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A HISTORY OF PREVAILING IDEAS ABOUT THE GENERAL CIRCULATION OF THE ATMOSPHERE

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ABSTRACT

During the past three centuries the prevailing ideas about the general circulation of the earth's atmosphere have evolved in a stepwise manner. Early in each step a new theoretical idea is formulated. Late in each step the idea gains general acceptance, but, more or less concurrently, new observations show that the idea is wrong. An account of three steps and part of a fourth is presented.

The general circulation of the earth's atmosphere has been the subject of many excellent studies during the last three centuries. Throughout much of this period the problem of the general circulation has been looked upon as one not yet solved, but offering a readily understandable qualitative solution. This situation has undoubtedly contributed to its popularity as a research problem. The continual appearance of new ideas has been interspersed with histories of these ideas; one could almost write a history of histories of the general circulation. Some of these accounts have appeared as introductions to presentations of new results (e.g., Hildebrandsson and Teisserenc de Bort, 1900). Others are found in textbooks or survey articles (e.g., Hann 1901). Perhaps the most readable history of all is contained in the Bakerian Lecture of Thomson (1892).

The present summary is based upon a rather detailed historical account prepared a decade ago (Lorenz, 1967; see pp. 1-4, 59-78). The reader is referred to that account for details not found in this summary.

The prevailing ideas have evolved in a manner which appears to be far from random. Indeed, to a present-day dynamic meteorologist, an account of the development of these ideas is suggestive of a giant stepwise numerical integration, with time steps of half a century or longer. At the beginning of each step, certain ideas appear more or less as established facts in the standard texts, but are questioned by the avant-garde. Within each step there occur a formulation of new theoretical ideas, an interval in which these ideas are rejected or simply ignored, an interval of fairly general acceptance, a more or less concurrent discovery of observational facts which contradict the new theory, an interval in which these observations are ignored or questioned, and a final acceptance of the new observations and a rejection of the theory by the new avant-garde. To many readers our time steps will be more suggestive of innings.

The initial time in our summary is the early eighteenth century. The generally accepted theory of the trade winds had been formulated by the astronomer Edmund Halley (1686), who is well remembered today for the comet which bears his name. Halley had carefully noted the presence of similar wind systems in three separate oceans, and had sought a common explanation. He identified solar heating as the driving force; this he believed would cause the air to rise in low latitudes and sink in high latitudes, whence the equatorward drift in the trade winds, and a poleward drift aloft, would follow from mass continuity. He maintained that the westward drift in the trades would likewise result from the tendency of the air to to follow the sun.

Here he seems to have made an error in logic which is as common in qualitative reasoning today as it was then; he failed to distinguish fully between a quantity and its time derivative. Thus "following the sun" appears to mean moving toward the sun when applied to the north-south motion, but it means moving in the direction in which the sun is moving when applied to the east-west motion. Halley did not discuss the middle latitude westerlies, and his work cannot be equated to a theory of the global circulation.

The opening event in the first step is the famous paper of George Hadley (1735). This account has been retold so many times that another repetition appears superfluous, but a few points should be mentioned. First of all, it introduces a new physical concept - the tendency of air to retain its present absolute angular momentum as it moves over a portion of the earth's surface having greater or less angular momentum. This tendency is precisely what we now call the east-west component of the Coriolis force.

Hadley had accepted Halley's ideas regarding the north-south motion. He therefore deduced that the equatorward moving air at low levels would be deflected westward, while the air returning poleward at higher levels would be deflected eastward. He invoked friction to explain why the easterly and westerly winds would not be much stronger than observed, and then noted that the presence of low-latitude easterlies, with their westward frictional drag on the earth, demanded the existence of westerlies at other latitudes, with an opposing drag. His account thus embraces the concept of a global circulation, whose various branches cannot be explained independently of one another. Fig. 1a shows this circulation schematically; a single thermally direct cell occupies each hemisphere.

For a number of years Hadley's paper remained virtually unknown - so much so, in fact, that the idea was rediscovered first by Immanuel Kant (1756), also without attracting attention, and later by John Dalton (1793). Perhaps it was partly because Dalton subsequently learned, and acknowledged, that he had been completely anticipated by Hadley that Hadley's paper finally gained notice. By this time, however, new observations were becoming more plentiful. At about the time that Hadley's theory became the generally accepted one, the observations revealed that the theory was wrong; the surface westerly winds in middle latitudes possessed a poleward drift, rather than an equatorward drift as the theory demanded.

The second step begins with various attempts to reconcile Hadley's physical reasoning with the new observations. The works with the most lasting influence were the rather similar ones of Thomson (1857) and Ferrel (1859). Like Hadley's work, they were founded upon a new physical concept - the presence of a greater, or smaller, centrifugal force acting upon air which rotates more rapidly, or less rapidly, than the underlying earth. This tendency is of course what we now call the north-south component of the Coriolis force.

