CHAPTER III

THE OBSERVED CIRCULATION

The characteristic features of the circulation of the Earth's atmosphere form a rather heterogeneous set. They include such familiar qualitative properties as approximate hydrostatic equilibrium, which prevails throughout the atmosphere, and approximate geostrophic equilibrium, which prevails in middle and higher latitudes. They include the quantitative spatial distributions of simple statistics such as the time-averaged wind velocity, and more complicated statistics such as joint probability distributions. Finally they include the existence of such entities as fronts and migratory cyclones, whose presence may not be apparent from an inspection of the quantitative time averages. All of these features serve to distinguish the circulation of our atmosphere from the circulations which may be found in other fluid systems.

In this chapter we shall consider the various aspects of the observed circulation. We shall give particular attention to the fields of motion, temperature, and moisture, averaged with respect to longitude and time. We shall see in the following chapters that even a partial explanation of these fields requires careful consideration of many of the remaining aspects, including the structure of cyclonic and anticyclonic disturbances.

Measurement of the circulation

The meteorologist who wishes to observe the circulation of the atmosphere cannot follow the customary procedures of the laboratory scientist. He cannot design his own experiment in such a manner as to isolate the effects of specific influences, and thereby perhaps disprove certain hypotheses and lend support to others. He can only accept the circulation as it exists. Moreover, since the circulation is global in extent, he cannot even make his measurements singlehandedly, but must rely for the most part upon those which have been made by other persons, in most instances for different purposes.

If the circulation were completely steady, the task of measuring it could be a straightforward matter of geographical exploration. Expeditions could travel to various points of the globe, in the manner of the famous Challenger Expedition of nearly a century ago (see Buchan, 1889), to measure the weather elements at various elevations, and the data gathered by these expeditions could be assembled into a three-dimensional picture of the atmosphere. If the circulation pattern varied with the time of the day and the season of the year, but not otherwise, the task would be prolonged but not too greatly complicated.

But perhaps the most easily observed characteristic of the circulation is its unsteadiness. Fluctuations occur on all space scales and all time scales, and include simple gusts and lulls in the local winds, the development and decay of individual thunderstorms, the passage of migratory cyclones and anticyclones, the extended-period oscillations between high-index and low-index circulation patterns, and the world-wide changes which presumably took place as the prehistoric continental ice-sheets advanced and retreated. If attention is confined to features of larger horizontal scale, the fluctuations of shorter time-scale largely disappear, but the long-term fluctuations remained undiminished.

In Chapter I we noted that the unsteadiness of the atmosphere offered us a choice of theoretical problems; we could consider the sequence of instantaneous weather patterns or confine our attention to the long-term statistical properties of these patterns. We chose the latter problem. We noted that it would be convenient to explain the long-term properties without first deducing the instantaneous patterns, but the possibility did not seem promising. It would likewise be convenient to measure the long-term properties without first observing the instantaneous patterns, but this possibility is also precluded. We are forced by our standard instruments to measure instantaneous quantities, or, more precisely, very short-term averages, since the instruments do not respond to the most rapid fluctuations. The desired long-term statistics must then be estimated by processing the instantaneous measurements.

Ideally the long-term average circulation is the limiting value of the average over a long interval, as the interval becomes infinite. For an idealized atmosphere which is governed by specified mathematical equations, the existence of such an average is sometimes assured. For the real atmosphere such an average, even if it does exist, is not likely to be the one which we desire, or which we can readily estimate from our available data. Over sufficiently long intervals the geographical features will change, and the atmospheric circulation will presumably change in response to the changed geography. Perhaps the most that we can hope to estimate from modern data is the long-term average circulation which would prevail if the geographical features of the Earth, and the output from the sun, could be prevented from undergoing any further changes.

We can never precisely determine long-term averages by computing averages over shorter intervals. One-week averages vary within a season, seasonal or annual averages vary within a decade, and ten-year averages vary within a century. All the weather data ever collected form no more than a statistical sample. The saving feature is that averages over separate intervals of a season or longer are often near enough alike so that either one affords an acceptable estimate of the other for many purposes; thus it is likely that both are acceptable estimates of longer-term averages.

It would therefore appear that averages over complete seasons are a minimum requirement, while considerably longer-term averages are preferable. It would clearly be impossible for one investigator or one small organization to embark upon a programme of performing all the needed measurements. Fortunately, weather data have been collected on a routine basis for a century or more at many locations, and the set of upper-level wind, temperature, and humidity observations which have been made daily or several times a day at each of several hundred stations during the past ten to twenty years forms one of the most remarkable data collections existing in any science.

These observations have been made primarily for the purpose of weather forecasting, and it is doubtful that any other potential use would have brought forth the necessary funds. The needs of weather forecasting have dictated to a considerable extent the locations of the observing stations, which are most abundant in densely populated regions. Over much of the ocean they are virtually absent; even during the International Geophysical Year there was but one southern hemisphere station reporting upper-level winds in the 50 degrees of longitude immediately west of South America. A more regular distribution of stations, perhaps at the intersections of standard parallels and meridians, would better serve the interests of global-circulation research. Nevertheless, the study of the circulation owes a great debt to the practice of weather forecasting, for without these observations our understanding could not have approached its present level.

Yet certain gaps will continue to exist in our knowledge of the circulation as long as extensive regions without regular observations remain. If we acknowledge that the density of observations in the more heavily populated regions is adequate, we must conclude that the present situation could in principle be remedied simply by establishing more weather stations in sparsely populated areas, and maintaining a sufficient number of weather ships in those parts of the oceans where there is little commercial shipping. But even a single weather ship is a costly affair, always vulnerable to discontinuation by an economy-minded government, while even a hundred weather ships would not provide sufficient coverage. If truly global weather information is to be obtained in the foreseeable future, some other procedure must be adopted.

Current plans call for international participation in the World Weather Watch — a truly global observation system in which the World Meteorological Organization will play a co-ordinating role. The primary aim of this system will be the further improvement of weather forecasts, particularly at a range of several days, but it is also intended that the system should enhance our understanding of the global circulation, especially since this understanding seems to be a prerequisite for successful extended-range forecasting. Certain feasibility studies are already in progress. In addition to the expansion of the existing observational network to cover less densely populated land areas, three techniques of observation which are not presently in routine use are under consideration.

First, under the proposed scheme several thousand balloons are to be kept aloft, drifting with the wind at various constant levels. Successive observations of their positions will make it possible to compute the wind fields at these levels. On an experimental basis a number of these balloons have been released in the southern hemisphere. Several balloons drifting at 200 mb have made one or more complete circuits about the globe, and, on 31 December 1966, one balloon had been aloft for 220 days, and had completed 18 circuits, while its latitude oscillated between subtropical and subpolar regions. At 500 mb the balloons have thus far been prevented from remaining aloft for long intervals by the accumulation of ice.

Second, the balloon observations are to be supplemented by more conventional measurements from a large number of floating buoys. These may be anchored or allowed to drift. Like the balloons, the buoys may be considered expendable, to be replaced as needed.

Finally, artificial satellites are to be used for remote sensing of the atmosphere. A wealth of information may be gathered by measuring the radiation received from the atmosphere or the underlying Earth over a wide portion of the spectrum. Measurements of infra-red radiation in several wavelengths are expected to yield reasonably reliable vertical profiles of temperature and humidity above the cloud-tops. Observations in visible light will continue to disclose the cloud systems of various sizes. Measurements in the ultra-violet are expected to reveal the distribution of ozone. It has even been proposed that the satellite, in addition to measuring natural radiation, may carry a downward-directed laser, and gain further information by measuring the back-scattered light.

The other essential role of the satellite will be one of data-collection and transmission. Many of the balloons and buoys will be in remote regions where conventional means of transmitting their measurements will be impracticable. It is planned that a balloon or buoy will store its information until such a time as one of a set of communication satellites passes overhead. At this time the information will be transmitted to the satellite. Later, when the satellite passes over a receiving station, it will retransmit its information, which can then be dispatched by conventional procedures.

Despite the well-known high cost of satellites, such a system is expected to be far less expensive than a large fleet of weather ships, and the necessary funds can reasonably be expected to be forthcoming, at least for a long enough time to determine whether the system is feasible. One can only speculate now as to the eventual benefits to be gained from such a system, but it may well mark the beginning of a new era in the measurement of global weather.

Hydrostatic and geostrophic equilibrium

The most prominent qualitative characteristic of the circulation is the presence of hydrostatic equilibrium, i. e. the approximate balance between gravity and the vertical pressure force. Closely associated with hydrostatic equilibrium is the tendency of the motion to be nearly horizontal. It may be noted that in small-scale turbulent flow or intense cumulus convection the vertical velocities are not small compared to the horizontal velocities, while the smallest-scale features need not even be hydrostatic. It is these very features which we prefer not to regard as part of the circulation. The smoothed circulation, which remains when the small-scale features have been subtracted out, is quasi-hydrostatic and quasi-horizontal.

The familiar hydrostatic equation expressing hydrostatic equilibrium (equation 30) may be written in a form relating pressure, temperature and elevation thus:

$$\partial\,(\ln p)/\partial z = -\,g/(RT). \tag{74}$$

For a moist atmosphere the slightly higher virtual temperature should replace the temperature.

It is perhaps somewhat odd that hydrostatic equilibrium should head a list of observed properties of the circulation, because it is not actually observed on a day-by-day basis; it is taken for granted. Ever since the barometer was invented the supposition that it measures the total weight of a column of air has seldom been seriously questioned. Nearly all routine upper-level observations measure temperature and humidity as functions of pressure, and the elevations at which the specific measurements are made are then calculated with the aid of the hydrostatic equation. Such measurements can neither confirm nor deny hydrostatic equilibrium. Nevertheless, the barometer has been used as an altimeter on countless occasions other than routine weather observations, even to measure small differences in elevation. The general agreement obtained whenever these measurements are compared with measurements by other methods indicates that approximate hydrostatic equilibrium is practically always present.

Moreover, appreciable departures from hydrostatic equilibrium, other than those of short duration which are associated with small-scale motions, would soon lead to large vertical motions, which are not observed. Although the average upward velocity over a large area cannot be directly measured by any existing technique, it is reasonable to assume that it could easily be measured if it were comparable in magnitude to a typical horizontal velocity.

The importance of hydrostatic equilibrium lies in the restrictions which it places on the form which the circulation may assume. The measurement, description, and explanation of the circulation are thereby greatly facilitated. As we have noted, once hydrostatic equilibrium is accepted, it is not necessary to measure both pressure and temperature as functions of elevation; it is sufficient to measure temperature as a function of pressure. The fields of pressure and temperature (or virtual temperature) become manifestations of the same field — the field of mass. The pressure field completely determines the temperature field, while the temperature field, together with the distribution of pressure at sea-level or any other single level determines the pressure field. Contrary to what has often been assumed, however, there is no method of inferring the sea-level pressure field hydrostatically from the three-dimensional temperature field alone.

Hydrostatic equilibrium also renders it unnecessary to measure the nearly unmeasurable vertical-velocity field. The field of vertical motion is assumed to be that field which is needed to maintain hydrostatic equilibrium by offsetting the tendency of the horizontal motions to disrupt it. Moreover, if hydrostatic equilibrium itself can be explained, it is no longer necessary to explain the vertical motion in terms of unbalanced vertical forces; the vertical motion must be that which is required to maintain the equilibrium. This motion is ordinarily found to be very weak compared to the horizontal motion.

In the previous chapter we observed that the presence of hydrostatic equilibrium rendered it convenient to use pressure as the vertical co-ordinate in the system of dynamic equations, and to let height become a dependent variable. It is equally convenient to use pressure as the vertical co-ordinate in presenting the observations. Since World War II routine weather reports have been transmitted in the form of heights, temperatures, humidities, and winds at standard pressures, and constant-pressure weather maps have replaced the previously used constant-level maps.

In this co-ordinate system the hydrostatic equation becomes

$$\partial z/\partial\,(\ln p) = -RT/g\,. \tag{75}$$

Our earlier remarks concerning the fields of pressure and temperature now apply to the fields of height and temperature.

Further qualitative properties which are somewhat analogous to hydrostatic equilibrium and quasi-horizontal motion and nearly as prominent are geostrophic equilibrium, i. e. the approximate balance between the horizontal Coriolis force and the horizontal pressure force, and quasi-non-divergent motion. Again, these properties are not characteristic of small-scale motions.

The geostrophic equation expressing geostrophic equilibrium may be written (see equation 61)

$$\mathbf{U} = (g/f)\,\mathbf{k} \times \nabla z\,. \tag{76}$$

Thus it relates the wind to the slope of a constant-pressure surface. Combined with the hydrostatic equation, it becomes the thermal wind equation

$$\partial \mathbf{U}/\partial p = -(R/fp)\,\mathbf{k} \times \nabla T \tag{77}$$

which relates the vertical shear of the wind to the gradient of temperature along a constant-pressure surface.

Whereas the hydrostatic equation is often treated as exact, the geostrophic equation must be treated as only a fair approximation for many purposes. It is especially unreliable in low latitudes. Thus it has become customary to distinguish between the wind $\mathbf{U}$ and the geostrophic wind $\mathbf{U}_g$, the latter being the wind which would have to accompany the existing height (or pressure) field in order to render equation (76) exact, and therefore being defined by the right side of (76). It would be equally logical to speak of the geostrophic height gradient (or pressure gradient) in referring to the gradient which would have to accompany the existing wind field to render (76) exact, but these expressions do not seem to be in common use. The unfortunate result has been a frequent tendency to assume that the wind field is somehow produced geostrophically by the height field, and to overlook the possibility that the fields determine one another through mutual effects.

Unlike hydrostatic equilibrium, geostrophic equilibrium is directly revealed by modern routine observations. Historically it was not always recognized. During the early nineteenth century there was considerable debate as to whether the wind tended to blow around or into a cyclone. By the middle of the century observations had become sufficient to decide in favour of the former alternative. Near the Earth's surface there is also a noticeable "frictional" component toward low pressure, which undoubtedly made the interpretation of the earlier observations more difficult.

Geostrophic equilibrium, like hydrostatic equilibrium, is important in that its presence facilitates the measurement, description and explanation of the circulation. Where wind measurements are absent, the geostrophically measured wind may often be used with fair confidence. Until fairly recently much of our knowledge of upper-level winds was derived in this manner. To the extent that geostrophic equilibrium prevails, the wind, height (or pressure), and temperature fields become separate manifestations of a single field.

The field of horizontal divergence is not quite strong enough to be reliably determined directly from the wind observations. It is often assumed to be that field which is needed to maintain geostrophic equilibrium by offsetting the disruptive effects of the rotational part of the wind. If geostrophic equilibrium can be explained, the field of divergence need not be explained in terms of unbalanced forces; it must be the field needed to maintain equilibrium. This field is generally found to be weaker than the field of vorticity. It must be emphasized, however, that the geostrophic approximation does not compare in accuracy with the hydrostatic approximation, and for many purposes separate measurements of wind and height (or pressure) and direct measurements of the divergence are desirable; this is especially true in tropical regions.

Resolution of the circulation

The observed fields of motion, temperature, and moisture in the atmosphere cannot be represented by any simple analytic formulae, and quantitative statistics of the circulation are most easily presented in the form of tables or graphs. The three-dimensional spatial distribution of any particular statistic, such as the time-averaged wind velocity, can be reasonably well represented by a set of two-dimensional charts, which may be horizontal maps or vertical cross-sections. However, a collection of charts consisting of a separate set of maps or cross-sections for every statistic of interest would be altogether unwieldy, and it would be quite incomplete if the statistics were limited to such familiar quantities as means and standard deviations. While a map of the time-averaged field of motion might afford a good description of the trade winds, it would not reveal the prevalence of migratory cyclones in higher latitudes. Maps of covariances at suitable time-lags and space-lags might imply the existence of cyclones, but a map of cyclone frequency would serve the purpose more readily.

It is evident that nothing short of a comprehensive atlas containing hundreds (or more likely thousands) of charts could present nearly all of the quantitative statistics of possible importance. We shall therefore limit our quantitative presentation to a few statistics which will have a special bearing on the remainder of this work, recognizing that these may not be the statistics of greatest interest to one who is pursuing a slightly different problem. We shall follow this account with a qualitative description of some of the remaining features of special significance.

It will be convenient to classify the principal features of the circulation into four categories, as follows:

(1) *Features which appear when the variables are averaged with respect to time and longitude.* These are typified by the familiar trade winds. A time average may mean an average over all time, or over all years at a particular time of the year. Some writers prefer to restrict the term "general circulation" to features in this category, and it is these features which, directly or indirectly, will receive the major attention in this monograph.

(2) *Features in addition to those of the first category which appear when the variables are averaged with respect to time alone.* These are typified by the Asiatic summer and winter monsoons. Most writers include these as features of the general circulation.

(3) *Features in addition to those of the first category which appear when the variables are averaged with respect to longitude alone.* These are typified by the familiar fluctuations of the zonal index. Studies of these features are ordinarily regarded as general-circulation studies by those engaged in them.

(4) *Features in addition to those of the first three categories which appear when the variables are not averaged.* These are typified by migratory cyclones. Many of these features are ordinarily regarded as secondary circulations; some of their over-all statistical properties are frequently considered to be characteristics of the general circulation.

Following the modified notation of Starr and White (1954), we shall let a bar (—) denote the time average of any quantity, and a prime (′) the departure of a quantity from its time average. Likewise we shall let brackets ([ ]) denote the average of any quantity with respect to longitude, and a star (*) the departure of a quantity from its longitudinal average. It is evident that the operators —, ′, [ ] and * are commutative.

The wind field $\mathbf{U}$ may now be resolved according to the formulae

$$\mathbf{U} = \overline{\mathbf{U}} + \mathbf{U}' \tag{78}$$

$$\mathbf{U} = [\mathbf{U}] + \mathbf{U}^* \tag{79}$$

and thus, in greater detail,

$$\mathbf{U} = [\overline{\mathbf{U}}] + \overline{\mathbf{U}}^* + [\mathbf{U}]' + \mathbf{U}^{*\prime} \tag{80}$$

Resolutions of the fields of temperature T, specific humidity q, and other quantities may be similarly performed.

There remains some ambiguity in the definitions of the averages, which must be removed. An average over time t may be an average for fixed values of λ, φ, z, or for fixed values of λ, φ, p, or for some other choice of independent variables. One type of averaging is not identical with another. Since our data consist mainly of observations at standard pressure levels, we shall let $\overline{\mathbf{U}}$ represent the time average for fixed λ, φ, p. Likewise, $[\mathbf{U}]$ will be the average with respect to longitude λ for fixed t, φ, p, i. e. the average along a latitude circle on an instantaneous isobaric surface.

Strictly speaking the features of $\mathbf{U}$ in the four categories previously enumerated are respectively those features appearing in the field of $[\overline{\mathbf{U}}]$, in $\overline{\mathbf{U}}$ but not $[\overline{\mathbf{U}}]$, in $[\mathbf{U}]$ but not $[\overline{\mathbf{U}}]$, and in $\mathbf{U}$ but neither $\overline{\mathbf{U}}'$ nor $[\mathbf{U}]$. To a considerable extent these are the features appearing respectively in the fields of $[\overline{\mathbf{U}}]$, $\overline{\mathbf{U}}^*$, $[\mathbf{U}]'$ and $\mathbf{U}^{*\prime}$. Similar remarks apply to the features of T and q. Some features, such as the jet stream, do not clearly fall into any one category.

Although the notation used in (78)-(80) has been adopted by a number of writers, there seems to be less uniformity in the accompanying terminology. We shall refer to the fields of $\overline{\mathbf{U}}$ and $\mathbf{U}'$ in (78) as the long-term or time-averaged or standing motion and the transient motion. We shall refer to the components $[u]$ and $[v]$ of $[\mathbf{U}]$ in (79) as the zonal circulation and the meridional circulation, and to the components of $\mathbf{U}^*$ as the eddies. We shall also include the field of $[\omega]$ demanded by continuity as part of the meridional circulation. Thus the terms in (80) become respectively the time-averaged or standing zonal and meridional circulations, the time-averaged or standing eddies, the transient zonal and meridional circulations, and the transient eddies.

The customary use of the terms "zonal" and "meridional" has led to some ambiguity. A "zone" generally means a latitude circle or a region extending along a latitude circle. "Zonal motion" generally means motion *parallel* to the zones, and is synonymous with u, while "meridional motion" means motions parallel to the meridians or meridional planes, and may be synonymous with v, or with v and ω. A "zonal average" generally means an average *within* zones, or with respect to longitude. "Zonal symmetry" generally denotes invariability within zones. We shall adhere to this usage.

The ambiguity arises in connection with the term "zonal circulation", which is sometimes used to mean the zonal motion u, sometimes the zonally-averaged motion $[\mathbf{U}]$, and sometimes zonally-averaged zonal motion $[u]$. We shall use the term only in the last sense. Likewise we shall use "meridional circulation" only to denote zonally-averaged meridional motion. The term "mean meridional circulation" has been used for the latter purpose, but it has also been used for the time-averaged meridional circulation. The frequently used term "mean motion" is not specific enough when both time averages and zonal averages are being considered.

The long-term zonally averaged circulation

Within this chapter we shall limit the quantitative statistics to the fields of $[\overline{\mathbf{U}}]$, $[\overline{T}]$ and $[\overline{q}]$. We do not imply by this limitation that the long-term zonally averaged fields are the only ones of importance. It does appear that these fields have received the greatest amount of theoretical attention. We shall presently see that a proper explanation of them also involves the transient motions and the eddies.

We consider first the distribution of $[\bar{u}]$, the long-term zonal circulation. In view of the copious data which is continually accumulating in ever greater amounts, it might seem that this field should be rather precisely known by now. This does not appear to be the case.

The logistics of weather data are rather intricate. The fact that an observation has been performed in the prescribed manner is no assurance that it will find its way into any particular data collection; it is even less certain that it will arrive without errors. Those who make immediate use of the data — the weather forecasters — are generally not the ones who are charged with storing them for possible future use. So many data are now stored in various collections that the mere process of extracting the portion needed for a particular study is a formidable task. The use of large digital computing machines has made it possible to handle sets of data which would otherwise be completely unwieldy, but it has also made it easy to overlook the type of error which would have been immediately evident in the days when the processing was done by hand. One or two wind speeds recorded on punched cards or magnetic tape as 500 m sec^{-1} instead of 50 m sec^{-1}, for example, will render a computed statistic quite worthless.

In any event, there appear to be no estimates of $[\bar{u}]$ based upon a major portion of the upper-level wind observations which have been collected since World War II. A number of studies have been based upon smaller portions of the data.

