea was apparently unacceptable to a generation of scientists who had come to regard the general circulation as a time-and-longitude average.

With the importance of large-scale asymmetries now recognized, the problem of the general circulation underwent further evolution; it was now necessary to explain why these irregularities should be present at all. Here there were also two schools of thought. One group maintained that a circulation without asymmetries could not constitute a solution of the system of dynamic equations, even if the earth had no oceans and continents or other topographic irregularities. Their reasoning seems unacceptable today; if one introduces symmetric initial conditions into a symmetric general-circulation model where no asymmetries are forced, the flow will remain symmetric, and will not violate any dynamical principle.

The other group maintained that symmetric flow, even though dynamically possible, would be unstable with respect to asymmetric perturbations, so that asymmetric disturbances would inevitably develop and amplify to finite size. The clearest exposition of this idea was given by V. Bjerknes (1937).

At this point, U had again become fairly small. A picture of a circulation that gave rise to large-scale turbulence, and was tempered by this turbulence, seemed to conform with much of what was observed. Certainly U did not reach zero, since characterizing a phenomenon as turbulence does not explain the phenomenon as long as the properties of turbulence itself have not been fully explained.

Meanwhile, Jeffreys (1926) had proposed that the large-scale asymmetries could also account for the poleward transport of absolute angular momentum across middle latitudes. For some time his work was less favorably received than Defant's. At least across lower middle latitudes, such transport would be from zones of lower to higher absolute angular velocity, in disagreement with turbulence theory. Following World War II, however, Starr (1948) and J. Bjerknes (1948) proposed that routine upper-level observations had finally become sufficiently plentiful to allow one to evaluate transports of angular momentum on a day-by-day basis. There followed systematic efforts by groups directed by Starr and Bjerknes to compute the transports at various levels across various latitudes, first from winds estimated geostrophically from isobaric height data and eventually from observed wind data, and computational results came forth in ever increasing volume as the observing network grew and the observations reached higher levels. Within a few years, it became evident that the basic assumption of Jeffreys was correct, and that a turbulence theory of the general circulation lacked some essential ingredients.

Gradually, the idea that large-scale disturbances were turbulent eddies became replaced to some extent by the idea, clearly stated by Eady (1950), that they were manifestations of baroclinic instability. In one study, Charney (1959) was able to produce a fairly realistic although highly simplified picture of the circulation by assuming that, aside from their amplitude, the disturbances were like the ones that would grow most rapidly when superposed on the baroclinically unstable zonal flow that would have prevailed in the absence of disturbances. Again, following its fourth relative maximum, U became moderately small. Again U did not vanish; calling a phenomenon baroclinic instability does not fully explain it any more than would calling it turbulence.

3. Recent trends

What has happened since 1970? New observational discoveries have been abundant, but it is hard to identify any that have initiated a 5th cycle by forcing us to give up some ideas that we had thought were reasonably well established. Instead, they have led to further augmentation of what we accept as constituting the general circulation. The dominant characteristic of the recent work seems to have been diversification.

First of all, there have been horizontal and vertical extensions. In retrospect, the circulation that we had previously talked about could almost have been called the general circulation of the middle-latitude troposphere. The tropics entered the picture largely as an energy source for the middle-latitude motions, while the stratosphere entered mainly as a lid. Studies of the tropics and the stratosphere for their own sake now occupy a considerably bigger portion of our attention.

Next, we have been less exclusively concerned with time-and-longitude averages or even with simple time averages. For a long time we have, of course, been interested in time averages at specific times of the year; maps of "normal" circulation patterns have typically been presented separately for summer and winter, or sometimes for four seasons. More recently, however, we have become interested in time averages for specific phases of cyclic or quasi-cyclic phenomena, where the cycle is not a seasonal one.

An outstanding example is the El Niño-Southern Oscillation (ENSO) phenomenon. Both El Niño, as an oceanic phenomenon, and the Southern Oscillation, as an atmospheric one, have been recognized for a long time, but the direct connection between them is a more recent finding. Even though successive occurrences are by no means identical, or even of equal duration, we can identify "typical" circulation patterns for years with or without an El Niño occurrence. Closely tied in with ENSO is the Walker Circulation, a fluctuating cell in the equatorial plane, identified some time ago by Bjerknes (1969). Unlike the Hadley and Ferrel cells, the Walker cell would nearly disappear in an average over longitude.

Another non-seasonal cycle is the downward-propagating quasi-biennial oscillation (QBO), strongest in the equatorial stratosphere but detectable some distance from the equator. Discovered some time ago (see Reed, 1965), the QBO would now, like ENSO, be accepted by many as a feature of the general circulation. It also appears to be more easily explained than ENSO, and is apparently driven by upward-propagating shorter-period waves (see, Holton and Lindzen, 1972). Laboratory experiments supporting such an explanation have been performed by Plumb and McEwan (1978).

Still another oscillation with identifiable phases is the Pacific/North American (PNA) index, quantified by Wallace and Gutzler (1981). Typical patterns when the index is positive or negative show features extending well beyond the eastern Pacific and North American regions used in defining the index. Palmer (1988) has found evidence that the extended-range predictability of the atmosphere varies with the phase of the PNA. More generally, any true teleconnections (see Bjerknes, 1969) would now seem to qualify as features of the general circulation.