Thomson and Ferrel accepted Hadley's ideas regarding the lower latitudes, and regarding higher levels in the remaining latitudes. They likewise invoked friction, and noted that this would cause the westerlies to decrease very rapidly with decreasing height through a shallow layer near the surface, while, in view of hydrostatic considerations, the northward pressure gradient would decrease only slowly. There would therefore be a substantial unbalance of forces near the surface, causing the lowest layers of air to proceed poleward, in agreement with observations.

Fig. 1b shows Thomson's circulation schematically. Ferrel differed with Thomson principally in not extending his low-level thermally indirect cells into the polar regions.

Thanks largely to Ferrel's continued writings, the new ideas gained attention much more quickly than had Hadley's a century earlier. But new observations were also accumulating more rapidly. At the end of the nineteenth century, just as the new view of the circulation was becoming generally accepted, the International Meteorological Organization was completing a study of upper-level winds, deduced from the motions of high clouds (see Hildebrandsson and Teisserenc de Bort, 1900). This study revealed that there was no high-level poleward current from tropical to temperate latitudes, as Thomson's and Ferrel's theories, and also Hadley's, would have demanded.

Early in the third step the ideas assumed divergent paths. A feature of the generally accepted theories had been a complete symmetry of the circulation pattern with respect to the earth's axis. This does not mean that the proponents of these theories had been unaware of the prevalence of intense storms and other departures from axial symmetry. Ferrel even wrote about the general circulation and storms in separate paragraphs of the same paper. But he never suggested that the general circulation and the storms might somehow influence one another.

Along one path, some investigators added more and more cells to Thomson's picture of the circulation, so as to establish compatibility with the new observations without destroying axial symmetry. One after another, those patterns which were not obviously physically impossible were found to disagree with still newer observations.

Along the other path, the idea began to emerge that the general circulation, which by now had come to mean the axially symmetric portion of the circulation, could not be explained independently of the storms which were superposed upon it. The idea was stressed by Bigelow (1902), who pictured an asymmetric circulation in higher latitudes, with cold and warm equatorial and poleward currents flowing side by side, and with storms developing as these currents interacted. It had been realized that the excess energy received from the sun in low latitudes had to be transported within the atmosphere to higher latitudes before being discharged, and the uniform upper-level poleward current had supposedly formed the means for this transport. When this current was found not to exist, an alternative transport mechanism had to be found. Bigelow maintained that the cold and warm adjacent currents provided the mechanism.

At this point we must turn back a full step to the ideas of the eminent meteorologist Dove (1837). He accepted Hadley's ideas regarding the lower latitudes, but described the middle-latitude circulation as consisting of alternate longitudes of north winds and south winds. Dove's "winds" appear to be the same as what we now call polar and tropical air masses. He regarded the migratory storms as originating from a conflict of these winds. His description resembles the one which we have attributed to Bigelow.

The reader may well inquire why we waited until this point to introduce Dove's advanced ideas. It is true that we could write a tidier story by pretending that Dove's work never existed, but this is not sufficient reason for doing so. Many historical accounts appearing in the later nineteenth century did, in fact, ignore Dove altogether. We may guess why they did so after examining the original edition of the excellent treatise of Hann (1901), who made no mention of Dove in his chapter on the general circulation, but described his work in detail in the following chapter on storms. Evidently the phenomena which Dove had so carefully observed were not considered by nineteenth-century meteorologists to be the general circulation. As a consequence, his work failed to influence subsequent general-circulation studies. Dove had not proposed, as Bigelow later did, that the north and south winds formed the principal mechanism for the heat transport. This is understandable; there was no reason then to suspect that the upper-level poleward current was absent.

The idea that asymmetries were essential to the general circulation received only minor support until Defant (1921) proposed that the motions in middle latitudes were simply a manifestation of turbulence on a very large scale. Defant went beyond Bigelow by applying the results of turbulence theory to estimate the amount of heat which would be transported poleward by turbulent eddies with diameters of thousands of kilometers. He found that this agreed well with the required transport, and concluded that his ideas were confirmed.

Asymmetries, whether or not they are regarded as turbulence, require an origin, and a suitable explanation was provided by V. Bjerknes (1937). He sought the circulation pattern which would develop if it were forced to remain symmetric, and concluded that it would look much like the patterns favored by Thomson and Ferrel. He maintained, however, that these patterns would be unstable with respect to asymmetric disturbances of small amplitude. Fully developed asymmetries would therefore characterize the actual circulation.