Figures 1 and 2 present the distribution of $[\bar{u}]$ for northern winter and southern summer (October-March) and southern winter and northern summer (April-September), as computed in separate studies by Buch (1954) and Obasi (1963). The data samples for these studies were somewhat limited. Buch used northern-hemisphere data for 1950 only, while Obasi used southern-hemisphere data for 1958 only. However, the studies are distinguished in that they are based entirely on wind observations, whereas the more extensive studies thus far completed have depended partly upon winds estimated from pressure observations. Moreover, Buch and Obasi used the same computational procedure, and the number of stations (145) available in the southern hemisphere in 1958 was comparable to the number (81) available in the northern hemisphere in 1950.

The method of computation consisted of determining the time average $\bar{u}$ at the 850-, 700-, 500-, 300-, 200- and 100-mb levels at each station, using all available data. These averages were then entered on hemispheric maps, and isopleths were drawn. From the isopleths, values of $\bar{u}$ at the intersections of standard parallels and meridians were recorded, and these values were averaged to obtain estimates of $[\bar{u}]$.

The most complete compilation of northern-hemisphere winds seems to be that of Crutcher (1959, 1961), who has prepared horizontal maps of $\bar{u}$ and other statistics at the 850- ,800-, 500-, 300-, 200- and 100-mb levels, and vertical cross-sections of these statistics for each ten degrees of longitude. It is a simple matter to average Crutcher's values; the resulting values of $[\bar{u}]$ are shown for winter (December-February) and summer (June-August) in Figures 3 and 4.

Crutcher's values are based upon at least five years of data in most regions, and in this respect are superior to Buch's. We prefer, however, not to combine Crutcher's values and Obasi's in the diagrams, since they were computed by different procedures and probably do not afford a reliable comparison of the hemispheres. In particular, in regions where observed-wind data were scarce, Crutcher used winds which had been estimated by the gradient-wind formula from constant-pressure charts, although he avoided the simpler geostrophic-wind formula.

Another thorough study covering both hemispheres is the one by Heastie and Stephenson (1958), also based upon five years of data. Cross-sections for January and July appear in Figures 5 and 6. Here no attempt has been made to utilize observed winds except in the tropics. North of 25°N and south of 25°S the winds are geostrophically estimated from the contours on constant pressure charts.

Finally, we mention a detailed and frequently quoted study by Mintz (1954), based essentially upon all available appropriate data prior to 1950. Again, the winds are geostrophically estimated, except between 20°N and 20°S. Except near Antarctica, Mintz's southern-hemisphere data are restricted to the longitudes of Australia and New Zealand. We therefore present only his northern hemisphere values of $[\bar{u}]$; these are shown in Figures 7 and 8.

Returning to Figure 1, we note that Buch finds a winter westerly wind maximum of 23 m sec^{-1} slightly below the 200-mb level at about 35°N. In Figure 3, however, the maximum has increased to 34 m sec^{-1}, and it is found slightly farther south. In Figure 5, it is similarly located, but it has attained a strength of 37 m sec^{-1}. Finally, in Figure 7, Mintz locates the maximum south of 30°N, with a strength of 42 m sec^{-1}. In view of this great diversity of estimates, we find it difficult to maintain that the average zonal westerly wind field is quantitatively known.

Some of the disagreement among the estimates can be easily explained. Buch found it necessary to combine the six months October-March for his winter computations, to obtain a reasonably large sample. Crutcher and Mintz, having more years of data at their disposal, used December-February, when the winds are stronger than in October and November, while Heastie and Stephenson used January alone.

Perhaps equally important is the bias toward light winds which is ordinarily present in observed-wind data. One of the principal causes of missing upper-level reports is the occurrence of excessively strong winds, which carry the balloon beyond the range of the receiving instrument before the highest elevations are reached. Virtually all collections of upper-level wind data, but particularly the less recent ones, are therefore biased in favour of light winds. Geostrophically estimated winds suffer only slightly from this bias for, although strong winds will cause a pressure observation as well as a wind observation to be missing, it is the pressure on either side of a strong current rather than the pressure within the current which is used in estimating the strength of the current.

Moreover, even without missing data the geostrophic approximation systematically overestimates the winds in the zones of strong westerlies. In the free atmosphere a wind equal to the geostrophic wind would have no horizontal acceleration, and would therefore tend to follow a great-circle trajectory. But, on the average, trajectories in the zone of westerlies are curved at least as greatly as the latitude circles; the average winds are therefore subgeostrophic. From (45), (47) and (48) we find upon averaging that

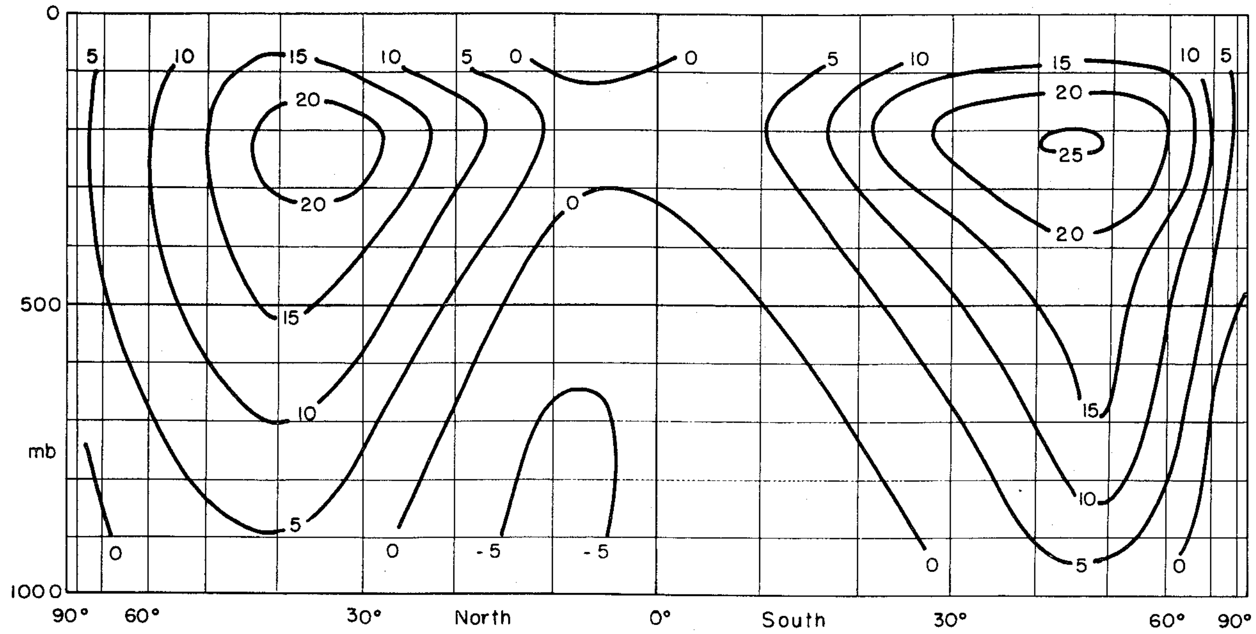


Figure 1. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in northern winter and southern summer (October-March) as estimated by Buch (1954) and Obasi (1963). Values are in m sec^{-1}

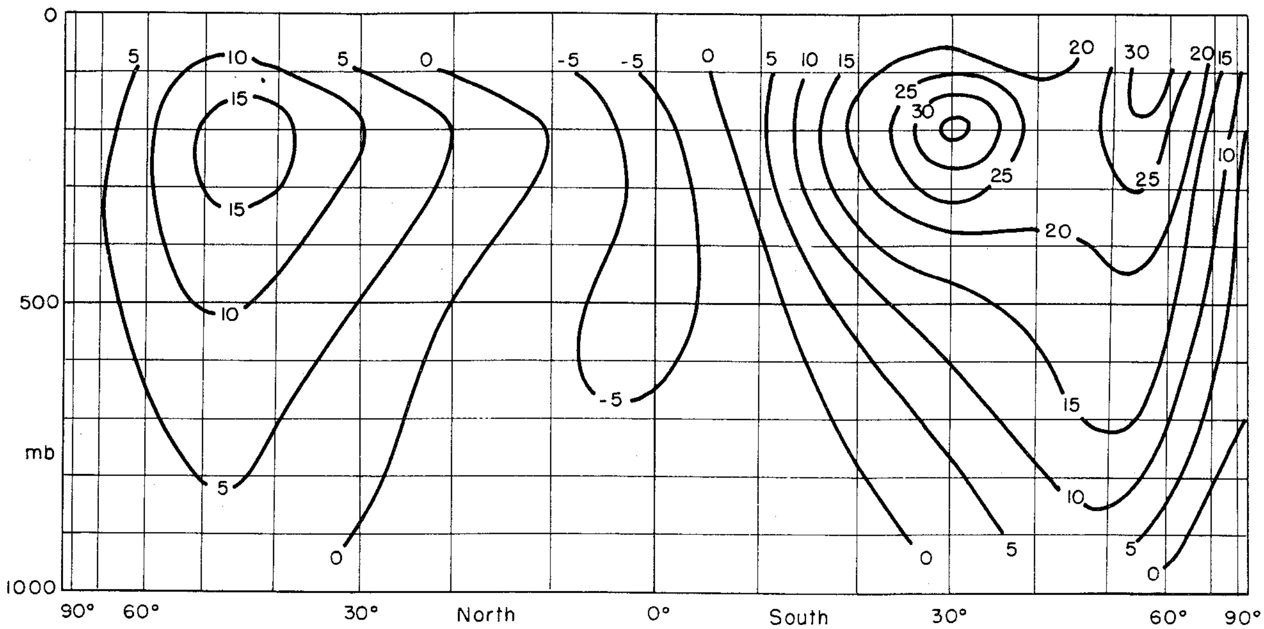
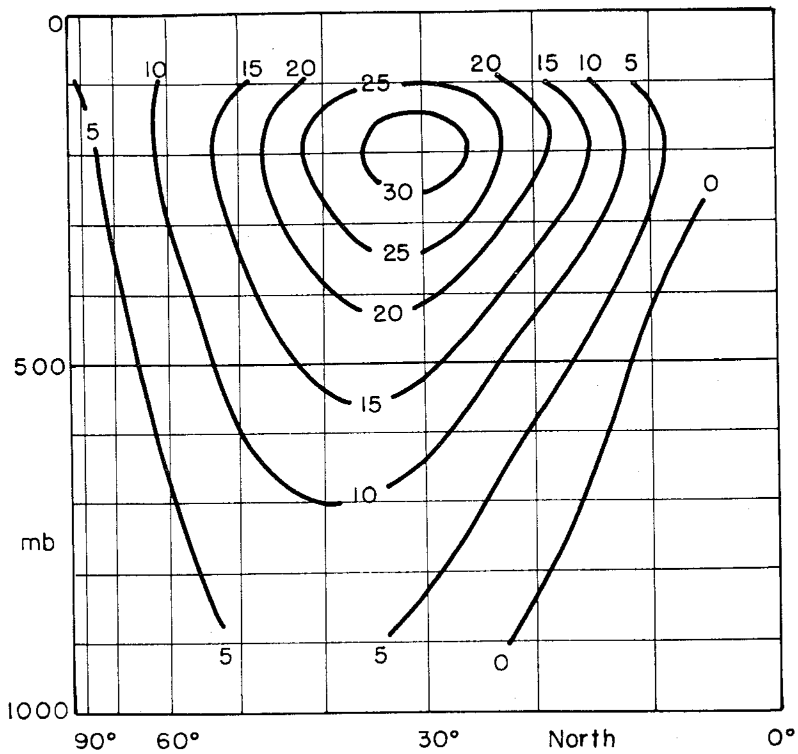


Figure 2. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in northern summer and southern winter (April-September) as estimated by Buch (1954) and Obasi (1963). Values are in m sec^{-1}

Figure 3. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in northern winter (December-February) as estimated from charts compiled by Crutcher (1959, 1961). Values are in m sec^{-1}



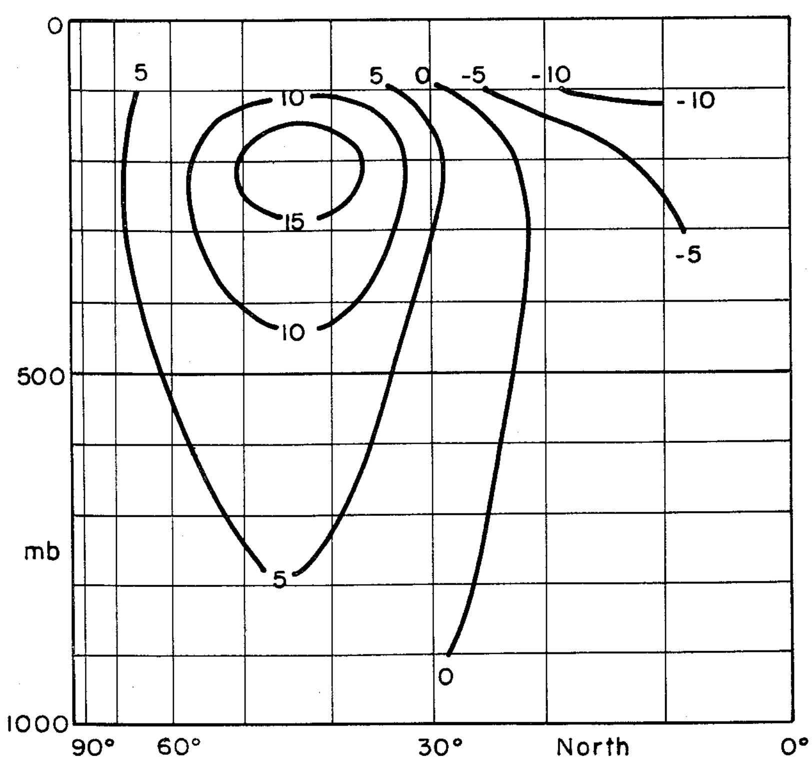


Figure 4. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in northern summer (June-August) as estimated from charts compiled by Crutcher (1959, 1961). Values are in m sec^{-1}

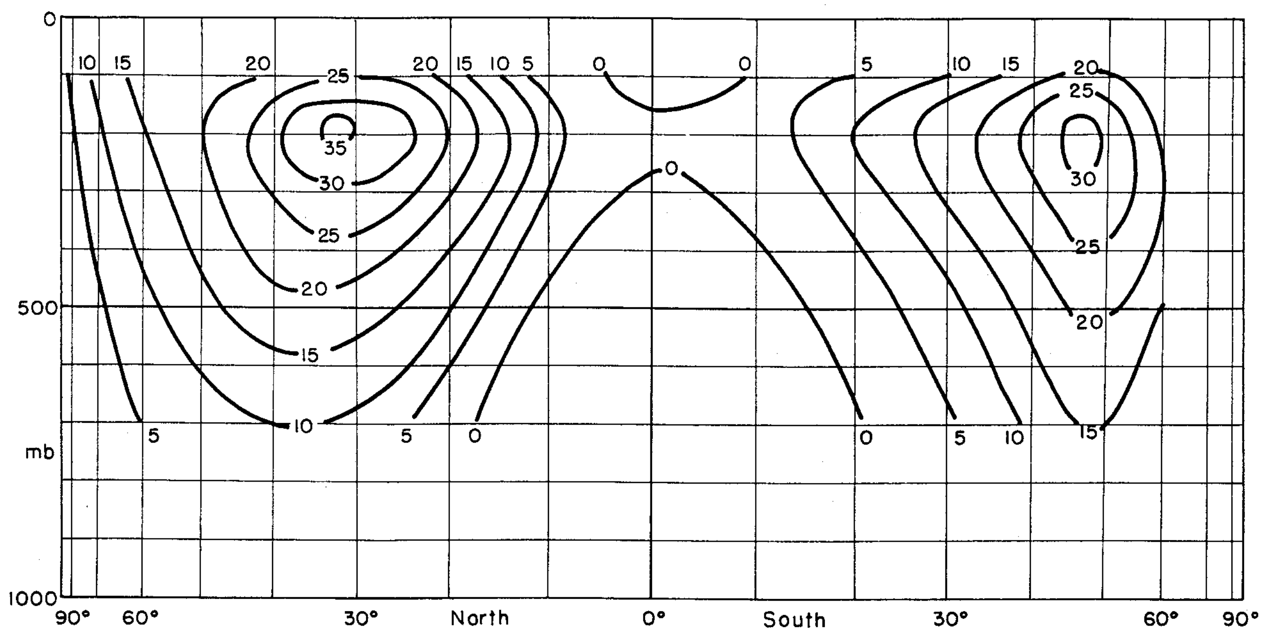


Figure 5. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in January as estimated by Heastie and Stephenson (1958). Values are in m sec^{-1}

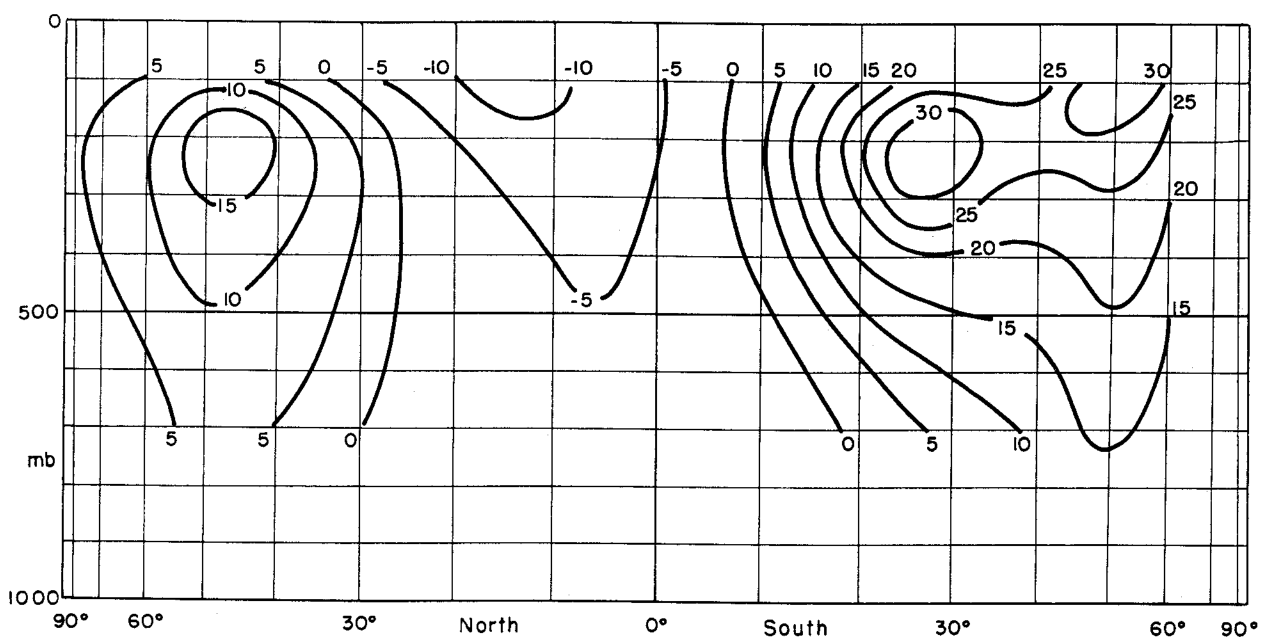
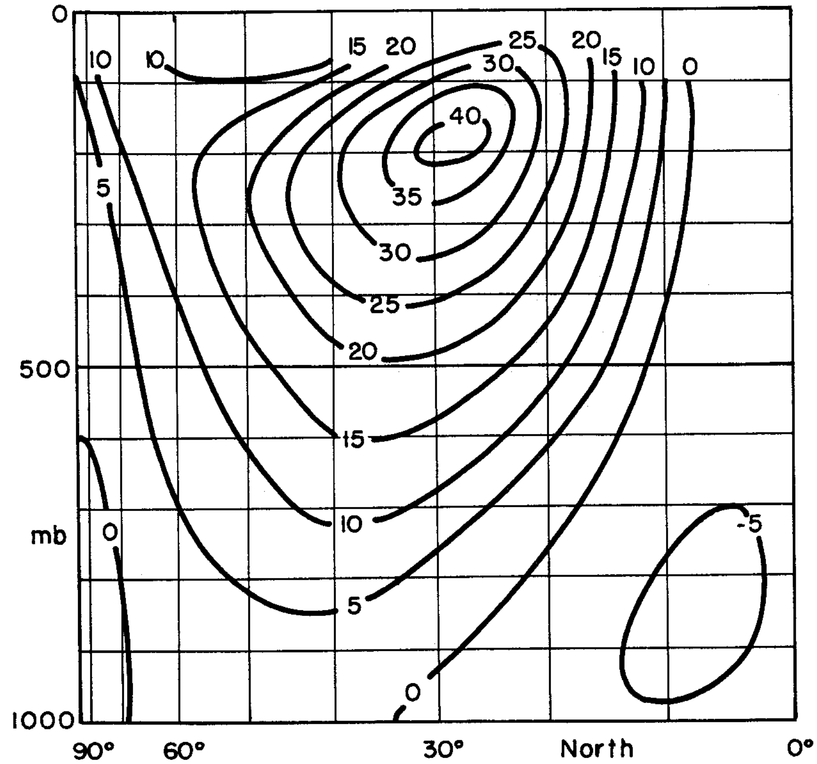


Figure 6. — The time-and-longitude averaged zonal wind $[\bar{u}]$ in July as estimated by Heastie and Stephenson (1958). Values are in m sec^{-1}

Figure 7. — The time-and-longitude averaged zonal wind $[\overline{u}]$ in northern winter (December-February) as estimated by Mintz (1954). Values are in m sec^{-1}



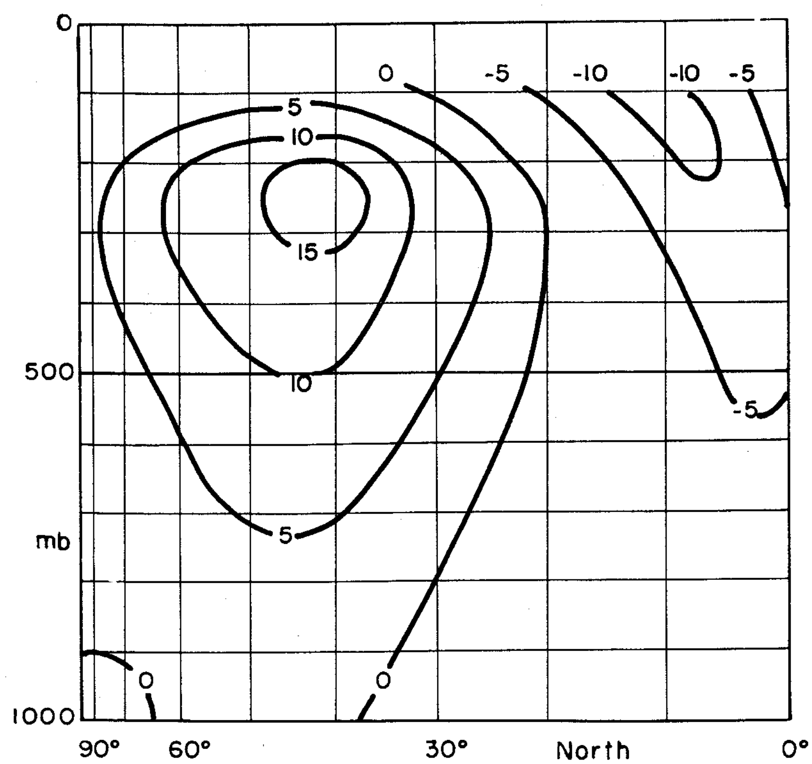


Figure 8. — The time-and-longitude averaged zonal wind $[\overline{u}]$ in northern summer (June-August) as estimated by Mintz (1954). Values are in m sec^{-1}

$$f\,[\bar{u}_g] = f\,[\bar{u}] + \frac{\tan\varphi}{a}\,[\overline{u^2}] + \frac{1}{a\cos\varphi}\frac{\partial}{\partial\varphi}\cos\varphi\,[\overline{v^2}] + \frac{\partial}{\partial p}\,[\overline{v\omega}] - [\bar{F}_\varphi] \tag{81}$$

The final term in (81) is presumably small, and the preceding term is difficult to estimate, but since it disappears in the vertical average it cannot be of one sign everywhere. The other terms depend upon readily estimated statistics. Crutcher's charts include the distributions of $\bar{v}$ and the standard deviations of u and v, from which $[\overline{u^2}]$ and $[\overline{v^2}]$ may be evaluated. Holopainen (1966) has evaluated the annual average nongeostrophic zonal wind $[\bar{u}] - [\bar{u}_g]$; it is shown in Figure 9. For the winter alone, $[\bar{u}_g]$ exceeds $[u]$ by 2.3 m sec^{-1} at 200 mb and 30°N, and the disparity between Figure 3 and 5 is thus nearly accounted for.