Perhaps the greatest diversification of all in recent years has been in methodology. One of the big fields of endeavor in meteorology today is general-circulation modeling; in 1970 it was still a little field. Indeed, an examination of recent titles in the *Journal of the Atmospheric Sciences* reveals that, at least in that journal, more articles are now regularly devoted to general-circulation modeling than to all other types of general-circulation research. A fair fraction of the papers are actually about the models, or about how to use them, rather than about the general circulation per se.

The original general-circulation model of Phillips (1956) was effectively a system of 450 coupled ordinary differential equations, converted to a system of difference equations for solution in discrete time steps. Some of today's models are smaller, but most are much larger, with as many as a million equations in refined operational models, such as the one developed and used at the European Centre for Medium Range Weather Forecasts (ECMWF).

General circulation modeling could probably never have been developed without the development of large computers. Actually one could numerically solve the equations of a very crude model, say one with twelve equations, by hand, and obtain useful results in roughly the same amount of time that would be subsequently needed to write up the results, provided that the model behaved suitably. It is unlikely, however, that such a task would ever have been completed, since our experience with computers has indicated that many models must usually be tested before a suitable variant is discovered.

One often hears the comment that if we succeed in nearly duplicating the observed circulation with a model, we have learned only that the model is a good one. In some instances this may actually be just what we wish to learn, i.e., we may wish to know whether or not the processes that we have built into the models are the truly important ones.

Recent experience with the ECMWF model offers an example. As with any operational model, modifications are frequently introduced in the hopes of improving the forecasts. Recently, observable improvements appear to have been obtained by removing the previously included vertical diffusion of momentum, except in a layer near the surface, and also by introducing a gravity-wave drag in the stratosphere. Each of these changes would appear to improve our picture of what processes in the real atmosphere are really important. Of course no model can include everything, and there always remains the possibility that some unsuspected process would have produced the same improvement as some process being tested.

Once a model has been established, it may be used to obtain a step-by-step picture of how a particular cause leads to a particular effect; a task that might not be feasible if one had to rely on the less complete observations of the real atmosphere. Necessarily, the reliability of the conclusions will depend on the trustworthiness of the model. This will naturally depend on, among other things, the manner in which processes on too fine a scale to be revealed by the model, such as cumulus convection or the influence of a rough underlying surface, have been parameterized.

At the other extreme, those who prefer to use analytical rather than numerical methods often find it convenient to use equations that have been at least partially linearized. In a way, what is now regarded as the general circulation may be less nonlinear than the general circulation as seen in 1970. Wave motion seems to play a greater role; perhaps this was already foreshadowed by the tendency to look upon large-scale disturbances as manifestations of baroclinic instability instead of turbulence. Numerous recent studies have dealt with *vertically* propagating gravity waves and Rossby waves; these waves have appeared more clearly relevant to the general circulation with the increased emphasis on the stratosphere.

Breaking Rossby waves have apparently been observed at high levels (see McIntyre and Palmer, 1983). Synoptic study of these waves has been considerably enhanced by the analysis of fields of potential vorticity on surfaces of constant potential temperature.

A remarkable development has been a simple transformation of variables in the equations governing the longitudinally averaged circulation (see, Andrews and McIntyre, 1976); this effectively removes a term from one equation and adds one to another. The outcome is that, in the transformed coordinate system, the transport of wave energy is parallel to a vector, now called the Eliassen-Palm flux, whose components are proportional to the poleward transports of angular momentum and sensible heat, while the transfer of wave energy to the zonal flow is proportional to the convergence of the Eliassen-Palm flux. Investigation of the wave behavior has been aided by the construction of Eliassen-Palm cross sections, introduced by Edmon et al. (1980), which show the instantaneous flux and its convergence as functions of latitude and elevation. Edmon et al. have also shown that observed flux patterns are often somewhat like the average flux during the life history of an idealized baroclinically evolving wave, but evidently not like the flux pattern of an incipient wave.

4. Concluding remarks

We have seen that the study of the general circulation of the atmosphere has followed a systematic but not particularly predictable course; the study of our chaotic atmosphere is itself a chaotic process. In particular, our ideas as to what constitutes the general circulation have advanced chaotically. Over a span of some 300 years, ending about 1970, a quantity U, representing the extent to which we have not explained those features of the general circulation of wich we are aware, increased and subsequently decreased four times, the cycles becoming more closely spaced as the years progressed. Since 1970, U has continued to vary, but not in a similar cyclic manner.

My own predictions as to what would happen after 1970 have been only partially fulfilled. We still see considerable relevance in time-and-longitude averages, even though we have become decidedly more interested in other features. The role of water has received moderate but not yet major attention, and, in fact, some of the most interesting general developments have involved the stratosphere, above the levels where much water is present. The extent to which numerical modeling has dominated the field was unanticipated.

What about the next twenty years? Recognizing more clearly than I did twenty years ago that general-circulation research is a chaotic process, I am reluctant to make a prediction.

5. Acknowledgment

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