Defant's description of the cyclones and anticyclones as turbulence, with its connotation of randomness, seems to have encountered some resistance. It should be remembered that the meteorologists who studied the general circulation and those who studied cyclones were not disjoint groups; to a considerable extent they were the same persons. Having dealt with cyclones in detail, and having identified certain regularities in their structures, they may have been reluctant to look upon them now as mere random eddies. Nevertheless, the idea that cyclones, like random eddies, should diffuse heat, and thus act to smooth out the symmetric portion of the temperature field, met with considerable favor.

This idea naturally extended itself to the motion field. It was pursued most vigorously by Rossby (1941, 1947). By postulating a diffusion of momentum, and later a diffusion of vorticity, Rossby was able to deduce flow patterns which agreed fairly well with reality. For a time his ideas were the ones quoted in the standard texts.

The refutation had its origin in the work of Jeffreys (1926). It had been realized that angular momentum as well as heat had to be transported poleward within the atmosphere, and the absence of a uniform upper-level poleward current, which had been thought to provide the mechanism, was posing further problems. Jeffreys proposed that this transport, like that of heat, was accomplished by the asymmetric eddies.

His ideas were not received enthusiastically. The transports which were deduced by applying turbulence theory were quite unlike those needed to fulfill the global balance requirements.

Following World War II, J. Bjerknes (1948), Priestley (1949), and Starr (1948) independently proposed that upper-level observations had now become plentiful enough for the direct evaluation of transports of angular momentum on a day-by-day basis. The ensuing computations confirmed what Jeffreys had maintained; throughout much of the atmosphere angular momentum was actually transported from latitudes of low to latitudes of high angular velocity, in opposition to what was demanded by turbulence theory. The third step had been completed.

It is more difficult to view the fourth step, which is currently in progress, from a historical point of view. A prevailing idea, clearly stated by Eady (1950), appears to be that cyclones and other asymmetries should conform to baroclinic-stability theory. Charney (1959) was able to deduce a fairly realistic circulation by postulating that the asymmetric disturbances, although of finite size, would assume the same shapes as the disturbances which, while of infinitesimal size, would amplify most rapidly. Work along these lines continues.

If our own most recent view of the general circulation (Lorenz, 1969) is accurate we may be nearing the end of the fourth step. We have pictured a circulation which, if not easily explainable in simple sentences (except by calling it a baroclinic-instability phenomenon), can at least be duplicated in its main features by numerical solutions of fairly realistic approximations to the governing dynamic equations. The statistics which have been evaluated from these solutions compare fairly well with those determined from real atmospheric data. There is a comfortable feeling that the problem is nearly solved.

We may therefore pause and ask whether this step will be completed in tha manner of the last three. Will the next decades see new observational data which will disprove our present ideas? It would be difficult to show that this cannot happen.

Our current knowledge of the role of the various phases of water in the atmosphere is somewhat incomplete; eventually it must encompass both thermodynamic and radiational effects. We do not fully understand the interconnections between the tropics, which contain the bulk of the water, and the remaining latitudes. Satellite observations have revealed various features, such as a frequent continuum of clouds extending northeastward from the tropical Pacific into the central United States, which were not previously recognized. Perhaps near the end of the twentieth century we shall suddenly discover that we are beginning the fifth step.

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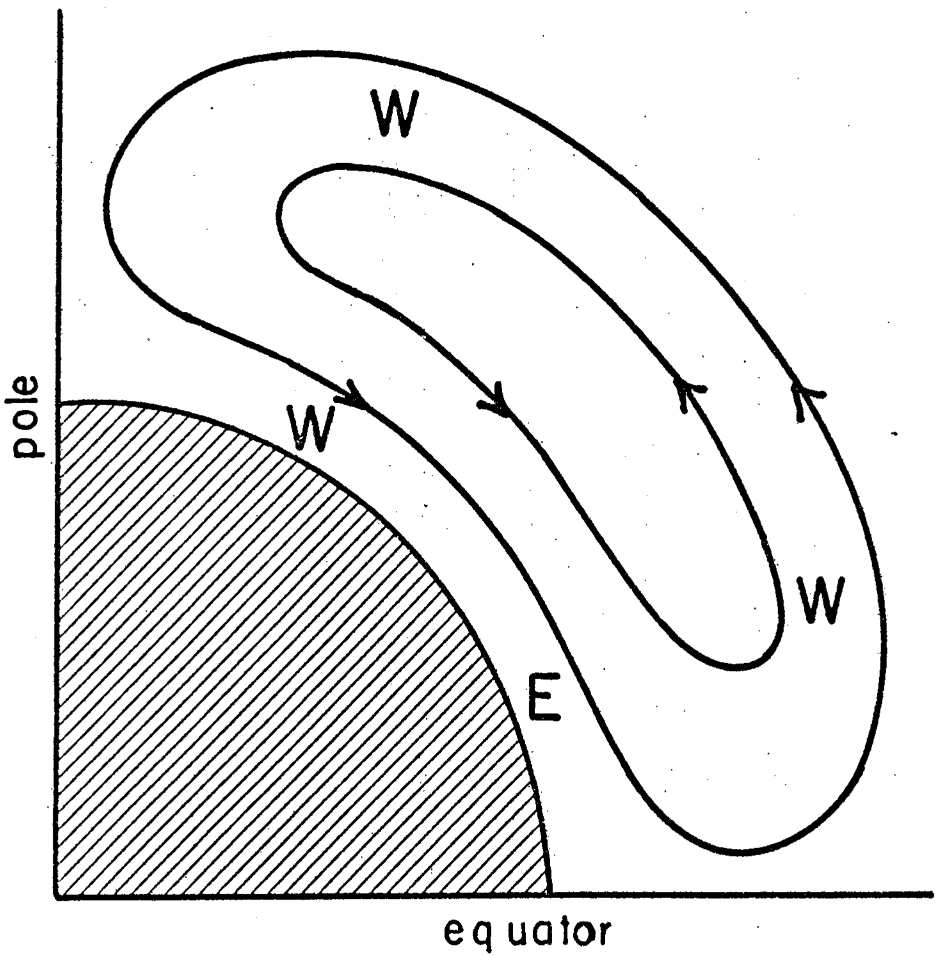
CAPTION