Yet, lest we be too hasty in maintaining that the three influences just mentioned completely explain the discrepancies, let us note that they are equally present in the southern hemisphere. Obasi's winter maximum of $[\bar{u}]$, based on observed winds, ought therefore to fall far short of the maximum given by Heastie and Stephenson. Yet reference to Figures 2 and 6 reveals essentially no difference. It appears that some of the differences between the estimates must result simply from the finite sizes of the samples. Different samples inevitably possess different mean values.

Despite the quantitative differences just noted, the qualitative features of $[\bar{u}]$ seem to be fairly well defined. At the surface there are the familiar trade winds and prevailing westerlies, with weak easterlies again in the polar regions. The eastward wind component increases upward from the surface to about 200 mb everywhere, except in low latitudes in northern summer. From 200 mb to considerably above 100 mb it decreases everywhere, except in high latitudes in winter, where it continues to increase. In the southern hemisphere there is a double maximum in winter. The only significant disagreement is in the equatorial regions, where Mintz's data show easterlies at all levels, while the remaining studies show westerlies near 200 mb in winter.

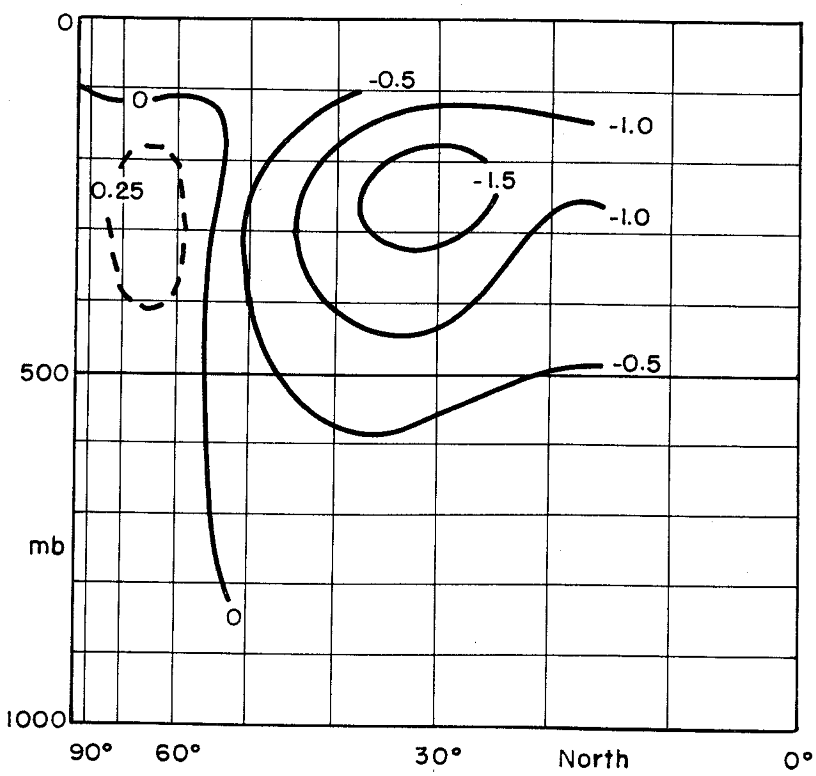


Figure 9. — The time-and-longitude averaged non-geostrophic zonal wind $[\bar{u} - \bar{u}_g]$ as estimated by Holopainen (1966). Values are in m sec^{-1}

Since the geostrophic wind affords a rather good estimate of $[\bar{u}]$ despite its tendency to overestimate the westerlies, many of the principal features just cited should have their counterparts in the field of $[\overline{T}]$. This proves to be the case. For uniformity, we present first the cross-sections of Peixoto (1960), supplemented by values at 50 mb and 30 mb obtained by Peng (1963, 1965), in Figures 10 and 11; these have been computed by a procedure similar to Buch's. Peixoto's data are again for the year 1950, and the network of stations is essentially the same; Peng's data are for 1958.

The cross-sections using the most complete compilation of data appear to be those of Palmén and Newton (1967). They are shown in Figures 12 and 13. They have been based largely upon the detailed maps of Goldie *et al.* (1958), which were constructed mainly from observations made during 1941-1952. Some aircraft measurements were used in regions where radiosonde data were scarce.

Unlike the estimates of $[\bar{u}]$, the estimates of $[\overline{T}]$ are in good quantitative agreement. The somewhat higher temperatures during the winter and lower temperatures during the summer obtained by Peixoto presumably occur because Peixoto's winter and summer were actually the six-month periods October-March and April-September, while Palmén and Newton presented data for January and July. We might add that no investigator has seen fit to estimate the temperature field geostrophically from wind observations.

The one feature of the temperature field which possesses no geostrophic counterpart in the wind field is its vertical variation. Here the principal feature is the separation of the atmosphere into the troposphere, where — except at low elevations in the polar regions in winter — the temperature decreases with elevation, and above it the stratosphere, where — again except in the polar regions in winter — the temperature no longer decreases. The tropopause separating these regions is not sharply defined in the averaged temperature fields, and will be mentioned later.

The increase of $[\bar{u}]$ with height in the troposphere is the geostrophic equivalent of the poleward decrease of $[\overline{T}]$. The decrease of $[\bar{u}]$ in the stratosphere is the equivalent of the poleward increase of $[\overline{T}]$ there. In the polar regions in midwinter the temperature decreases poleward even in the stratosphere, and the westerly wind speed increases with elevation. Near the Equator, where the geostrophic equation is less dependable, no important horizontal variations of $[\overline{T}]$ are revealed.

Peixoto and Crisi (1965) have also made estimates of the zonally averaged specific humidity $[\bar{q}]$, using northern hemisphere data for 1958. Data became much more plentiful between 1950 and 1958, and 345 stations were available. Their computational procedure was again similar to the one used by Buch. Their cross-sections are shown in Figures 14 and 15.

At the surface in tropical latitudes $[\bar{q}]$ is very high; in fact it is higher than the values which would prevail at temperatures a few degrees lower under saturated conditions. Thus $[\bar{q}]$ falls off with increasing latitude and elevation, and to a first approximation is determined by the field of $[\overline{T}]$.

Indeed, the variations of q follow those of the saturation specific humidity q_s so closely that some of the interesting aspects of the field of moisture are more readily seen in the field of zonally averaged relative humidity $[\overline{q/q_s}]$. Figures 16 and 17 present tropospheric estimates by London (1957). The outstanding feature is the dry region in the subtropics at middle levels in both winter and summer.

Other estimates (see the discussion by Manabe *et al.* 1965, p. 776) have indicated much lower relative humidities in the upper troposphere. Above 500 mb all estimates seem to be based upon rather limited data.

We finally consider the long-term meridional circulation $[\bar{v}]$ and the field of $[\bar{\omega}]$ related to it through continuity. Unlike $[\bar{u}]$, which often affords a moderately good approximation to instantaneous values of u,

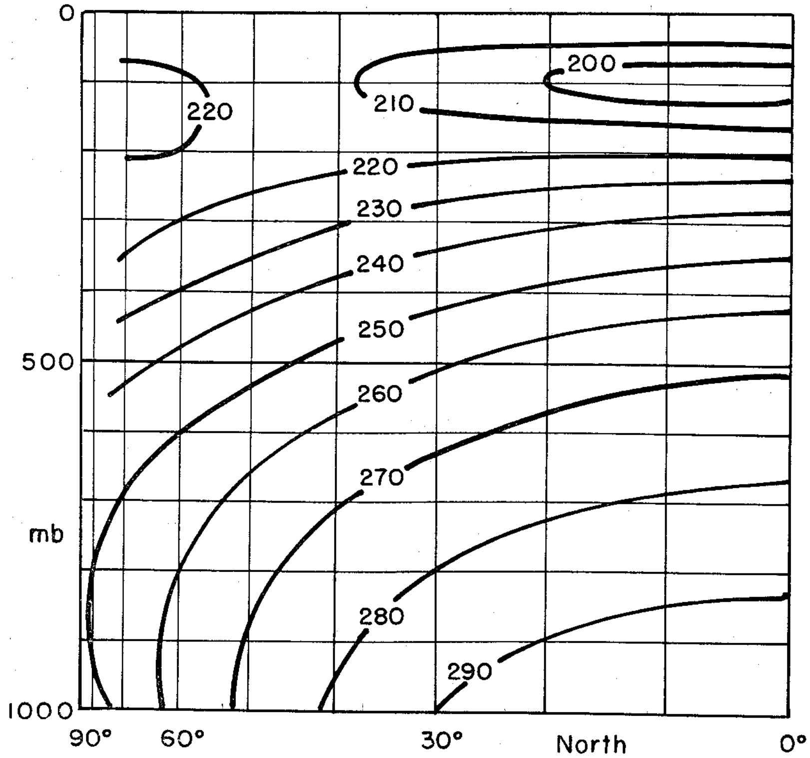


Figure 10. — The time-and-longitude averaged temperature $[\overline{T}]$ in northern winter (October-March) as estimated by Peixoto (1960) (1000 mb-100 mb) and Peng (1963, 1965) (100 mb -30mb). Values are in degrees K

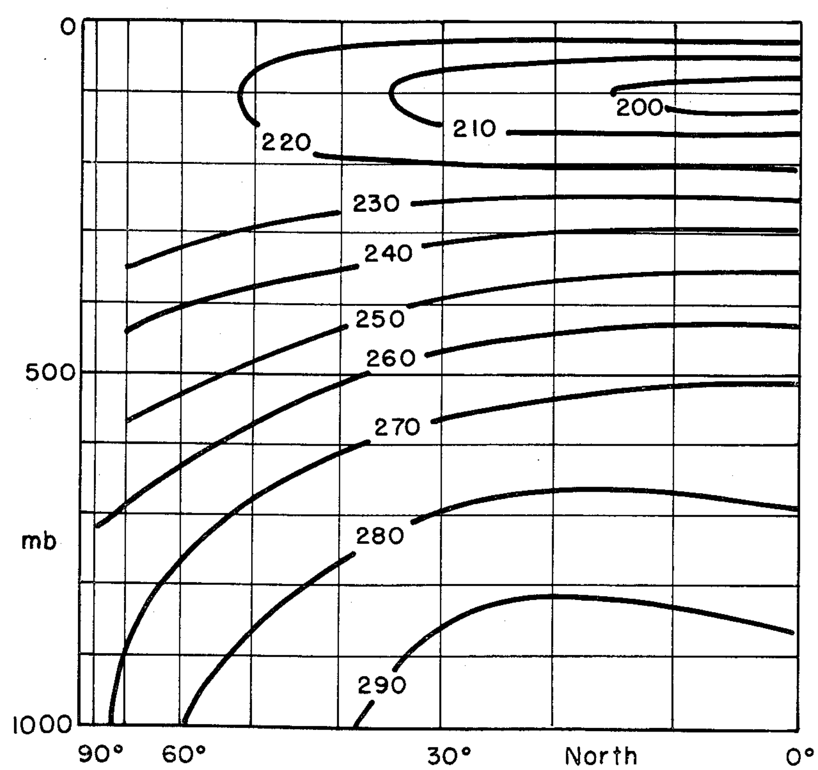


Figure 11. — The time-and-longitude averaged temperature $[\overline{T}]$ in northern summer (April-September) as estimated by Peixoto (1960) 1000 mb-100 mb) and Peng (1963, 1965) (100 mb-30 mb). Values are in degrees K

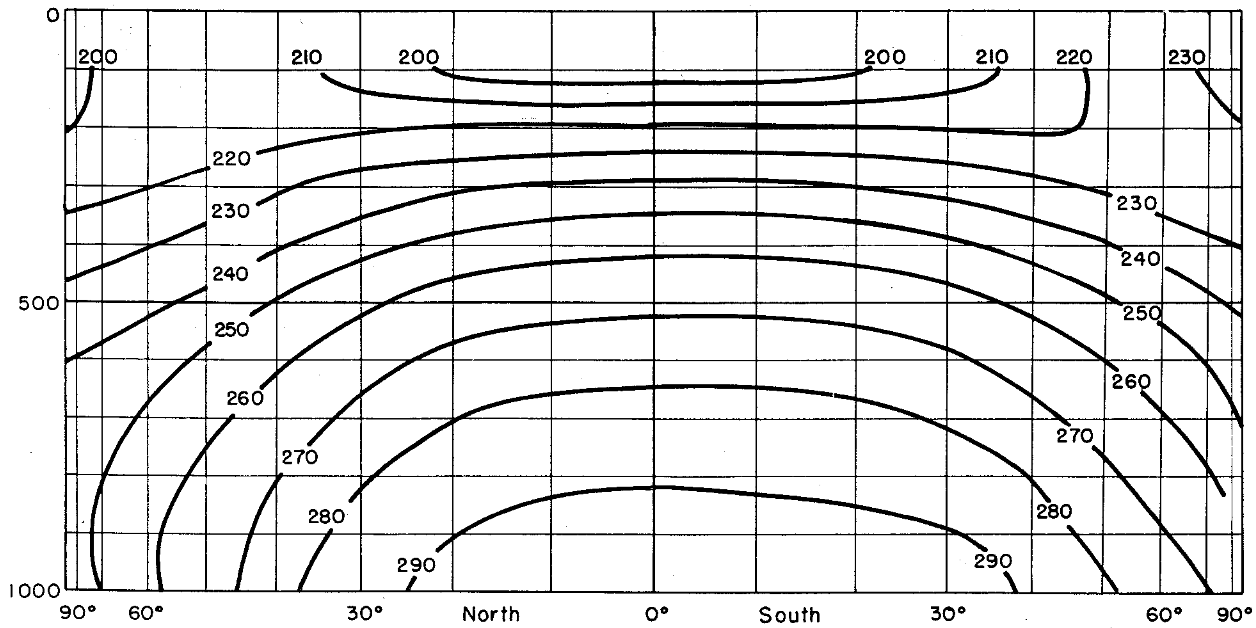


Figure 12. — The time-and-longitude averaged temperature $[\overline{T}]$ in January as estimated by Palmén and Newton (1967). Values are in degrees K

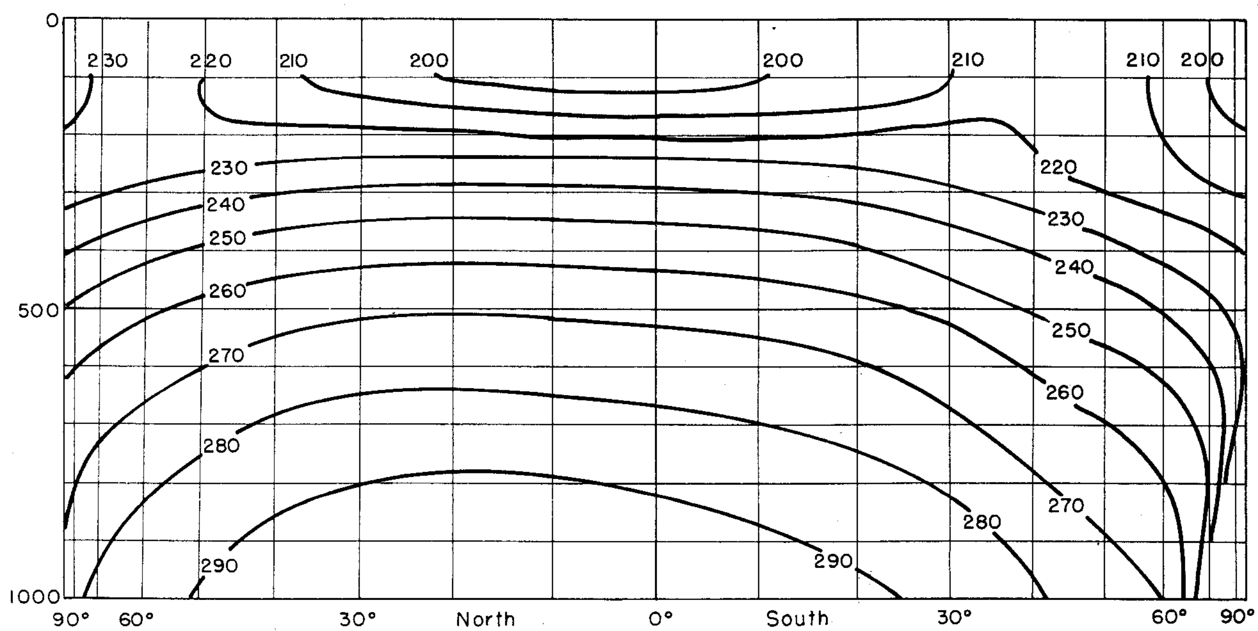


Figure 13. — The time-and-longitude averaged temperature $[\overline{T}]$ in July as estimated by Palmén and Newton (1967). Values are in degrees K

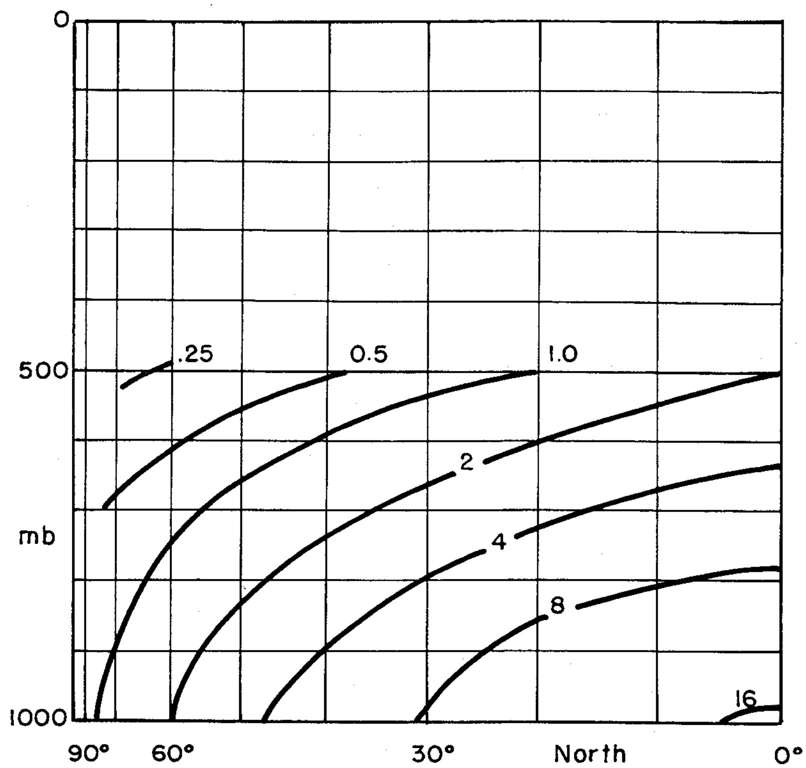


Figure 14. — The time-and-longitude averaged specific humidity $[\overline{q}]$ in northern winter (October-March) as estimated by Peixoto and Crisi (1965). Values are in thousandths

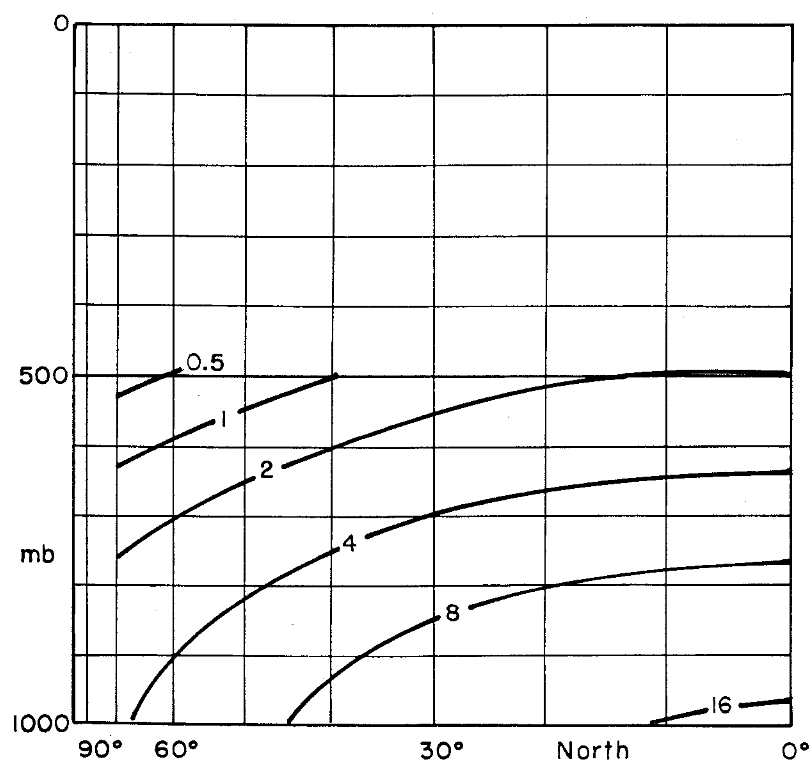
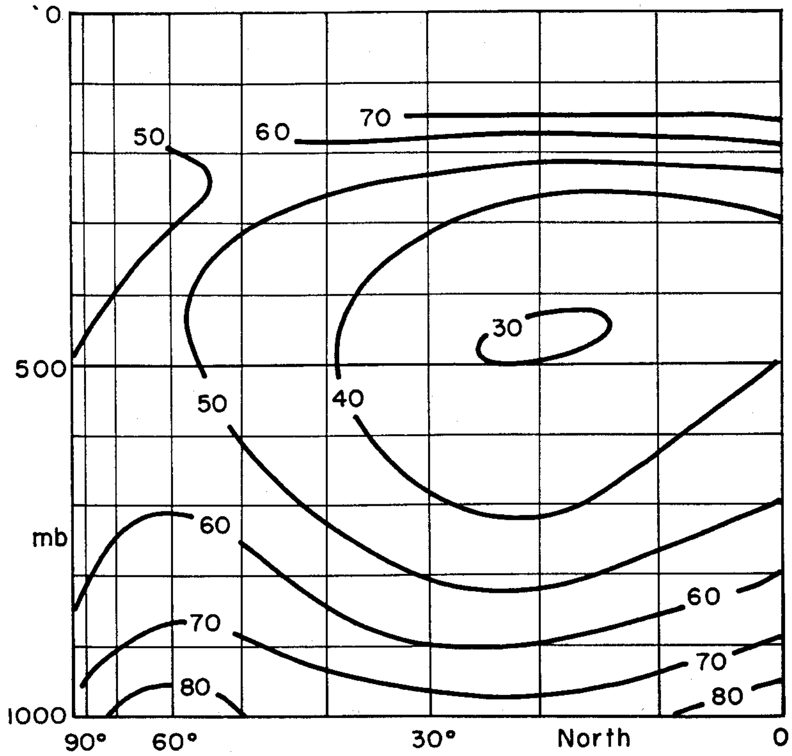


Figure 15. — The time-and-longitude averaged specific humidity $[\overline{q}]$ in northern summer (April-September) as estimated by Peixoto and Crisi (1965). Values are in thousandths

Figure 16. — The time-and-longitude averaged relative humidity $[\overline{q/q_s}]$ in northern winter as estimated by London (1957). Values are in per cent



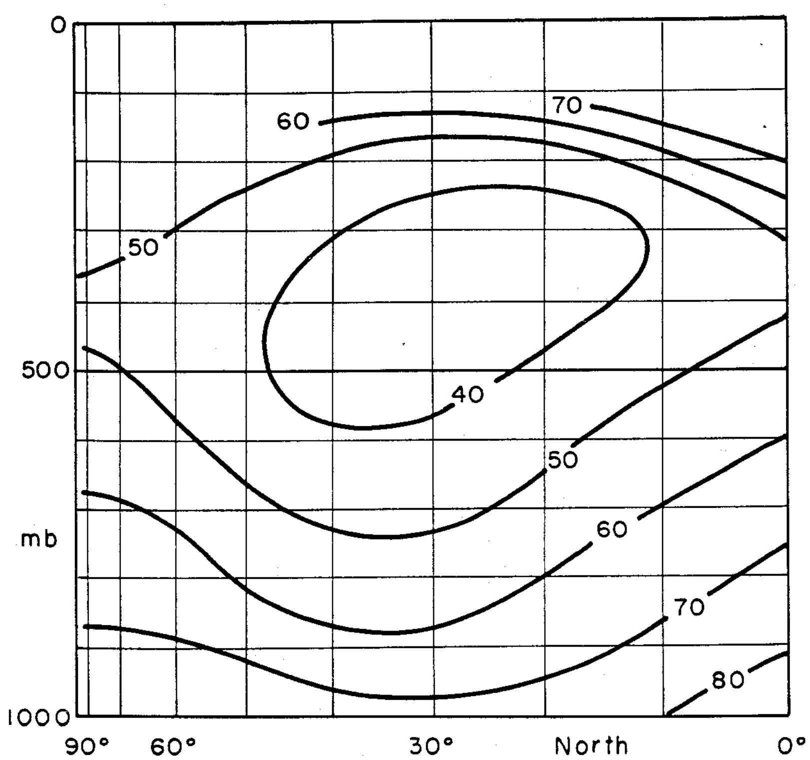


Figure 17. — The time-and-longitude averaged relative humidity $[\overline{q/q_s}]$ in northern summer as estimated by London (1957). Values are in per cent

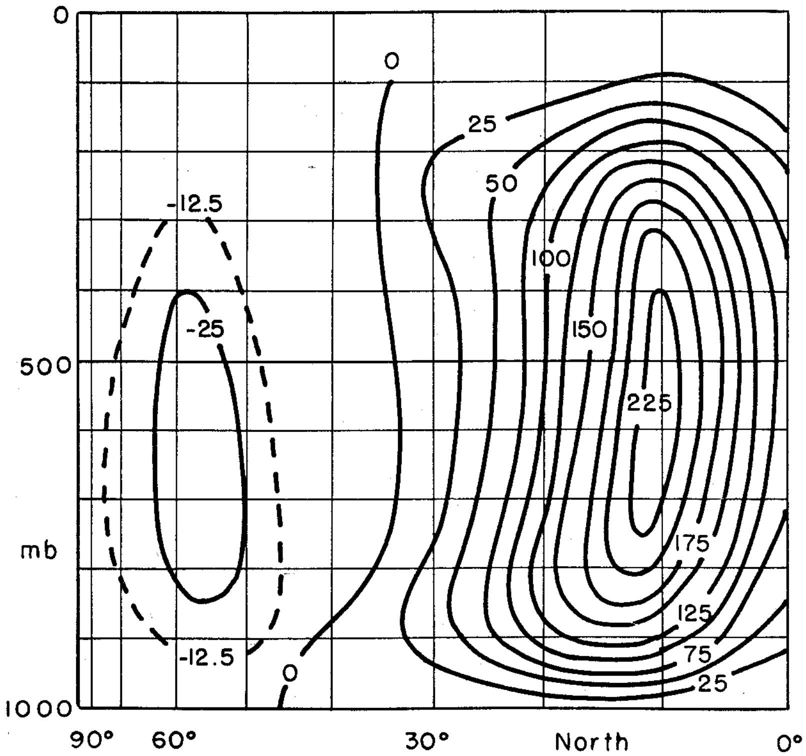


Figure 18. — The time-averaged meridional circulation in northern winter as estimated by Palmén and Vuorela (1963). The unit for stream function $\overline{\Psi}$ is 10^{12} g sec^{-1}

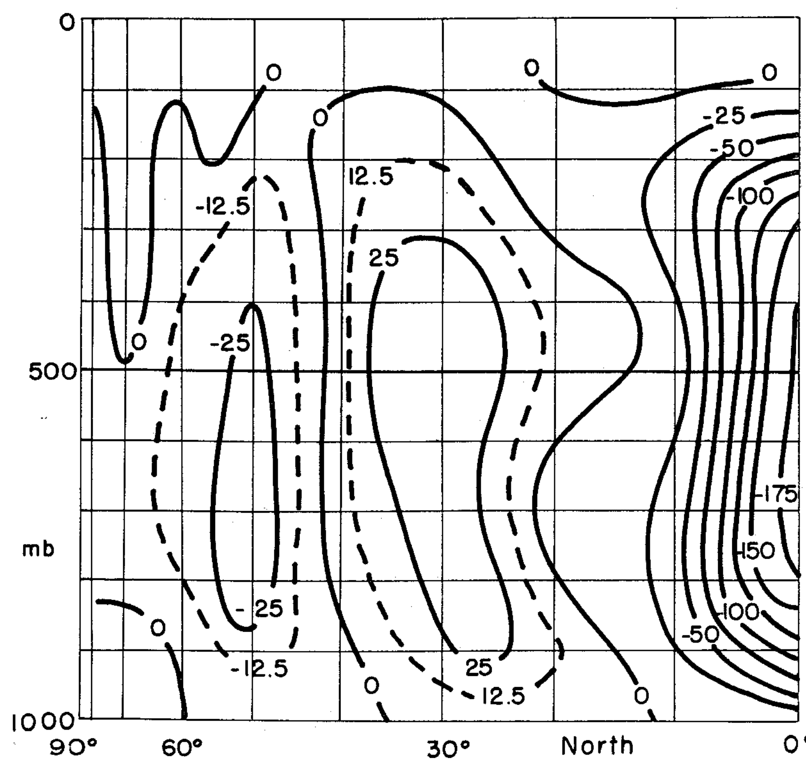


Figure 19. — The time-averaged meridional circulation in northern summer as estimated by Vuorela and Tuominen (1964). The unit for stream function $\overline{\Psi}$ is 10^{12} g sec^{-1}

$[\bar{v}]$ is essentially a statistical residual which results from averaging large values of v having opposite signs. An exception occurs within the trade winds, where the equatorward component is fairly persistent. Hence far less confidence can in general be placed in estimates of $[\bar{v}]$ than in those of $[\bar{u}]$, which themselves are somewhat uncertain. In Figures 18 and 19 we present recent estimates of the time-averaged meridional circulation by Palmén and Vuorela (1963) for winter, and by Vuorela and Tuominen (1964) for summer.

The lines shown are streamlines of mass flow. On the basis of equations (42) and (48) it is possible to introduce a "stream function" Ψ for total mass flow such that

$$2\pi\ a \cos\varphi\ [v] = g\,\partial\,\Psi\,/\,\partial p \tag{82}$$

$$2\pi\ a^2 \cos\varphi\ [w] = -\ g\,\partial\,\Psi\,/\,\partial\varphi\,. \tag{83}$$

With $\Psi = 0$ along the Earth's surface, the values of $\bar{\Psi}$ aloft can be determined from observed values of $[\bar{v}]$.

A meridional circulation ordinarily consists of one or more meridional cells, each cell being a circulation about an extreme value of Ψ. A cell in which the warmer air rises and the colder air sinks is called thermally direct; if the warmer air sinks and the colder air rises, it is thermally indirect. The circulation envisioned by Hadley contains a single direct cell in each hemisphere. As we shall show in Chapter V, in a thermally forced circulation a direct cell converts the energy introduced by the heating into kinetic energy; an indirect cell has the opposite effect, and must depend upon the remainder of the circulation for its kinetic energy.

Figure 18 shows a strong direct cell in low latitudes. Such a cell is now called a "Hadley cell". In higher latitudes there is a weaker indirect cell. In northern summer the southern-hemisphere Hadley cell extends well into the northern hemisphere. The equatorward flow in the Hadley cell is confined mainly to the surface friction layer, while the return flow, rather than being uniformly spread through the remainder of the atmosphere, is concentrated near the tropopause.

Needless to say, various estimates of $[\bar{v}]$ and $[\bar{\omega}]$ differ considerably. Tucker (1959), for example, finds a less intense Hadley cell (in winter), a well developed middle-latitude indirect cell, and good indications of another direct cell in the polar regions. The long-term meridional circulation can also be estimated by various indirect methods. We do not regard these estimates as part of the "observed circulation", and we shall delay their description until the following chapter.

The eddies and the transient motions

As noted, a reasonably complete presentation of the features in the remaining three categories previously mentioned would require an inconveniently large collection of charts. We shall merely describe some of the main qualitative features. The quantitative influence of these features upon the fields of $[\overline{\mathbf{U}}]$, $[\overline{T}]$, and $[\bar{q}]$ will be considered in the following chapter.

Features of the second category serve to distinguish the climate of one locality from that of another in the same latitude, and have received much attention from climatologists. They would not be present in an idealized atmosphere without geographical features to distinguish one longitude from another. As a result, they have been disregarded altogether in many theoretical studies of a fluid dynamical nature, while in other studies simple idealized oceans and continents have been introduced.

In general there is a tendency toward high sea-level pressure with anticyclonic circulation over the continents and low sea-level pressure with cyclonic circulation over the oceans in winter, except at very low latitudes. The opposite situation tends to prevail in summer, except at rather high latitudes. This tendency is most clearly revealed by the Asiatic winter and summer monsoons — the intense winter anticyclone centred over northern Asia and the equally great summer cyclone centred over southern Asia. The great Icelandic and Aleutian cyclones in winter also fit this pattern. In the southern hemisphere, where the continents are smaller, the tendency is present but less pronounced.

The temperature tends to be low over the continents and high over the oceans in winter, while the opposite tendency prevails in summer. Thus, in agreement with the thermal wind relation, the monsoons decrease in intensity with elevation. The temperature field is also influenced by the circulation itself, and the Icelandic and Aleutian cyclones are colder on their western sides, whence, again in agreement with the thermal wind equation, they are displaced westward with elevation. In the middle and upper troposphere there are two principal troughs in the westerly flow, located near the east coasts of North America and Asia. In the southern hemisphere the time-averaged flow is more nearly parallel to the latitude circles.

It might be supposed that features of the third category, like those of the second, would be absent in an idealized atmosphere, but numerical solutions reveal that pronounced time variations of $[u]$ and $[T]$ may occur, even though longitudinal variation of $\bar{u}$ and $\overline{T}$ do not occur. The distinction is that the time interval over which the averaging is performed is infinite, while the space interval is limited by the circumference of the Earth. In an idealized atmosphere circulating above a plane of infinite west-east extent, $[u]$ and $[T]$ would presumably not vary with time, while $\bar{u}$ and $\overline{T}$ would vary with longitude if the averages were taken over finite time intervals.

In the real atmosphere the best-known feature in the third category is probably the continual irregular oscillation between high-index and low-index patterns. The index of the zonal westerlies, or simply the zonal index, was originally defined by Rossby (1939) as the average geostrophic eastward wind component at sea-level between 35°N and 55°N. A typical low-index situation is however regarded as one where the westerly winds are not only weaker than average but are also displaced farther toward the Equator, while the northward and southward motions are unusually well developed. The opposite conditions characterize a typical high-index situation. Successive maxima or minima of the zonal index tend to occur at intervals of from two weeks to two months, but the intervals are not uniform enough for the index to show any periodicity. The zonal index serves as a fundamental quantity in some systems of extended-range forecasting.

Irregular fluctuations also characterize the easterlies. The trade winds are virtually always present, and vary only in intensity, but the polar easterlies are often replaced by westerlies. Indeed, there is usually neither an anticyclone nor a cyclone centred at the Pole, and a negative or positive value of $[u]$ near the pole is simply a measure of whether the vorticity is anticyclonic or cyclonic. As Mintz (1954) points out in his discussion of the long-term zonal circulation, the polar easterlies in the northern hemisphere have even been absent in averages over entire seasons; in essence they are a statistical residual.

In marked contrast to the non-periodic fluctuations in middle latitudes are the variations in the middle stratosphere in equatorial latitudes. During the past ten years the zonal winds have been observed to alternate between strong easterlies and strong westerlies, with a period of slightly more than two years. Moreover, superposed variations of shorter period are rather minor. The variations of u at different longitudes appear to be in phase, so that $[u]$ exhibits similar variations. The oscillation is most intense at the 30-mb level, and the various phases of the oscillation occur later at lower elevations. This so-called 26-month or quasi-biennial oscillation is generally regarded as a separate phenomenon from the irregular fluctuations which predominate in most of the atmosphere, and it has been the subject of numerous studies (see Reed 1965). Five or six complete cycles, which are certainly not perfect duplications of one another, do not prove that the equatorial stratospheric zonal wind will continue to oscillate in this manner. Assuming, however, that it continues to oscillate as it has ever since it was first regularly observed, it can be predicted two years or more in advance with considerable accuracy. Certainly the zonal index cannot be predicted a whole cycle in advance, much less two years, on the basis of its own past behaviour alone (see Namias 1950).

At the end of the spectrum are the fluctuations of the circulation which presumably accompanied the changes between glacial and interglacial epochs. Despite the evidence for major temperature variations, it is difficult to reconstruct the accompanying variations in the field of motion. It has nevertheless been surmised that the changes are very much like those characterizing the changes of the zonal index, with a low-index type of circulation prevailing during periods of extensive glaciation (see Willett 1949).

Features in the fourth category include many of those entities most familiar to the synoptic meteorologist. The most obvious ones are the migratory anticyclones and cyclones, including tropical cyclones. At upper levels large troughs and ridges, which give a wave-like appearance to the westerly wind current, frequently occur in preference to closed cyclones and anticyclones. While individual cyclones and anticyclones and individual troughs and ridges are usually regarded as secondary circulation systems, their frequency of occurrence, average intensity, and average daily displacement as functions of geographical location are properly regarded as additional characteristics of the general circulation. Likewise the wave number, or the number of principal troughs or ridges intersecting a given latitude circle, is a general-circulation characteristic.

Fronts and frontal surfaces form another feature in the fourth category. While frontal passages at individual locations are usually regarded as rather small-scale phenomena, the entire polar front — the principal discontinuity between air masses of tropical and polar origin — is often looked upon as a feature of the general circulation, and it has played a prominent role in some theories. Because its position is continually oscillating, no corresponding discontinuities appear in the time-averaged or longitude-averaged fields of motion and temperature.

Similarly the intertropical convergence zone — the principal zone of convergence between low-level currents originating in the southern and northern hemispheres, generally extending most of the way around the globe near the Equator — is logically regarded as a general-circulation feature. Since its position also oscillates, it appears as a fairly broad zone of transition rather than a narrow zone in averaged fields of motion.

A further feature which is not clearly evident in averaged temperature fields because its position is continually oscillating is the tropopause — the surface separating the stratosphere from the troposphere. The upward temperature decrease which prevails in the troposphere ordinarily ceases rather abruptly at the tropopause, but in the fields of $\overline{T}$ or $[T]$ it ceases more gradually. To obtain an abrupt tropopause in the field of $[\overline{T}]$ one would have to perform the averaging in a new co-ordinate system in which elevation above the tropopause replaced absolute elevation as a vertical co-ordinate.

The presence of a narrow meandering jet stream in either hemisphere, or often several jet streams, is not clearly represented by any single term in equation (80). The zonal westerly wind maximum which is such a prominent feature in the field of $[\bar{u}]$ is sometimes identified with the jet, but it does not possess the full average strength of the jet, nor does it occupy the proper average latitude. The fields of $\bar{u}$ at different longitudes possess maxima at different latitudes and elevations; if these maxima are averaged with respect to longitude, the result according to Crutcher's winter data is 38 m sec^{-1}, as opposed to a maximum of 34 m sec^{-1} for $[\bar{u}]$, while at longitude 140°E, just south of Japan, $\bar{u}$ reaches 65 m sec^{-1}. Likewise the fields of $[u]$ at different times possess maxima at different latitudes and elevations. But the full strength of the jet, often exceeding 100 m sec^{-1} at individual points, and the full amplitude of its meanders, are evident only in observations which have not been averaged.

CHAPTER IV

THE PROCESSES WHICH MAINTAIN THE CIRCULATION

In Chapter II we introduced the physical laws which govern the circulation of the atmosphere, and the dynamic equations which express these laws in mathematical form. In Chapter III we described some of the features of the circulation as revealed by observations, giving particular attention to the average fields of wind velocity, temperature, and water-vapour content. The application of the physical laws to the explanation of the observed circulation is the task which now confronts us.

The most direct way to accomplish this task would be to solve the dynamic equations. At present we lack a suitable means of solution. We must therefore proceed more indirectly.

In their usual form the dynamic equations enumerate the physical processes which directly affect any quantity. For example, the thermodynamic equation states that the temperature may be altered by advection, adiabatic compression or expansion, and net heating. It is sometimes possible to evaluate the long-term influence of each process affecting some feature of the circulation by recourse to the observational data. A knowledge of the magnitude of each process will not by itself constitute an explanation of the circulation, since it will not reveal why each process assumes the value which it does. Nevertheless, an understanding of the relative importance of the separate processes can be of considerable aid in formulating a qualitative explanation or in assessing the worth of any explanation which may have been offered.

The balance requirements

Consider the total mass of water contained in the region of the atmosphere north of a given latitude. This quantity may be temporarily increased by evaporation from the underlying Earth or decreased by precipitation falling to the Earth. It may also be increased or decreased by an inflow or outflow of moist air across the southern boundary. There is no necessity for the amount of evaporation taking place north of a given latitude to balance the amount of precipitation falling there, but, if the long-term average rates of evaporation and precipitation fail to balance, enough water must be transported within the atmosphere into or out of the region to balance the deficit or excess of evaporation. The need for this condition to be fulfilled constitutes the balance requirement for the transport of water in the atmosphere. It is a matter of convention that the evaporation and precipitation are regarded as determining a balance requirement for the transport rather than vice versa; nothing implies that one process is the cause while another is the effect.

It has been known for some time that evaporation exceeds precipitation in subtropical latitudes, while precipitation exceeds evaporation in middle and higher latitudes and also near the Equator. It follows that the motions of the atmosphere must be such as to transport water from the subtropics to lower and also to higher latitudes. A compensating return transport results from the combined effect of oceans, rivers, and flow within the ground.

A number of estimates of the average annual evaporation and precipitation at various latitudes are available. Figure 20 illustrates a recent Pole-to-Pole compilation of estimates by Sellers (1966). From the excess of precipitation over evaporation the required northward transport of water by the atmosphere has been derived, and is shown in Figure 21. The peak values in the tropics and in middle latitudes are the most prominent features. An interesting detail is the northward transport across the Equator needed to feed the intertropical convergence zone, whose mean position is somewhat to the north.

The precipitation curve in Figure 20 is based mainly upon direct measurements. These, however, are almost non-existent over the open ocean, while the amounts measured at island stations may differ considerably from the amounts falling over the oceans nearby. The evaporation curve is derived largely from the detailed work of Budyko (1956, 1963) and his collaborators. Over the oceans the evaporation has been computed from an empirical formula involving temperature, humidity, and wind speed, while over land areas it has been computed from precipitation and run-off. Much care has gone into the estimates of evaporation and precipitation, but in view of the many uncertainties we cannot yet regard them as the final word.

For computational purposes it is desirable to express the water balance in analytic form. If we integrate the general formula (10) over the volume of the region north of latitude φ_1, using (11), and then average over time, we find that

$$\int_0^\infty 2\pi r \cos\varphi_1 \, [\overline{\rho X v}] \, dz = -\int_0^\infty \int_{\varphi_1}^{\pi/2} 2\pi r^2 \cos\varphi \, [\overline{\rho dX/dt}] \, d\varphi \, dz \,, \tag{84}$$

where X may be any scalar quantity, $[\overline{\rho X v}]$ is measured at latitude φ, and it is assumed that there is no transport across the lower boundary by the motion of the atmosphere. An approximation to (84) obtained by integrating (47) over the mass of the region, using (48), is

$$\int_0^{p_0} 2\pi a \cos\varphi_1 \, [\overline{X v}] \, g^{-1} \, dp = -\int_0^{p_0} \int_{\varphi_1}^{\pi/2} 2\pi a^2 \cos\varphi \, [\overline{dX/dt}] \, g^{-1} \, d\varphi \, dp \,. \tag{85}$$

In (84) the longitude and time averages denoted by brackets and bars are for fixed φ and z; in (85) they are for fixed φ and p. The latter form is more convenient for computation when the available data are at standard pressure-levels.