Fig. 1 a) Equator-to-pole cross section of the earth and the atmosphere, showing the symmetric circulation pictured by Hadley (1735). Streamlines indicate north-south and vertical motion. Letters E or W indicates motion from the east or west. b) The same, for the symmetric circulation pictured by Thomson (1857).



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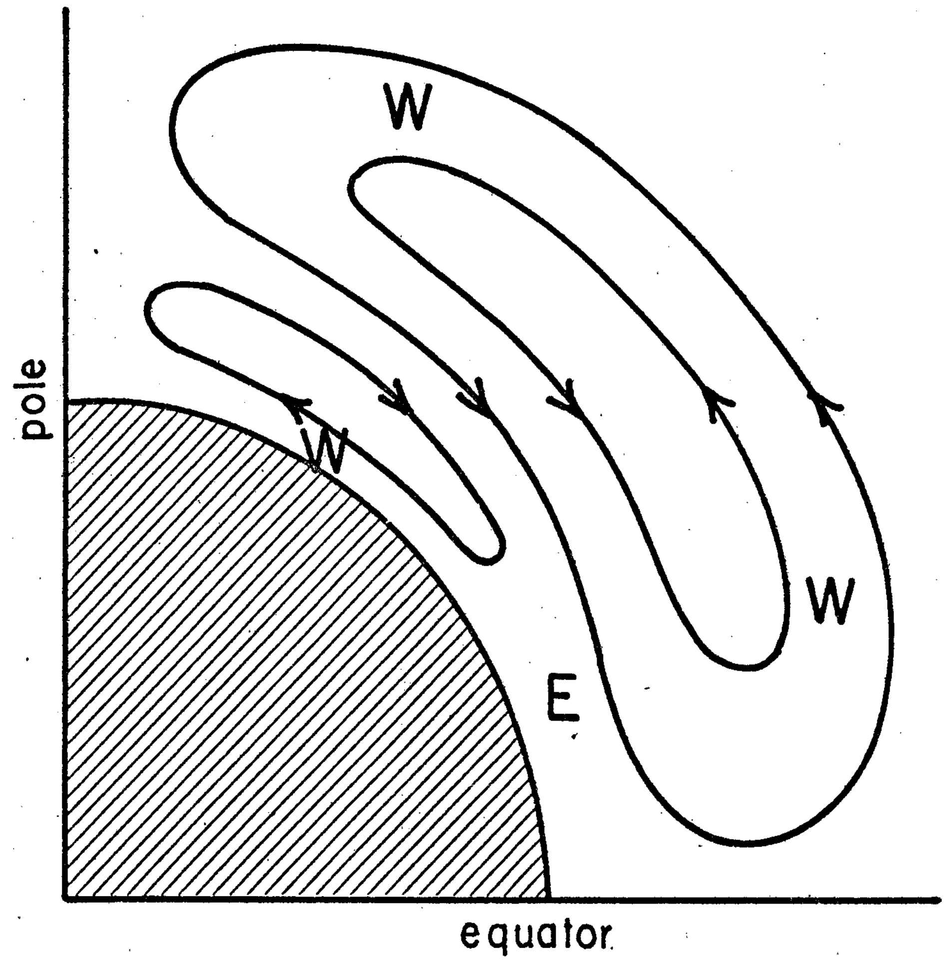

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