If q represents the mass of water per unit mass of air,

$$\int_0^{p_0} (dq/dt) \, g^{-1} \, d\,p = E_0 - P_0 \,, \tag{86}$$

where E_0 and P_0 denote the rates of evaporation from the Earth and precipitation upon the Earth. Equating X to q in (85), we obtain the water-balance equation

$$\int_0^{p_0} 2\pi a \cos\varphi_1 \, [\overline{q v}] \, g^{-1} \, dp = -\int_{\varphi_1}^{\pi} 2\pi a^2 \cos\varphi \, [\overline{E_0 - P_0}] \, d\varphi \,. \tag{87}$$

The left-hand side of (87) represents the total transport of water across latitude φ_1. The right-hand side has been used in constructing the transport curve in Figure 21 from the values of E_0 and P_0 in Figure 20. In most applications the horizontal transport of liquid and solid water is disregarded, and q is assumed to represent specific humidity.

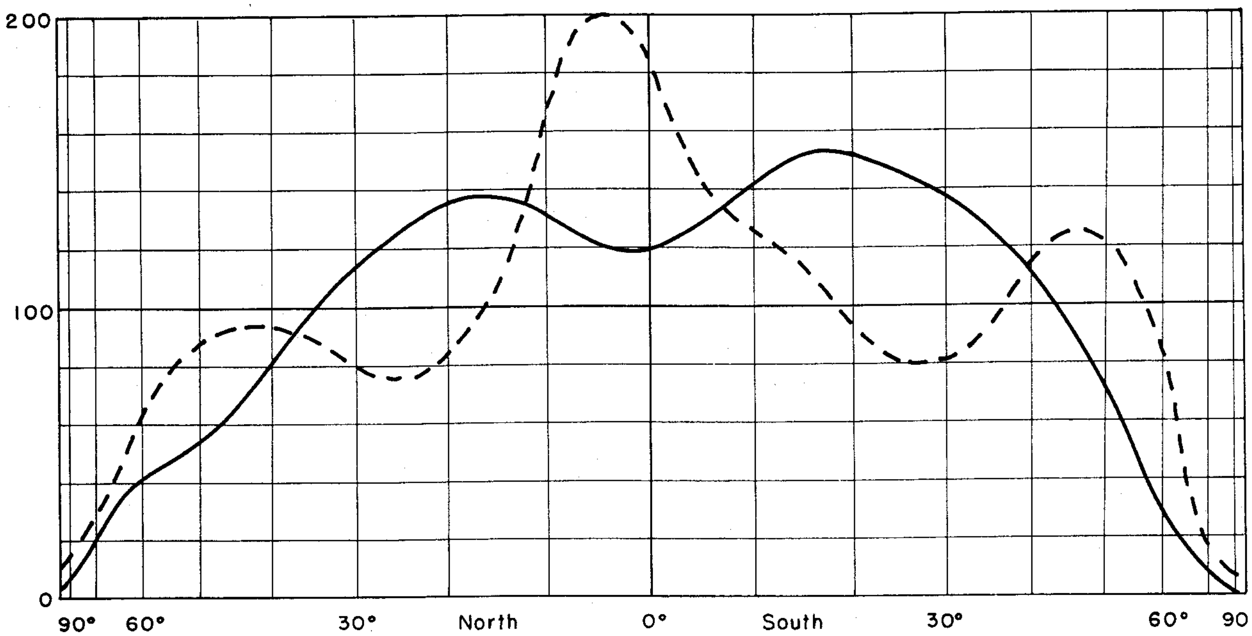


Figure 20. — Average annual evaporation (solid curve) and precipitation (dashed curve) per unit area as given by Sellers (1966). Values are in centimetres of water per year, or g cm^{-2} year^{-1} (scale on left)

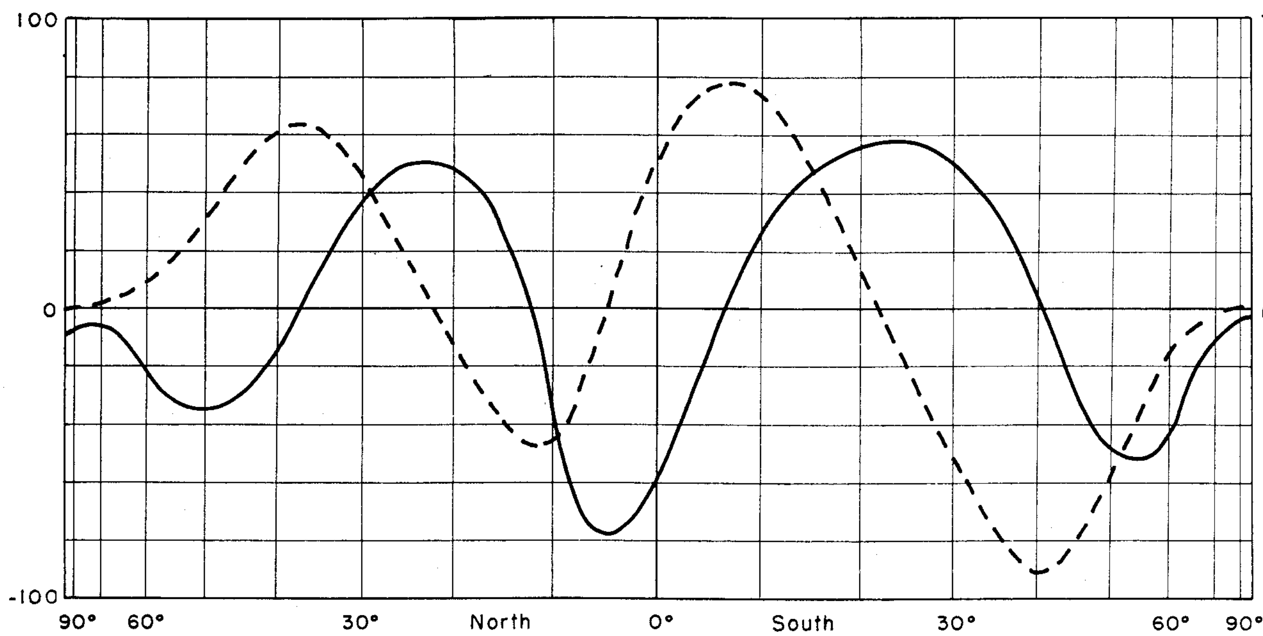


Figure 21. — Excess of evaporation over precipitation (solid curve) as given by Sellers (1966), in g cm^{-2} year^{-1} (scale on left), and northward transport of water in the atmosphere required for balance (dashed curve) in units of 10^{11} g sec^{-1} (scale on right)

It is to be stressed that the evaporation and precipitation require a transport of water only in the sense that if they exist, the transport must exist. They cannot be regarded as the cause of the transport. If the circulation were unable to carry water into the equatorial zone, for example, there would simply be less precipitation or more evaporation there.

The atmosphere does not exchange dry air — the fraction of the air other than water in its various phases — with its environment in significant amounts. The balance requirement for dry air is therefore very simple; there can be no net long-term flow of dry air across any latitude. It follows that there is a net flow of air across each latitude, equal in mass to the net flow of atmospheric water. As much mass flows across latitude 40°S, for example, as would if there were a uniform north wind of 0.3 cm sec^{-1} at all levels. The fact that there is no net flow of dry air rather than no net total flow of air across each latitude is inconsequential for many purposes, but it must be recognized for a proper appreciation of the energy balance.

The balance of absolute angular momentum is analogous to that of water. Angular momentum may be exchanged between the atmosphere and the underlying surface, and it may be transported horizontally by the motion of the atmosphere. Although individual masses of air do not conserve their angular momentum even approximately, still the pressure torque within the atmosphere transfers angular momentum only from one longitude to another, while the frictional torque transfers it almost entirely from one elevation to another. Any net exchange of angular momentum with the underlying Earth by the region of the atmosphere north of a given latitude must therefore be balanced by a transport of angular momentum across that latitude.

As Hadley noted long ago, the atmosphere exerts a westward frictional drag upon the Earth in the latitudes of the trade winds, whence angular momentum is transferred to the atmosphere from the Earth. In middle latitudes where the westerlies prevail, angular momentum is returned to the Earth. There is an additional weak transfer to the atmosphere in the polar caps.

The frictional drag is often spoken of as if it were the only means for exchanging angular momentum between the atmosphere and the Earth, but another mechanism can operate wherever there are mountains, hills, or smaller irregularities. If there is a horizontal pressure difference across a mountain range, the air will effectively push the mountain, and the rest of the Earth with it, toward lower pressure; the mountain therefore pushes the air toward higher pressure. Although it seems natural to picture the air as piling up on the windward sides of mountains, and thereby augmenting the frictional torque, it is not obvious that this should be the case, since many mountain masses are so large that the pressure difference across them depends mainly upon the positions of migratory cyclones and anticyclones.

If X represents the angular momentum M in (84), we find from (17) and (25) that

$$\int_0^\infty 2\pi r^2 \cos\varphi_1 \, [\overline{\rho u v}] \, dz + \int_0^\infty 2\pi r^3 \, \Omega \cos^3 \varphi_1 \, [\overline{\rho v}] \, dz$$

$$= \int_{\varphi_1}^{\pi/2} 2\pi r^3 \cos^2 \varphi \, [\overline{T_{0\lambda}}] \, d\varphi - \int_0^\infty \int_{\varphi_1}^{\pi/2} r^2 \cos\varphi \, (\Sigma \overline{p}_{\mathrm{E}} - \Sigma \overline{p}_{\mathrm{W}}) \, d\varphi \, dz, \tag{88}$$

where Σp_{E} and Σp_{W} are the sums of the pressures on the east and west sides of the mountains or other sloping terrain intersecting the latitude circle in question, and $T_{0\lambda}$ is the eastward component of the frictional stress T_0 at the Earth's surface. The terms on the left represent the transports of relative angular momentum and Ω-momentum.

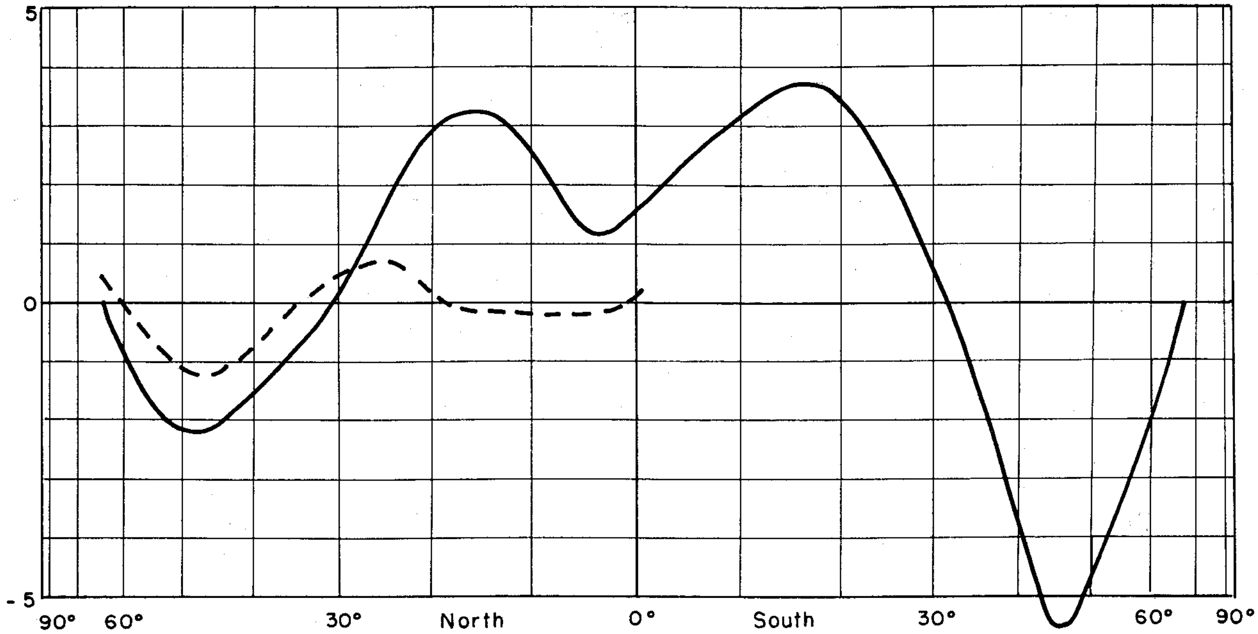


Figure 22. — Average eastward torque per unit area exerted upon the atmosphere by surface friction (solid curve) as estimated by Priestley (1951), and by mountains (dashed curve) as estimated by White (1949) north of 25°N, and Yeh and Chu (1958) south of 25°N. Unit is 10^8 g sec^{-2} (scale on left)

Figure 22 shows the frictional torque as determined by Priestley (1951), together with the mountain torque north of 25°N as determined by White (1949), and south of 25°N as given by Holopainen (1966) on the basis of a study by Yeh and Chu (1958). Priestley used the familiar empirical formula

$$T_{0\lambda} = C_D\, \rho\, [u^2 + v^2]^{1/2}\, u \tag{89}$$

to compute the frictional drag over the oceans, choosing the value 0.0013 for the dimensionless drag coefficient C_D. He then determined the torques which would follow if the ocean stresses were representative of entire latitude belts. He omitted the polar caps, where the torques should be small in any case. White based his computations upon normal pressure charts and simplified profiles of the principal mountain ranges.

Evidently the mountain torque is by no means negligible; in temperature and subtropical latitudes it tends to have the same sign and order of magnitude as the frictional torque. If this result holds also in the southern hemisphere, the effect of the mountain torque might be fairly well incorporated by increasing the drag coefficient C_D, whose proper value is not very well known in any case. Hutchings and Thompson (1962) have found that the relatively small New Zealand Alps add nearly 10 per cent to the total torque in their latitude band; perhaps the much higher Andes could nearly double the torque. Possibly there is a further contribution of the same sign from hills and other irregularities of intermediate size, whose effects are presumably not accounted for by Priestley's value of C_D.

It is well to note the implications of the apparent agreement between the mountain torque and the frictional torque. If the torques were of equal strength, and if the mountain torque did not agree in sign with the frictional torque, the total torque could not be inferred from the surface wind field. The theory of the general circulation on an idealized uniform Earth would then not be applicable to the real atmosphere.

Priestley observed that his middle-latitude torques were insufficient to balance his low-latitude torques, and noted that the situation could not be remedied simply by assuming a larger drag coefficient over land. He thereupon multiplied the middle-latitude torques by the factor 1.4 needed to achieve a balance. His amended torque, and the transport of angular momentum demanded by the balance requirements, are shown in Figure 23. The mountain torque is not explicitly included. The outstanding feature is the poleward transport across the subtropics in either hemisphere.

This transport must be almost entirely a transport of relative angular momentum. Although Ω-momentum is typically much greater than relative momentum, it is nearly constant at a fixed latitude, and in the absence of any appreciable net mass transport there can be no large transport of Ω-momentum. Actually, since Ω-momentum increases with elevation, a direct meridional cell will bring about some poleward transport, while the net flow of mass across certain latitudes requires an additional flow of Ω-momentum; these amounts may be computed from the second term on the left of equation (88). The intense Hadley cell determined by Palmén and Vuorela (1963), shown in Figure 18, yields an extreme poleward Ω-momentum transport of 2.2×10^{25} g cm^2 sec^{-2} across latitude 12°N in winter, but there is little transport across this latitude in summer. Moreover, the contribution by the Hadley cell is virtually cancelled by an equatorward flow of 1.4×10^{25} g cm^2 sec^{-2} across 12°N brought about by the net mass flow. The extreme poleward transports by the mass flow of 1.3×10^{25} and 1.7×10^{25} g cm^2 sec^{-2} occur near 35°N and 40°S, where the meridional cells are too weak to contribute appreciably. Comparison with Figure 23, where the required transports reach 20×10^{25} and 40×10^{25} g cm^2 sec^{-2} in the two hemispheres, suggests that most of the total transport is a transport of relative angular momentum.

There can be little doubt that the estimates in Figures 22 and 23 have the proper sign and order of magnitude, but by comparison with Figures 20 and 21 the actual values are poorly known. The mountain torque has received insufficient attention, but the uncertainty of the frictional torque is due largely to our inadequate knowledge of friction, particularly over irregular land areas.

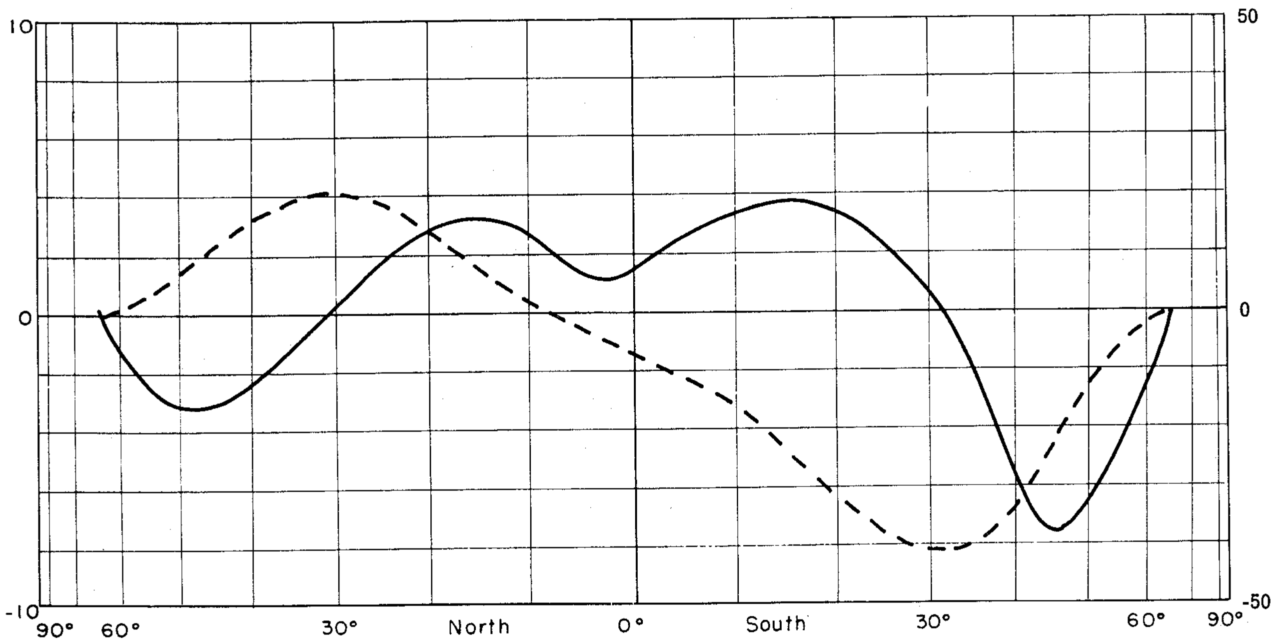


Figure 23. — Modified estimate by Priestley (1951) of average eastward torque exerted upon the atmosphere by surface friction (solid curve) in units of 10^8 g sec^{-2} (scale on left), and the northward transport of absolute angular momentum required for balance (dashed curve) in units of 10^{25} g cm^2 sec^{-2} (scale on right)

As in the case of the water balance, the torques are not a causal requirement for the transport of angular momentum. If the circulation were unable to transport angular momentum to middle latitudes, the surface westerlies there simply would not occur.

The balance of total energy presents a more complicated problem. Not only does the atmosphere exchange energy with the underlying Earth, but both the atmosphere and the underlying Earth gain energy from the sun and lose it to outer space through radiation. On this account it is desirable to examine first the energy balance of the entire atmosphere-ocean-Earth system, and then the more complicated energy balance of the atmosphere alone.

The incoming solar energy, which is the ultimate driving force for the atmospheric and oceanic circulations, is more intense in low than in high latitudes. Some of this energy is reflected or scattered back to space and plays no further role in the energy balance. The remainder is absorbed by the atmosphere and the Earth's surface; this portion, like the total, is more intense in low latitudes.

The energy re-radiated to space by the atmosphere and the Earth's surface is also more intense in low latitudes, although not so much more intense as one might expect in view of the higher temperature. Much of the outgoing radiation takes place from the uppermost layers of water vapour in the atmosphere; these extend to great heights in low latitudes and are therefore about as cold as the uppermost water vapour in higher latitudes. The net result is therefore a considerable excess of heating in low latitudes. It follows that there must be a poleward transport or transfer of energy across virtually every latitude. This transport may occur within the atmosphere or the oceans.

In contrast to the scarcity of numerical estimates of the angular-momentum exchange, there are numerous estimates of the incoming and outgoing radiation. Figure 24 is again based upon the values compiled by Sellers (1966) from a number of sources. The upper curve shows the solar energy reaching

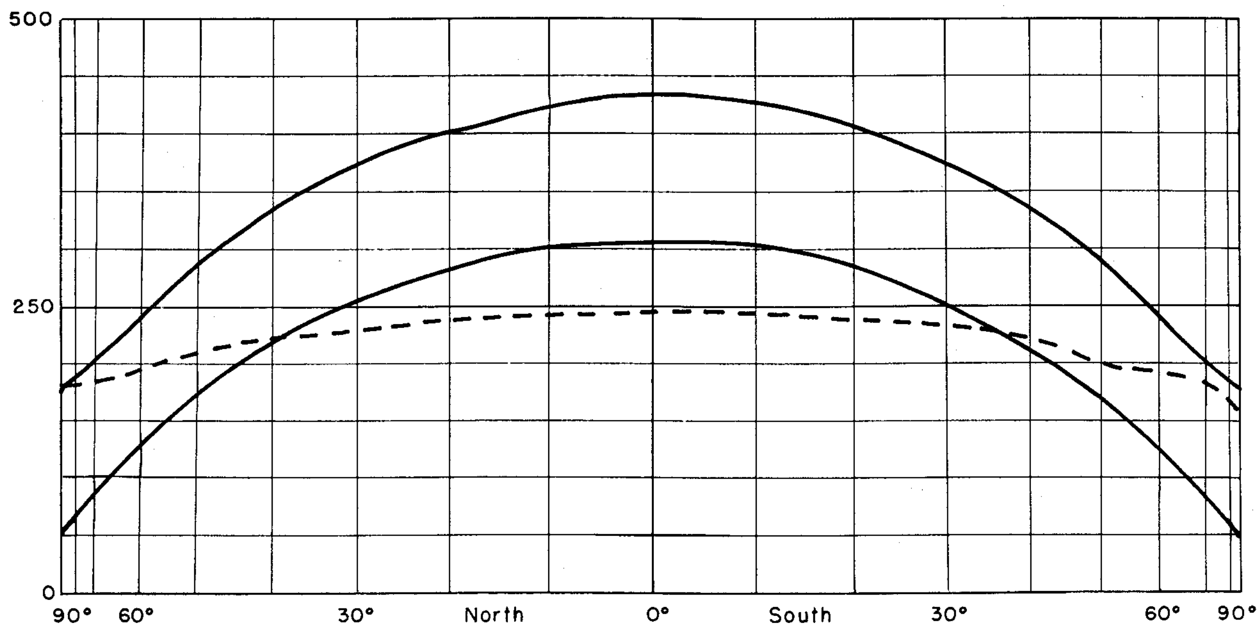


Figure 24. — Average solar energy reaching the extremity of the atmosphere (upper solid curve), average solar energy absorbed by the atmosphere-ocean-Earth system (lower solid curve), and average infra-red radiation leaving the atmosphere-ocean-Earth system (dashed curve), as given by Sellers (1968). Values are in watts m^{-2} (scale on left). (1 watt $m^{-2} = 1.435 \times 10^{-3}$ cal cm^{-2} min^{-1} = 0.754 kilolangleys $year^{-1}$.)

the extremity of the atmosphere; by comparing this amount with the amount absorbed, we see that the albedo, or the fraction reflected or scattered back to space, is about 30 per cent in low latitudes but exceeds 50 per cent in the polar regions. Figure 25 presents the net radiation and the required northward transport and transfer of energy. The principal features are again the peak values in middle latitudes.

The usual method of estimating the outgoing radiation requires rather involved computations of the emission in various wavelengths from the various atmospheric constituents, and individual estimates are far from being in complete agreement. Direct measurements have recently been made possible by the satellite. Figure 26 compares the values already shown in Figure 23 with the values obtained by Winston (1967) from one year of satellite observations. The calibration problems of the satellite-borne radiometers are not completely solved, and it would be premature to replace the conventional values by the new ones, but the general shape of the satellite curve is of considerable interest. It shows a pronounced relative minimum at 5° north, which actually appears near the Equator in every season. This is apparently due to the presence of the intertropical convergence zone, where the clouds and moisture ordinarily extend to great heights and hence radiate at a lower temperature than do the surrounding latitudes. Some of the pre-satellite estimates have indicated a similar although less pronounced minimum just north of the Equator.

Again the net heating cannot be regarded as the cause of the energy transport, except in the sense that heating is the ultimate cause of the entire atmospheric and oceanic circulations. If the circulations were unable to transport so much energy, the low latitudes would simply be warmer and the high latitudes would be colder, and the excess net heating in low latitudes would not be so great.

The energy balance of the atmosphere alone presents still further complications. Total energy need not include all forms of energy, but, having included one form, it must include all others which are directly or indirectly converted into this form in significant amounts. In the atmosphere and the underlying ocean and land the important forms are kinetic energy, potential energy, and internal energy, the last form including thermal internal energy and the latent energy of condensation and fusion of water. Moreover, in addition to being directly transported in each form, energy may be transferred horizontally by the pressure forces.

Before describing the atmospheric energy balance we must note that there is some ambiguity in defining the transport of energy by the atmosphere alone or the ocean alone; this ambiguity is the combined result of two circumstances. First, the zero marks on the scales for measuring internal and potential energy are somewhat arbitrary. Second, there is an exchange of mass between the atmosphere and the ocean, and hence a net flow of mass within the atmosphere and also within the ocean. The ambiguity may be removed by choosing zero marks, but different choices will lead to different pictures of the energy balance.

Ordinarily sea-level is chosen as the surface of zero potential energy, in which case the atmosphere will not exchange potential energy with the oceans, although the Earth will gain some potential energy when rain or snow falls at higher elevations. Likewise, absolute zero is generally chosen as the temperature of zero thermal internal energy, in which case the horizontal transfer of energy by internal pressure forces will be proportional to, and additional to, the transport of thermal internal energy, in the ratio R/c_v, or about 2/5. The sum of these quantities will then equal the transport of sensible heat, whose value per unit is $c_p T$.

Both the liquid and vapour phases of water have been chosen as the phases of zero latent energy. The former choice is the most frequent. In this event the oceans transport no latent energy, but the atmosphere transports large amounts poleward across middle latitudes. If the vapour phase is chosen

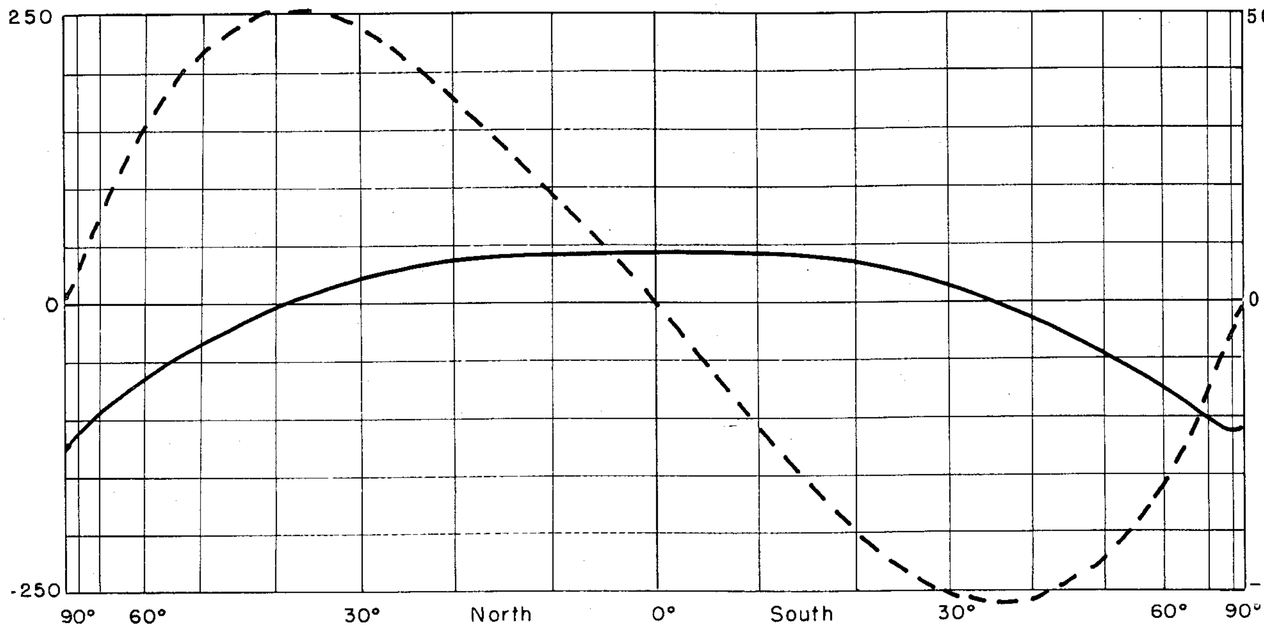


Figure 25. — Excess of absorbed solar radiation over outgoing infra-red radiation (solid curve), as given by Sellers (1966), in watts m^{-2} (scale on left); and northward transport of energy by the atmosphere and oceans required for balance (dashed curve), in units of 10^{14} watts (scale on right)

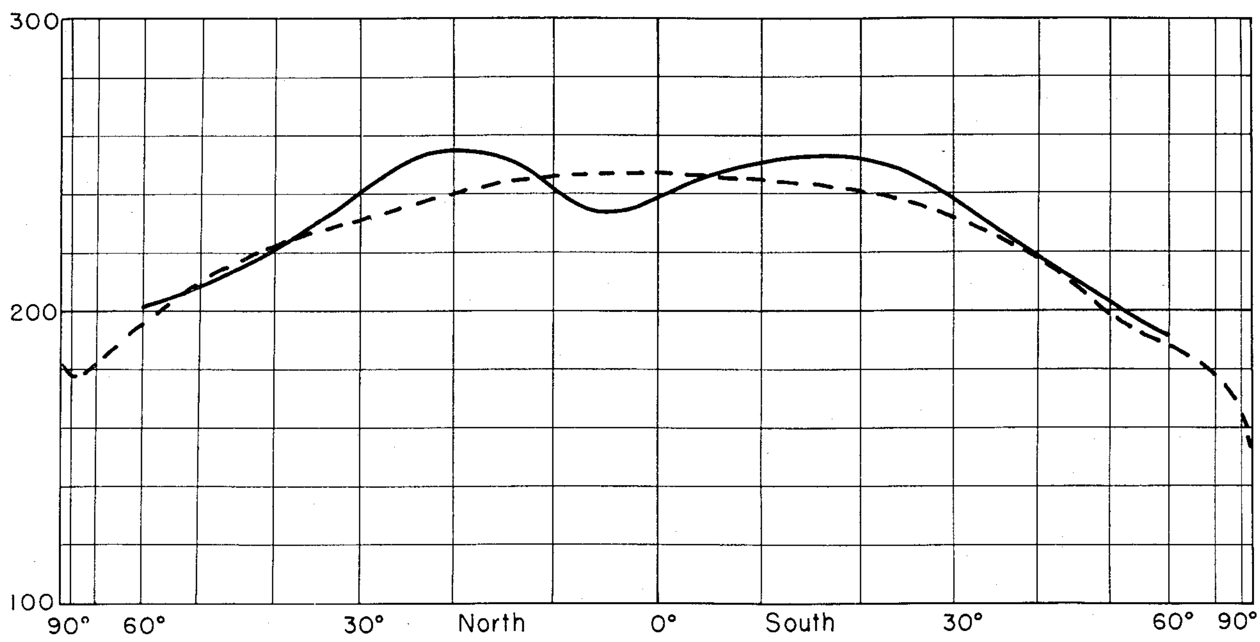


Figure 26. — Average infra-red radiation leaving the atmosphere-ocean-Earth system determined by conventional methods (dashed curve), as given by Sellers (1966); and determined from satellite measurements (solid curve), as given by Winston (1967). Values are in watts m^{-2} (scale on left)

instead, the oceans transport large amounts of negative latent energy equatorward across middle latitudes, i.e. they transport energy poleward, while the atmosphere transports only minor amounts.

Since the atmosphere must satisfy the balance requirement for total energy in either event, this requirement must also depend upon the choice of phase. If the liquid phase is chosen, the net radiation received or emitted by the atmosphere, the thermal internal energy transferred to the atmosphere from the Earth, and the latent energy supplied to the atmosphere by evaporation must together be balanced by transports of sensible heat, potential energy, kinetic energy, and latent energy within the atmosphere. If the gaseous phase is chosen, evaporation adds no energy to the atmosphere, but precipitation removes negative energy, i.e. adds energy.

If we set $X = K + \Phi + I$ in (84), we find from (24) after some arrangement of terms that

$$\int_0^\infty 2\pi r \cos \varphi_1 \left[\overline{\rho (K + \Phi + I)\, v + p v}\right] dz = -\int_0^\infty \int_{\varphi_1}^{\pi/2} 2\pi r^2 \cos \varphi \left[\overline{Q + \mathbf{V}\cdot\mathbf{F}}\right] d\varphi\, dz. \tag{90}$$

The term pv on the left represents the work done against a unit area of the southern boundary by the pressure forces. As already noted, for a dry atmosphere it would be proportional to, and additional to, the term $\rho I v$, which would represent the transport of internal energy. For the real atmosphere pv is still proportional to the transport of thermal internal energy.

In the strictest sense equation (90) is not applicable, because the mass transport across the Earth's surface was neglected in deriving it. However, it may be used if I includes both thermal and latent internal energy, and if the gain of latent energy resulting from evaporation from the surface is included in Q. Alternatively, I may include only thermal internal energy, and the release of latent heat by condensation may be included in Q.

Figure 27 shows the amounts of energy gained or lost by the atmosphere by various processes, including evaporation rather than precipitation. Again the values are those given by Sellers. The sum of these amounts and the required transport of energy in the atmosphere are shown in Figure 28. Again the transport presents a rather smooth curve, with peak values in middle latitudes. One may again question how accurately the various curves are really known.

The transport of latent energy in the atmosphere is for practical purposes proportional to the transport of water, which balances the excess of evaporation over precipitation. It follows by subtraction that the transport of the remaining forms of energy must balance the net radiation, the internal energy supplied from the Earth, and the latent energy released by condensation. This is the result which would have been deduced directly if the vapour phase of water had been chosen as reference.

The curves in Figure 29 are determined directly from the curves in Figures 21, 25, and 28. They show separately the required transports of thermal internal energy by the ocean currents, of sensible heat plus potential and kinetic energy by the atmospheric circulation, and of latent energy. The last quantity is regarded as being transported by the atmosphere under the usual convention, but would be considered to be transported mainly by the ocean under the less common convention that the vapour phase possesses zero latent energy.

The transport of internal energy by the ocean again conforms to the pattern of a single peak in each hemisphere, but a striking feature of the remaining transports is their relative irregularity as compared to the total transport. The two curves have steep slopes of opposite signs in the tropics. A simple explanation, which however requires verification, would be that the air flowing into the intertropical convergence

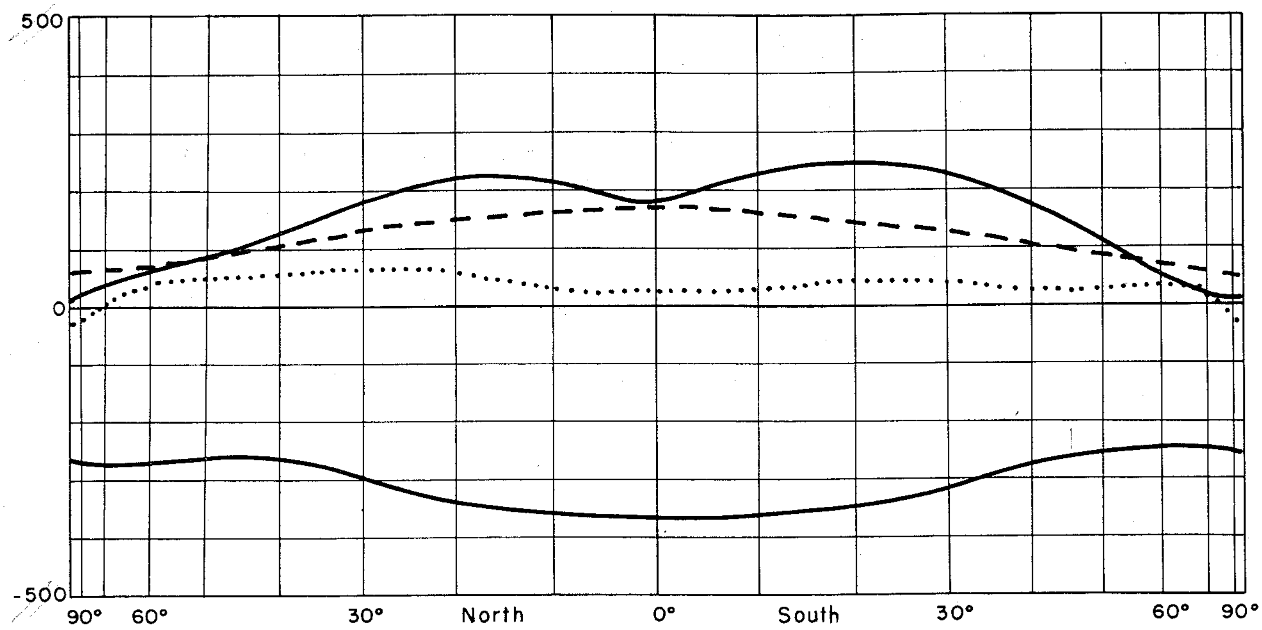


Figure 27. — Average short-wave radiation absorbed by the atmosphere (upper solid curve), average sensible heat received by the atmosphere from the underlying surface (dotted curve), average latent heat received by the atmosphere from the underlying surface (dashed curve), and average infra-red radiation leaving the atmosphere (lower solid curve), as given by Sellers (1966). Values are in watts m^{-2} (scale on left)

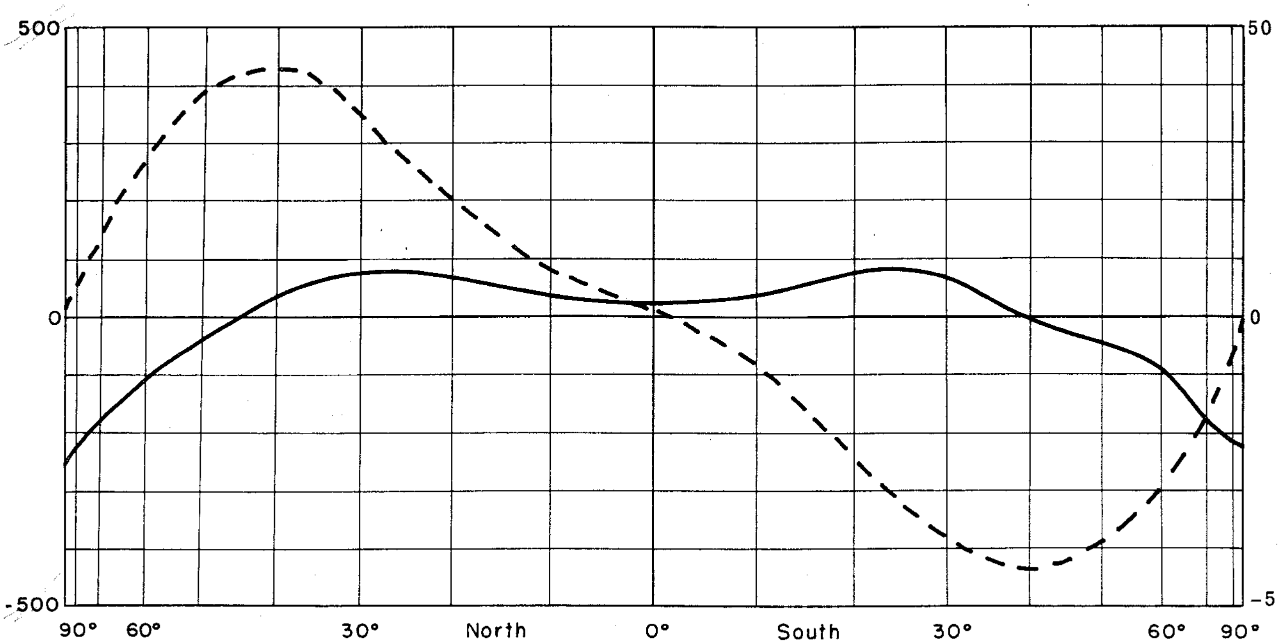


Figure 28. — The average net gain of energy from environment by the atmosphere (solid curve), as given by Sellers (1966) in watts m^{-2} (scale on left); and the northward transport of energy by the atmosphere required for balance (dashed curve), in units of 10^{14} watts (scale on right)

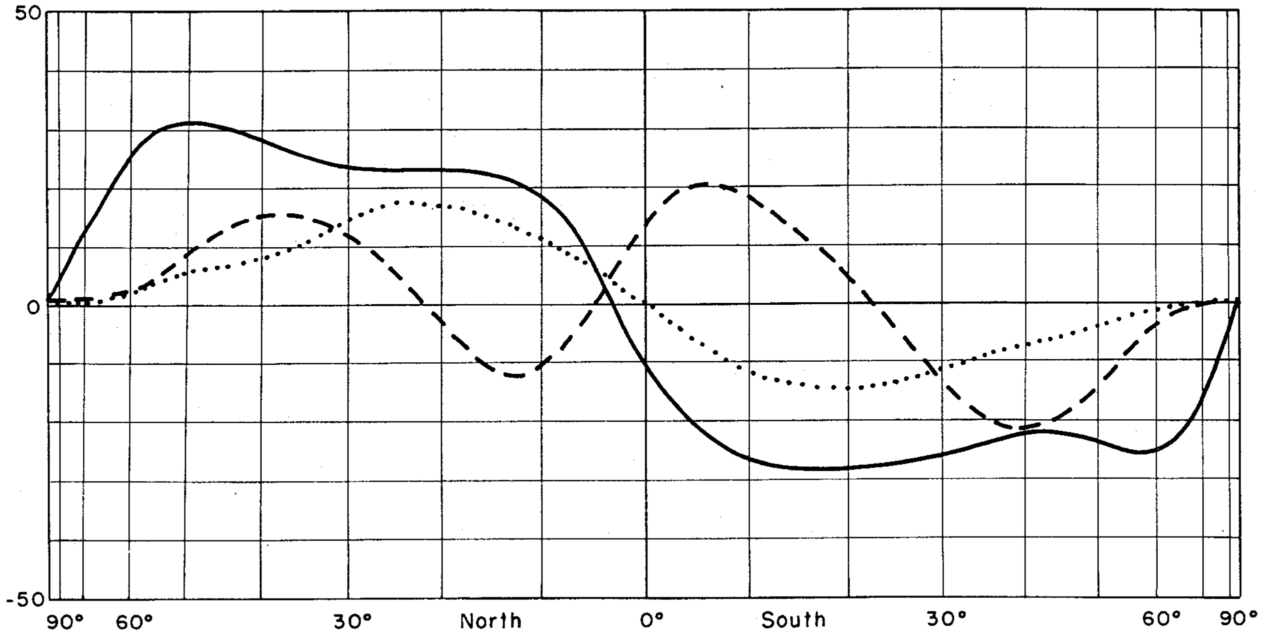


Figure 29. — The northward transport of sensible heat plus potential energy plus kinetic energy by the atmosphere (solid curve), of latent heat by the atmosphere (dashed curve), and of sensible heat by the oceans (dotted curve), required for balance, in units of 10^{14} watts (scale on left)

zone is loaded with latent energy; as the air rises and precipitation is released this latent energy is converted into thermal internal energy and potential energy, which is then transported out of the convergence zone.

It is to be stressed that the preceding curves show only the transports demanded by the observed exchanges of water, angular momentum, and energy between the atmosphere and its environment or between the system and its environment. Whereas the general features of these curves have been known for many years, it is only recently that observations have enabled us to evaluate the transports directly.

Former theories of the general circulation

Before considering the manner in which the balance requirements now appear to be fulfilled, we shall examine some of the views which prevailed in the past. So-called theories of the general circulation, whether they were real attempts to account for the circulation by dynamical theory, or merely descriptive schemes unaccompanied by explanations, appeared in abundance during the nineteenth and early twentieth centuries. Bergeron (1928) even remarked that there were as many theories as authors. We cannot discuss or even mention the great majority of these, but we shall attempt to identify those ideas which most greatly influenced the subsequent development of the subject, and which have led us to our present state of knowledge.

It is a relatively easy matter today to determine whether any newly proposed scheme of the general circulation agrees in its main features with observations, and to discard the scheme if it does not. In judging the worth of an older theory, we must therefore recall that much of what we now look upon as the observed circulation was unobserved as recently as World War II, and that at the close of the nineteenth century even such familiar entities as the stratosphere had not been discovered. Thus the main features of some of the former schemes were their speculations as to the circulation in the regions where observations were not available.

If the circulation were uniquely determined by the governing laws, any proposed scheme later found not to agree with the newer observations would necessarily violate some dynamical principle. We must therefore note that there may be many different circulations which satisfy the dynamic equations. Moreover, even if the external conditions should determine the circulation uniquely, considerably different circulations might be properly deduced from slightly different assumptions concerning the external conditions; this possibility has been cited by Bergeron (1928) as a contributing factor to the abundance of theories.

We should therefore regard a theory as a worthy contribution to the knowledge of its time if it contains no flaw in its dynamical reasoning, and if it is consistent with the observations available when it was formulated. A necessary condition for a theory to be dynamically sound and also compatible with observations is that it satisfy the balance requirements demanded by these observations. The condition is not sufficient; a proposed circulation may transport the proper amounts of angular momentum, water, and energy across each latitude and still be deficient in other respects. Noting this possibility, we may yet judge the worth of a proposed scheme partly by its ability to satisfy the balance requirements.

The circulation pictured by Hadley (1735), discussed in detail in the first chapter, satisfies the balance requirements demanded by observations which were then available, although not all of the requirements which more recent observations demand. The upper current carries as much mass poleward as the lower current carries equatorward. It carries more angular momentum, since the westerlies aloft are stronger. It also carries more sensible heat and internal energy, if the stratification is stable.

Hadley's scheme does not contain the weaker equatorward flow of angular momentum at high latitudes, but neither does it contain the polar easterlies which would demand it. Hadley did not consider water, but presumably his circulation would carry more water equatorward than poleward across every latitude, yielding the equatorial excess of precipitation and perhaps the deficit in the subtropics, but also giving a deficit in the polar regions, in contrast to what is observed.

Figure 30 illustrates Hadley's circulation schematically. Hadley himself presented no figure; we have introduced Figure 30 for comparison with the figures which have accompanied the numerous subsequent works.

In Hadley's picture the horizontal transports needed to satisfy the balance requirements are accomplished by the simplest possible mechanism — a meridional circulation where the uniform poleward current at one elevation carries a different amount of each transported property from the uniform equatorward current at another elevation. Since the atmosphere is not constricted to behave independently of longitude and time, other mechanisms are available. Whenever large-scale eddies such as cyclones are present, poleward flow at one longitude is accompanied by equatorward flow at the same time and elevation at another longitude. The oppositely directed currents may carry different amounts of any property. Theories of the general circulation therefore conform to one of two general schemes — those in which eddy motions are either absent or irrelevant, and those in which the eddies influence the zonally averaged motion by transporting some property from one latitude to another.

Following the publication of Hadley's paper there was a period of more than half a century during which the scientific community was generally unaware of its existence. Similar explanations were even rediscovered by such savants as Immanuel Kant (1756) and John Dalton (1793). Later Dalton encountered Hadley's paper, and, in acknowledging that his own contribution had been completely anticipated, noted

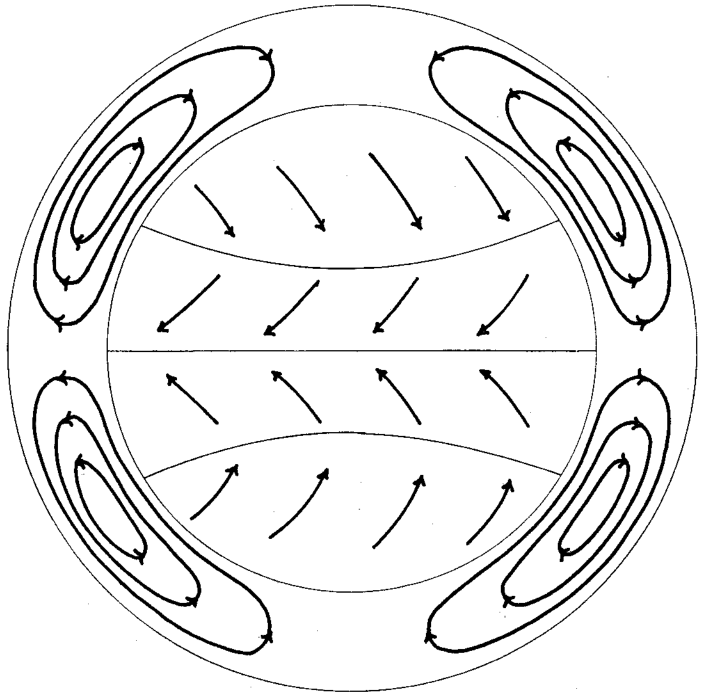


Figure 30. — A schematic representation of the general circulation of the atmosphere as envisioned by Hadley (1735)

the tendency for current works to continue to quote the older inadequate theories, while continuing to ignore the more recent and more acceptable ones. His remarks remain true today.

In due time, however, Hadley's explanation became the one which was quoted in the standard works. Early nineteenth century observations being what they were, there was little reason to doubt the explanation in its essential features.

As the nineteenth century progressed, observations began to cast some doubt upon Hadley's scheme. In particular, there was growing evidence that the prevailing westerlies in the northern hemisphere tended to blow from somewhat south of west, instead of from somewhat north of west as Hadley's explanation would have demanded. Undoubtedly the available data did not really justify such a conclusion, as they were confined largely to oceanic regions. Nevertheless the conclusion was evidently correct, as indicated by today's vastly more complete observations.

With the realization that the surface separating the trade winds from the south-westerlies above them sloped downward toward the north, and reached the Earth's surface in the horse latitudes, the notion became established that the middle-latitude south-westerly current at the surface was simply an extension of the current above the trades. The question then arose as to how the air returned from higher latitudes.

An answer was provided by the eminent meteorologist and climatologist Heinrich Wilhelm Dove (1837). Earlier (1835) Dove had been one of those to rediscover Hadley's explanation of the trades, again under the assumption that absolute velocity rather than absolute angular momentum would be conserved in the absence of east-west forces. He now accepted Hadley's ideas completely as far as low latitudes were concerned, but he favoured the prevailing idea that the south-westerly winds in middle latitudes were a continuation of the south-westerlies above the trades, since he felt that their warmth and humidity demanded an equatorial origin. At that time it was not generally realized that air rising to high levels and sinking again would have to lose most of its moisture.

It followed naturally that the trades themselves should be a continuation of a return current from higher latitudes. Dove rejected the possibility that this current could occur at upper levels, since it appeared impossible for oppositely directed currents to cross in the horse latitudes without altering one another. He was thereby led to a scheme where south-westerly and north-easterly winds in middle latitudes flowed side by side at different longitudes at the same level, rather than one above the other. His warm moist equatorial current was fed by the south-westerlies above the trades while his cold dry polar current fed the trade winds. The equatorial current preferred the oceans and the west coasts of continents, while the polar current preferred the interiors and east coasts, but the longitudes of the currents were not fixed, and familiar local weather changes accompanied the replacement of one current by the other. He could explain the net northward flow, volumewise at least, by the greater specific volume of the equatorial current, but he also felt that it might be largely fictitious, since the observed northward flow could be compensated by southward flow over the interiors of continents, where observations were less abundant.

He even maintained that there were only two winds in middle latitudes — the north and the south — other directions being simply variants. Thus his polar and equatorial currents seem to be none other than what we now call polar and tropical air masses, which he chose to identify by their preferred motion rather than their quasi-conservative thermodynamic properties. He furthermore regarded the middle-latitude storms as resulting from the conflict of the two currents. His circulation is shown schematically in Figure 31.

Dove's scheme can certainly satisfy the energy balance requirements, since the equatorial current is warmer than the polar current. It can satisfy the momentum balance requirements, since the south-westerly winds carry more momentum than the north-easterlies. Under the assumption that the equatorial current cannot carry its water aloft at low latitudes, as Dove had supposed, but must lose it and then reacquire it from the ocean after descending, the scheme can satisfy the balance requirements for water.

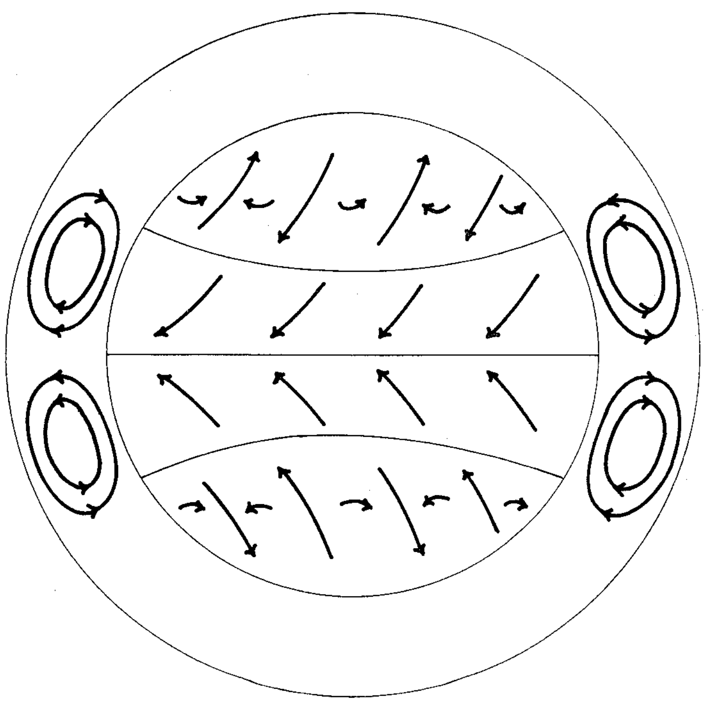


Figure 31. — A schematic representation of the general circulation of the atmosphere as envisioneed by Dove (1837)

Yet it does not have the appeal of Hadley's scheme. As a keen observer rather than a theoretician, Dove offered no tidy explanation for the circulation which he so carefully recorded. His arguments involving the Earth's rotation are more applicable to Hadley's circulation than to his own.

Among the most diligent compilers of weather observations was the naval officer Matthew Maury, whose charts of the winds over the oceans had led to considerable reductions in the normal sailing times between distant points. In 1855 he came forth with his own scheme of the general circulation, which departed considerably from Hadley's, and contained precisely the features which Dove had rejected some years before. It is shown in Figure 32. Instead of the single meridional cell in either hemisphere, or opposing currents side by side, there are two cells — a direct cell like Hadley's within the tropics, and an indirect cell in higher latitudes. The flow above the north-east trades is from the south-west, and the upper-level flow at higher latitudes is apparently supposed to be from the north-east.

Like Dove, Maury used no mathematical formulae, but he was extremely conscious of the balance requirements for water. A distinctive feature of his scheme was the crossing of the meridional currents as they sank in the horse latitudes and also as they rose in the doldrums, and he devoted great efforts to justifying these crossings. He was a great believer in the Grand Design, and he rejected the possibility that the converging currents would mingle and then depart, now in the direction from which they came, and now in the opposite direction, on the grounds that the circulation could not be left so completely to chance. He could see no reason why the currents must cross instead of returning, but he insisted that the lack of balance between precipitation and evaporation in low latitudes and also in high latitudes indicated that they did cross. Like Dove, he was unaware that a high-level current cannot retain its moisture. He believed that crossing without mixing could occur by having vertical columns of air pass one another; his envisioned columns seem to have the dimensions of cumulonimbus towers.

Maury was unable however to offer an explanation for what seems now to be the principal feature of his scheme — the indirect cells in higher latitudes. He accepted Hadley's explanation for the trade-wind cells, and simply said that the cause of the indirect cells had not been explained by philosophers.

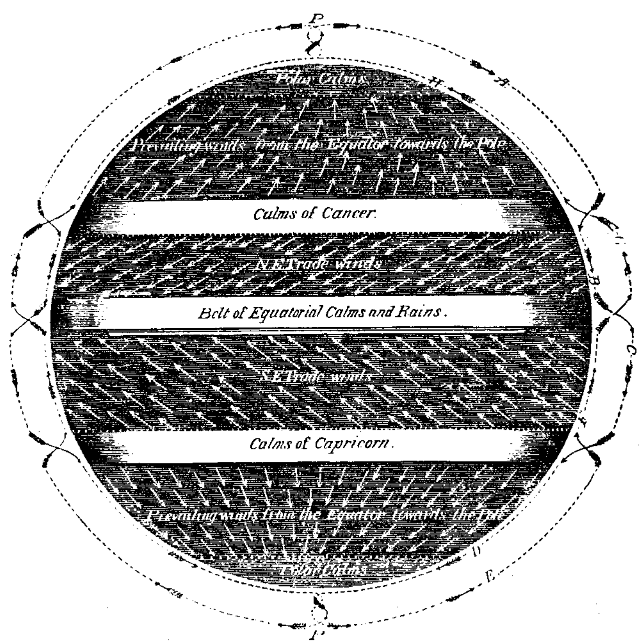


Figure 32. — The general circulation of the atmosphere according to Maury (1855)

Maury's scheme seems to satisfy the balance requirements for angular momentum, in view of the assumed crossing currents in the horse latitudes and the upper-level easterlies in higher latitudes, which, however, are inconsistent with modern observations. It certainly cannot satisfy the energy balance requirements, except for an atmosphere which is heated at the Equator and the Poles and cooled in between. Nevertheless, Maury's book is extremely readable. It became rather popular in his day, and it was instrumental in initiating some of the more rational theories which were to follow.

Among those who read Maury's book was the school teacher William Ferrel, whose interest in the subject was thereby aroused. Here he first learned that the normal pressure was not uniform over the Earth's surface, but highest in the horse latitudes, and lower in the doldrums and especially in the polar regions. He found that he disagreed with some of Maury's ideas, particularly the crossings of the meridional currents, which he felt ought to mix rather than cross. In the following year (1856) he came forth with a theory of his own.

The circulation which he envisioned is shown in Figure 33. It is somewhat like Maury's, except that there are now three cells in either hemisphere, which he felt were demanded by the observations. Unlike Maury, however, Ferrel believed that he could present a complete explanation.

Ferrel's great contribution in this paper was the introduction of a "new" force, the north-south component of the Coriolis force, which he incidentally identified with one of the terms in Laplace's tidal equations, formulated long before Coriolis, and which he believed had not been previously recognized in any meteorological work. Actually he had been anticipated in an unnoticed paper by Tracy (1843), who with inadequate arguments nevertheless deduced the proper direction of the deflection. Ferrel believed that proper consideration of the new force would account for the previously unexplained features not only of the general circulation but of cyclones and smaller disturbances as well.

Ferrel agreed with Hadley that the prime moving force of the atmospheric circulation was the Pole-to-Equator density gradient brought about by solar heating, which he believed should lead to meridional

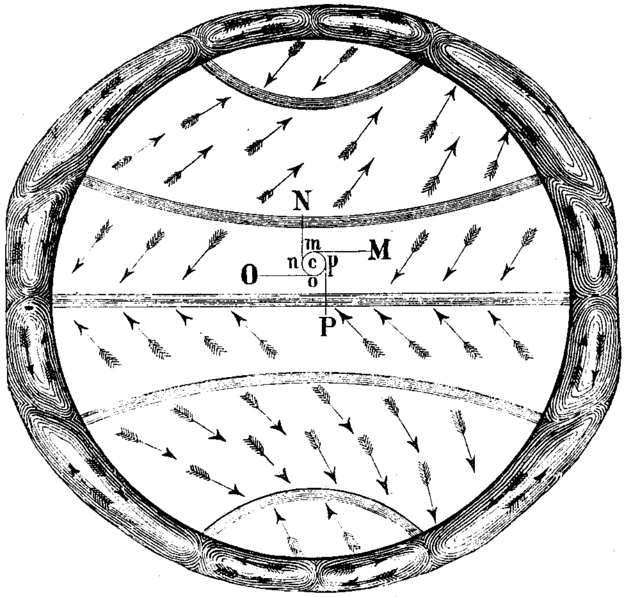


Figure 33. — The general circulation of the atmosphere according to Ferrel (1856)

motions, and hence, through the action of the east-west Coriolis force, to easterly and westerly winds distributed much as Hadley had supposed; but he did not find in Hadley's theory any explanation for the distribution of pressure. He then noted that through the action of the new force the easterlies in low latitudes and the westerlies in higher latitudes should be deflected away from the Equator and the Poles toward the subtropics, thereby creating the observed deficit of pressure at the Equator and the Poles and the excess in the subtropics. To explain the poleward drift in the surface westerlies, he observed that because of surface friction, the winds near the ground would be considerably weaker than the winds somewhat higher up, while the horizontal pressure gradient would be reasonably uniform. The southward Coriolis force near the ground would therefore be insufficient to balance the pressure gradient, and the westerlies would turn poleward, later to rise and return equatorward.

Ferrel also noted that for hydrostatic reasons the latitude of highest pressure must be displaced toward the Equator with elevation. He apparently felt that the opposing currents aloft must meet at this latitude in order to maintain the high pressure, whence he showed inclined boundaries between the low-latitude and middle-latitude cells.

There are certain obvious deficiences in Ferrel's scheme, as well as in his explanation of it. The indirect cells in middle latitudes must transport angular momentum and energy toward the Equator, and neither balance requirement can be satisfied. The middle-latitude westerlies aloft were originally supposed to be maintained by the action of the Coriolis force upon the poleward currents, but now these currents have been replaced by equatorward currents while the westerlies are allowed to remain.

Nevertheless it would be difficult to overestimate the importance of Ferrel's paper. Here he first presented to the meteorological world a correct account of the Coriolis force, a quantitative description of the geostrophic wind, and a partial explanation for its occurrence. His demonstration that the pressure field could adjust itself to fit the wind field, rather than forcing the wind to do all of the adjusting, has often been overlooked by succeeding generations of meteorologists.

Another scientist who read Maury's book was the physicist and inventor James Thomson, who found himself in considerable disagreement with some of Maury's ideas. Thomson was understandably unaware of Ferrel's work, which had been published in a local medical journal, but he had attended a lecture delivered by Murphy (1856), who had also read Maury's book and had suggested that the low pressure at the Poles resulted from the centrifugal force of the westerly currents, which could be treated as large circumpolar vortices. Thomson soon produced his own scheme (1857), which is shown in Figure 34.

After noting Hadley's error concerning the conservation of angular velocity, he otherwise accepted Hadley's arguments with regard to the bulk of the atmosphere, but maintained that the westerly winds near the ground, being slowed by friction, would possess a deficit of centrifugal force relative to the stronger westerlies immediately above, and would therefore drift poleward. In this respect his argument is the same as Ferrel's, differently worded. The southward or northward component of the Coriolis force, as Ferrel pointed out, is simply the excess or deficit of centrifugal force as compared to the centrifugal force of a particle rotating with the Earth. The excess Coriolis force of a rapid west wind over that of a slow west wind is therefore the same thing as the excess centrifugal force of the rapid wind over the slow wind.

Just as there was little observational evidence in Hadley's day to contradict his scheme, so there was little evidence in Thomson's day to contradict his. Thomson's scheme is admirable for its simplicity, and it also satisfies the balance requirements. The indirect cell is confined to such a shallow layer that it transports very little angular momentum or energy. At the same time it can produce the needed poleward transport of water, since the water-vapour content decreases so rapidly with elevation.

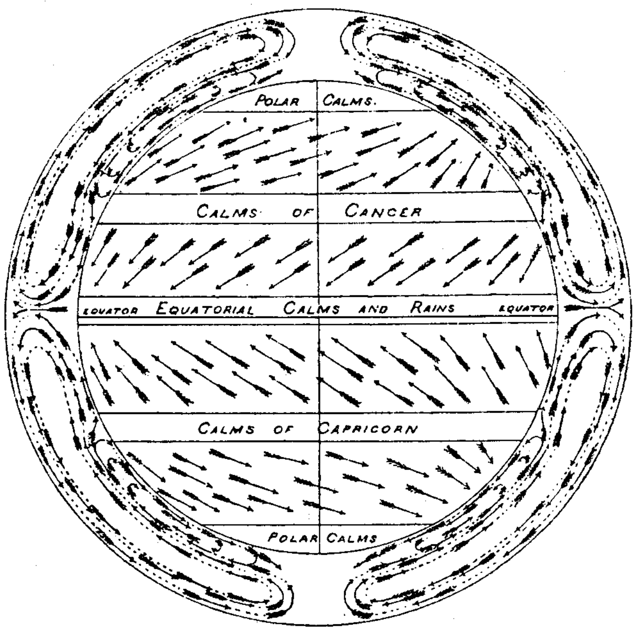


Figure 34. — The general circulation of the atmosphere according to Thomson (1857, 1892)

Thomson's published contribution was limited to an abstract, but years later (1892) in his Bakerian Lecture, delivered two months before his death, he returned to the problem of the atmosphere, and reiterated his belief in his former scheme. He also discussed critically the other schemes which had been offered, and noted the difficulties entailed by Maury's and Ferrel's converging currents aloft.

Ferrel was, however, a relative newcomer to the field, and his ideas were anything but static. He set about formulating his work in mathematical terms, and as result he came up with a revised scheme (1859). It is shown in Figure 35. It is very much like Thomson's, except in the polar regions where definitive observations were lacking in any case. It is therefore equally effective in fulfilling the balance requirements. His paper contains the complete equations of motion for the atmosphere, and an account of the thermal wind relation.

In justifying his scheme, he maintained that if surface friction were absent, while internal friction still existed, the atmosphere would assume a condition of uniform absolute angular momentum. It is often pointed out that such a condition would lead to unrealistically violent winds at high latitudes, but Ferrel went a step beyond his successors and noted that the accompanying pressure gradients required by geostrophic balance would leave the polar regions completely devoid of air. In his computations he had treated the atmosphere as a liquid; as a gas the only real singularities would be at the Poles, but even between 30° and 60° latitude the pressure would drop by a factor of three.

Ferrel maintained that with surface friction the atmosphere would tend toward the same distribution, but to a much lesser extent, the latitudes separating the easterly and westerly surface winds being ultimately determined by the requirement of no net surface torque. He thus purported to account for the observed distribution of zonal wind. He explained the poleward drift of the surface westerlies as in his earlier paper, observing that this drift required a return current somewhere aloft. He noted, however, that in view of the thermal wind relation, the upper-level westerlies must be stronger than the

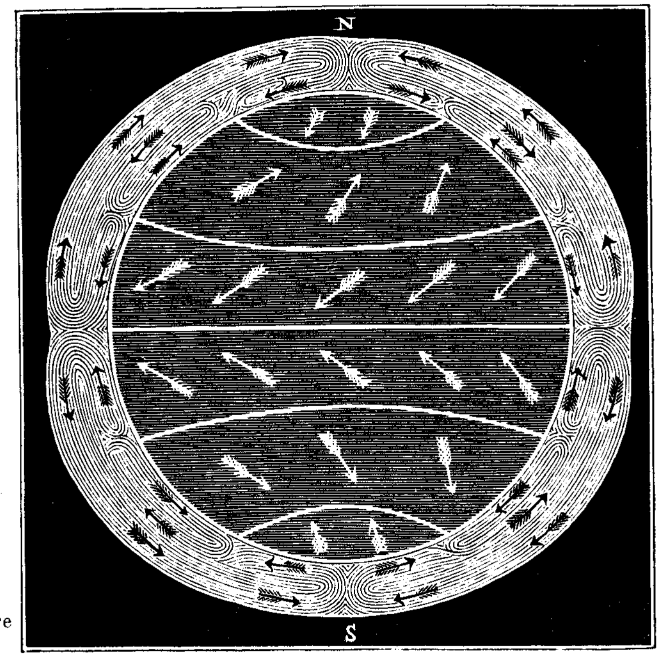


Figure 35. — The general circulation of the atmosphere according to Ferrel (1859)

surface westerlies and must be maintained by the action of the Coriolis force upon a poleward current. He therefore placed the equatorward current at an intermediate level, noting that according to observations it should lie above the fair-weather clouds.

We cannot agree with Ferrel's premise that with internal viscosity but without surface friction the atmosphere would tend to acquire a state of uniform absolute angular momentum. Such a circulation would possess strong internal stresses. Neither does it seem very likely that the ultimate circulation with surface friction would be an attenuated form of the circulation without friction, despite Ferrel's observation that there can be no resistance to motion until there is motion. Whereas Thomson by-passed an explanation of the surface easterlies and westerlies by simply agreeing with Hadley, Ferrel's attempt in this respect yielded no improvement. Beyond this point, Ferrel presented some penetrating arguments, and he used the thermal wind relation to good advantage.

Ferrel's subsequent work led to further modifications, his final scheme (1889) differing slightly from his second one. He was intrigued by the possibility of deriving mathematical expressions for the circulation, but felt that this could not be done because the frictional forces could not be properly formulated. He continually maintained that the circulation must be derived from a knowledge of the temperature field, rather than the field of solar heating, and his system of equations does not contain the thermodynamic equation. It was a great loss to nineteenth-century meteorology that the man who introduced the equations of motion never saw fit to seek a complete solution of them.

The task which Ferrel had regarded as unfeasible was finally attempted in a pair of papers by Oberbeck (1888), who represented the effects of friction by a simple coefficient of viscosity. Like Ferrel, Oberbeck sought to derive the motion from the temperature field, and he did not use the thermodynamic equation. He represented the temperature by a simple analytic function of latitude and elevation.

Oberbeck sought first the circulation which would prevail in the absence of rotation and advection, and the set of equations which he first solved expressed a balance between the effects of friction and the pressure forces. The circulation which he obtained was necessarily entirely meridional, and consisted of a single direct cell. To obtain the next approximation he balanced the east-west Coriolis force, as determined by his first approximation, against friction. The added circulation was entirely zonal and proportional to the Earth's angular velocity Ω, and consisted of low-latitude easterlies and high-latitude westerlies at low levels, and westerlies at all latitudes at high levels.

On the whole his circulation bears considerable resemblance to Hadley's. We feel, however, that this resemblance is fortuitous. In a steady symmetric circulation, the Coriolis force resulting from the net north-south motion in any vertical column is zero. Hence Oberbeck was balancing the frictional drag at the base of each column against the net Coriolis force resulting from the weak vertical currents, and he thus obtained easterlies and westerlies just above the surface, in their proper latitude. In a mathematical description of Hadley's circulation, the frictional drag is balanced by non-linear terms, which Oberbeck had not used at this point.

In his second paper Oberbeck sought the final corrections needed for an exact solution, but since the system of equations governing these corrections was as complicated as the original system, containing all the non-linear terms, he found it necessary to make further approximations. The added circulation was again entirely meridional, and proportional to Ω^2, and consisted of a direct cell in low latitudes and an indirect cell in high latitudes. In essence he had found the first three terms of a power series in Ω. For the value of Ω appropriate to the Earth, the added cell in high latitudes was insufficient to reverse the direction of the original cell, and simply weakened it there while intensifying it in low latitudes.

It is no discredit to Oberbeck that he was forced to stop with the quadratic terms in Ω, yet it must be conceded that on this account his solution is not a particularly good approximation to the exact solution which he sought. His increase of westerly wind speed with height is proportional to Ω, whereas, according to the thermal wind relation, it should be inversely proportional to Ω. It is not at all certain that Oberbeck could have improved his results by computing more terms, since, as noted by Brillouin (1900), there is no assurance that the series would converge. A power-series expansion does not reveal that $\Omega/(1 + \Omega^2)$, for example, becomes small as Ω becomes large.

Oberbeck's work marks the beginning of a new field of endeavour — representing the global circulation by solutions of the dynamic equations, as opposed to using the equations simply to deduce general properties. A mathematical solution is simply one type of description of the circulation, but its advantages are obvious. If the equations have been correctly formulated and correctly solved, with no crippling approximations, the description is assured of being internally consistent in every way, and in particular it will satisfy its own balance requirements.

If more theoretical meteorologists had followed Oberbeck, and had sought actual solutions of the dynamic equations in preference to circulations which could merely be rendered plausible by qualitative arguments based upon the dynamic equations, many of the impossible schemes which were subsequently offered might never have appeared. Still, the idea that manipulation of mathematical symbols ought to replace qualitative reasoning could scarcely have appealed to the many competent meteorologists who were nevertheless not mathematically inclined. When further attempts to solve the equations yielded circulations which were no more realistic than Oberbeck's, this was cited as evidence that the whole procedure was meaningless. The fact that the equations had not really been solved was disregarded.

It must be admitted that even very recent analytic solutions of the equations have had a certain unrealistic flavour. It is only with the advent of numerical solutions by digital computers that the equations have begun to acquire the status which they deserve.

In Oberbeck's work, as in that of most of his predecessors with the notable exception of Dove, the general circulation was treated as being completely symmetric with respect to the Earth's axis. It must not be supposed on this account that the various authors were unaware of the presence of cyclones and other disturbances. Both Ferrel and Oberbeck were deeply concerned with the cyclone problem, and Ferrel often dealt with the general circulation and cyclones in separate sections of the same papers, regarding the cyclone circulations as being much like the general circulation on a smaller scale. Yet nowhere in Ferrel's work is there any suggestion that cyclones owe their origin or subsequent behaviour to the general circulation, or that the general circulation in turn is affected by the presence of cyclones.

The idea that storms were dependent upon the general circulation had been proposed long before Ferrel's time, and it formed an essential part of Dove's work. In modern studies where the field of motion has been analysed into "zonal" and "eddy" components, there is often a tendency to regard all departures from zonal symmetry as having a similar nature, and to refer to them loosely as storms. As an observer of weather phenomena rather than a formalist, Dove distinguished between cyclonic storms on the one hand and the equatorial and polar currents on the other, regarding the storms as originating from disturbances of the opposing currents. It would have meant nothing to him to inquire whether these currents influenced the general circulation; to him they were the general circulation. Indeed, such a question is meaningful only if the general circulation has been defined. A less ambiguous question would ask whether the zonally averaged motions are different from what they would be if departures from zonal symmetry were absent. Dove might have answered this question in the affirmative.

It is remarkable that Dove's rather advanced description of the circulation, which would have been dynamically possible in a dry atmosphere, went completely unmentioned by many subsequent writers (for example Brillouin, 1900) who included thorough treatments of the works of Maury, Ferrel, Thomson, Oberbeck and others in their historical discussions of general-circulation theories. Among those who did mention Dove, Waldo (1893), while presenting extensive reviews of the other works, merely states that Dove made certain modifications of Hadley's theory; he does not even say what these modifications were.

Perhaps the neglect of Dove's work may be traced to his refusal in his later years to accept any of the newer ideas, with the result that all of his work tended to become discredited. Perhaps his work was ignored because he offered only descriptions rather than explanations, although the same criticism could be made of Maury. It seems very likely, however, that most of the writers of the later nineteenth century simply did not consider that the motion of which Dove spoke was the general circulation. The notion that the general circulation meant the time-averaged or the time-and-longitude-averaged circulation had become rather well established, and Dove's currents varied with time and longitude. It is noteworthy that Hann (1901), in what is still one of the most comprehensive meteorological treatises yet produced, makes no mention of Dove in his chapter on the general circulation, but reviews Dove's ideas in detail in the following chapter on storms.

Yet despite the apparent rejection of Dove's scheme, the symmetric theories of the general circulation could not endure forever. Even as Ferrel and Thomson were making their final contributions, specific objections to them were being raised.

One of these was based mainly upon theoretical considerations. Except near the Earth's surface, friction was generally considered to be negligible. It was often pointed out that in the schemes of Ferrel and Thomson the poleward-moving air aloft would acquire unheard-of velocities in middle latitudes, in conserving its absolute angular momentum. Some writers took the attitude that such high velocities, never being observed, could not possibly exist, and that the schemes were dynamically impossible.

Others took the more moderate attitude that the high velocities simply did not exist, and that the schemes, while perhaps possible, were incorrect. In any event the thermal wind relation, adhered to by Ferrel, would not allow such high velocities to occur aloft unless excessively high velocities occurred at the Earth's surface also. The simple scheme of having the poleward current aloft so weak that there would be ample time for even weak friction to reduce the westerlies does not seem to have found favour.

In a remarkable paper the renowned physicist Hermann von Helmholtz (1888) attacked this problem. He had previously (1868) been the first to emphasize that the motion in a fluid need not be everywhere continuous. He began the present work by noting that friction was extremely ineffective in the atmosphere, except at the Earth's surface and at internal surfaces of discontinuity. Clearly, he was here referring to molecular friction. He then noted the large velocities which would be required by a circulation between the Equator and 30° latitude, and maintained that while such a circulation did occur, the large velocities were not found. He thereupon sought the means by which the winds were prevented from attaining them.

In seeking a solution Helmholtz virtually developed a theory of the general circulation. By reasoning somewhat like Ferrel's, he deduced that in the absence of friction there would be easterly winds in low latitudes and westerlies in higher latitudes. He then proceeded to determine how this circulation would be modified by heating and friction. Like Ferrel and Thomson, he found that surface friction would produce a poleward drift in the surface westerlies, and intensify the equatorward drift in the surface easterlies.

In his next step he proceeded beyond the earlier authors. He maintained that the returning air above the trades must come into immediate contact with the cooler and more slowly moving air below, with the formation of a surface of discontinuity. At such a surface the equilibrium would be unstable, so that vortices would form, and ultimately bring about vertical mixing.

In the polar regions he felt that the effect of cooling would outweigh the effect of surface friction, and lead to additional equatorward spreading. Easterly winds would thereby develop, and the resulting friction would cause further spreading. Again a surface of discontinuity would form between this air and the returning air aloft, and vertical mixing would again occur.

He concluded with the opinion that the principal deterrent to stronger winds aloft was not surface friction, but the mixing of layers of different velocities by means of vortices forming on surfaces of discontinuity.

Helmholtz's paper is often regarded as the original statement that cyclones must form upon surfaces of discontinuity, and that these cyclones will in turn alter the general circulation. Admittedly some statements are subject to more than one interpretation, but we do not feel that this is what Helmholtz was saying. The vortices which he visualized seem to have horizontal scales of hundreds of metres, or perhaps a few kilometres, but not thousands of kilometres, and he frequently mentioned billow clouds. He referred to cyclones in only two connexions, in neither case as disturbances on unstable surfaces of discontinuity. He first suggested that they should form in middle latitudes under the masses of ascending air. Later he clearly maintained that the permanent circumpolar anticyclonic motion at the Earth's surface and the cyclonic motion above it should break up into smaller cyclones and anticyclones as a result of surface irregularities, such as mountains. In neither case did he specifically say that these cyclones would affect the general circulation; it is the vortices which develop on the surfaces of discontinuity which were assigned this role. In a second paper (1889) he mentioned that the numerous disturbances should cause the principal surface of discontinuity to break "into separate pieces which must appear as cyclones," but he did not elaborate further.

If Helmholtz did not give the meteorological world the wave theory of cyclones, he gave it the concept of turbulent viscosity, whose presence is an essential feature of every modern theory of the general circulation. It is worth noting in this connexion that if the coefficient of turbulent viscosity is assumed to have a value throughout the atmosphere of from 10 to 100 g cm^{-1} sec^{-1}, a figure frequently quoted for the surface friction layer, the upper-level poleward current cannot exceed from 1 to 10 cm sec^{-1} without giving rise to upper-level westerlies in excess of those allowed by the thermal wind relation.

We therefore feel that Helmholtz's work, far from disproving the ideas of Ferrel and Thomson, tends to support them by showing that the absence of excessively high winds can be accounted for. At the same time, it plainly suggests how departures from zonal symmetry can be important. Ultimately it led the way to the great work of Vilhelm Bjerknes and his co-workers, and to their theories of the general circulation in which cyclones played a fundamental role.

Further objections to the symmetric theories of the general circulation were based upon observations. Routine soundings by balloons were non-existent in the nineteenth century, and many of the ideas concerning upper-level conditions had been deduced from mountain observations. However, as a result of an international conference convened by the International Meteorological Organization in 1891, a decision was made to perform a world-wide investigation of upper-level currents at different levels by observing the typical motions of different forms of clouds. All countries were invited to participate. The proposed programme became a reality in 1896 and 1897 (see Bigelow 1900).

The results of this programme, together with earlier cloud observations at selected stations, formed the basis of an assessment of the theories of the general circulation by Hildebrandsson and Teisserenc de Bort (1900). The authors concluded that the only dynamically acceptable schemes so far proposed were those of Ferrel and Thomson, but they noted that at upper levels these schemes were based upon theory rather than observation. From their cloud study they found that the average winds at the cirrus-cloud level were easterly over the Equator, becoming south-east, south-west, and finally west as the thirtieth parallel north was approached. They found no evidence at all for an upper-level poleward current extending through middle latitudes, and, although recognizing that the high-level motions were observed only when high clouds were present and no other clouds were below them, they nevertheless concluded that the schemes of Ferrel and Thomson were contradicted by the observations, and must be abandoned. They refrained from offering a scheme of their own.

Today we would regard the selection of stations as quite inadequate for eliminating the possibility of an upper poleward current. It is likely, however, that the authors were seeking a current whose strength was similar to that of the trade winds, perhaps a few metres per second, which the cloud observations may have been sufficient to eliminate. Certainly currents as weak as 10 cm sec^{-1} could not have been disproved. Yet modern observations deny the upper current just as surely as they confirm the poleward current near the surface.

Similar conclusions were drawn by Bigelow (1900, 1902), on the basis of the cloud observations over the United States. He noted an almost perfect balance between northerly and southerly wind components at high and intermediate levels and cited this as evidence against all the "canal" theories, as he termed those theories which allowed no variations with longitude. He then offered a scheme of his own, in which the flow at high levels was fairly uniform, but where cold and warm countercurrents from high and low latitudes flowed beside one another, mainly in the lowest three kilometres, and where the interaction of these currents gave rise to cyclones and anticyclones. In this respect his description seems familiarly like Dove's.

Yet Bigelow went considerably beyond Dove, who had been content to present the observations. He proposed that the warm and cold currents were the means by which the required poleward heat transport was accomplished. He moreover identified the cyclones and anticyclones as the means by which the excessively strong winds at upper levels were forestalled; specifically, he noted that the upward currents in the cyclones and the downward currents in the anticyclones would bring about a vertical exchange of momentum.

Bigelow's theory leaves some questions unanswered; there is still no specified mechanism for carrying angular momentum horizontally across middle latitudes, even though the vertical transport is present. Yet not only is the energy balance satisfied, but there is a clear statement of the necessity for a poleward heat transport, and of the mechanism through which it is accomplished. Only the more recent observations have revealed that Bigelow should not have concentrated the irregularities in the lowest kilometres.

By the turn of the century the study of the circulation had undergone a permanent change. A few years earlier the works of Ferrel and Thomson had appeared to offer a nearly complete explanation of the circulation. Now with the increasing realization that the general circulation involved more than the zonally symmetric motions, it became apparent that a full explanation was a far more difficult task than had been supposed. There was even some feeling that the circulation could not be explained at all.

From this time on few of the important new papers attempted to account for the circulation *in toto*. The most significant contributions were often confined to a single aspect. Among these was the further investigation of the role actually played by cyclones and other disturbances.

For a number of years Bigelow's ideas were often quoted but not pursued. A major advance was finally made by A. Defant (1921). In this famous paper Defant introduced the idea that the motion in middle latitudes was simply turbulence on a very large scale. He regarded the cyclones and anticyclones as the individual turbulent elements, by means of which the required amount of heat was transported from low to high latitudes. He also looked upon this large-scale turbulence as the means by which excessive wind speeds aloft were prevented from occurring.

If he had stopped at this point, he would have done no more than repeat Bigelow's ideas in a new language. Instead, he warned that his conclusions could not be considered valid unless they were quantitatively acceptable. Accordingly, he first applied the recently formulated mixing-length theory of turbulence. Assuming a mixing length of 15 degrees of latitude and an average north-south wind component of 3 m sec^{-1}, he found a horizontal Austausch coefficient — the ratio of the transport to the gradient of sensible heat — of 5×10^7 g cm^{-1} sec^{-1}, nearly a million times larger than the Austausch coefficient characterizing smaller-scale turbulence. He then estimated the Austausch coefficient by other procedures, and also found values somewhere near 10^8 g cm^{-1} sec^{-1}.

Choosing different values of the Austausch coefficient ranging from 5×10^7 to 5×10^8, he calculated the temperature distribution which should prevail north of 30°N, assuming the temperature distribution which would prevail in the absence of any horizontal exchange to be known, and he found that a value of 10^8 would lead to temperatures agreeing fairly well with observations. He thereupon concluded that his case for the circulation as a form of turbulence was established.

Whatever the general attitude may have been toward looking upon the circulation as turbulence, Defant's claim that the cyclones and anticyclones accomplished the needed transport of heat seems to have been well accepted. Perhaps it was hard to deny that north winds were colder than south winds. The conclusions which logically follow should have obviated some of the mistaken reasoning which occurred in many subsequent works. If some of the required poleward heat transport is accomplished by contrasting currents at the same level, the amount of heat remaining to be transported by other mechanisms is less than it would otherwise be. The meridional circulation will therefore not by itself transport enough heat to satisfy the balance requirements, and the zonally averaged flow will not by itself be a solution of the dynamic equations. Any attempt such as Oberbeck's to determine a zonally symmetric general circulation must therefore, if correctly completed, disclose a meridional circulation differing from the one actually occurring in the atmosphere. Any attempt to force the symmetric solution to agree with the atmosphere will fail; if it appears to succeed, it will do so only because of some unjustified steps.

The other major contribution of this period concerning the role of cyclones was made by Jeffreys (1926). Unlike any of his predecessors, Jeffreys was concerned with the manner in which angular momentum was conveyed from low to high latitudes, rather than the manner in which it was brought down from high levels after being conveyed to high latitudes. He noted that in the long run Ω-momentum need not be considered, since its net transport is proportional to the net mass transport. Thus he arrived at the conclusion that the net angular-momentum transport was proportional to the product of the eastward and northward wind components.

Assuming a zonally symmetric flow with no meridional motion except within the lowest kilometre, or the friction layer, he found that the amount of angular momentum carried northward across middle latitudes was too small by a factor of at least 20 to balance the angular momentum transferred into the Earth. He concluded that the bulk of the required transport must be accomplished by large-scale eddies, which he identified as cyclones, and which he felt should extend to considerable heights.

There are certain difficulties with Jeffreys's arguments. Because he regarded the winds as essentially geostrophic except in the friction layer, he did not consider the necessary return flow aloft, which is somewhat surprising, since he had carefully used the principle of mass continuity to eliminate the need for considering Ω-momentum. This point was eventually straightened out by Douglas (1931), who noted that if the needed return flow took place at the level of the strong upper westerlies, it would transport angular momentum equatorward and lead to even greater difficulties. He observed that a balance could be achieved if the return flow occurred immediately above the friction layer, with additional poleward flow at high levels, in the manner proposed by Ferrel and Thomson. However, he found no observational evidence for equatorward flow in the lowest four kilometres, and concluded that Jeffreys was correct in deciding that the exchange of angular momentum was carried out by currents lying side by side.

Jeffreys must be given credit for first stating the need for a horizontal angular-momentum transport and for correctly identifying the mechanism through which it is accomplished. Perhaps meteorologists found Jeffreys's notation somewhat unfamiliar, but if they had simply turned to the equations of motion and written the expression for the rate of change of angular momentum, they would have been forced to conclude that there was either a direct meridional cell operating across middle latitudes, or else a correlation between the eastward and northward wind components within latitude circles. If they had believed in an indirect cell in middle latitudes, as many of them did, they would have been forced to accept the latter conclusion. As it was, a generation had to pass before Jeffreys's ideas were generally accepted.

Another aspect of the general-circulation problem which received considerable attention was the explanation for the very existence of departures from zonal symmetry. The problem as to why cyclones exist is fundamental in cyclone theory, but with the realization that cyclones played a role in the general circulation it gained importance in general-circulation theory as well.

A number of writers maintained that a circulation without cyclones was dynamically impossible. Jeffreys believed that he had established the necessity for cyclones, and he is often quoted as having shown that a symmetric circulation is impossible. Actually, in addition to his omission of the return current aloft, his argument is based on the observed structure of the friction layer; at most he could have shown that a symmetric circulation cannot possess a friction layer like the one actually observed.

Shortly thereafter another argument began to appear in the literature. It was pointed out that zonally uniform equatorward or poleward motion would be kinematically possible, but that no zonally uniform eastward or westward pressure gradient could accompany it, since the pressure could not possibly vary in the same sense at all points of the same latitude circle. The argument was generally attributed to Exner (1925).

It is easy to see how such an argument might have arisen at a time when there was excessive reliance upon the geostrophic wind equation, since there certainly can be no zonally uniform equatorward or poleward geostrophic wind, but it is surprising that it should have been so frequently quoted as a proof that zonally symmetric flow is impossible. It is reminiscent of the remark made many years earlier by Dalton. There is however one additional feature: Exner did not make the statement with which he is credited.

Exner like many others before him was interested in explaining the absence of the excessive upper-level winds which conservation of angular momentum would seem to require. He noted that turbulent mixing would be one means of reducing the angular momentum aloft, but he felt that it would probably be insufficient, and he maintained that it was much more likely that east-west pressure gradients would develop and accomplish the same end. He observed that in this case the circulation could not be zonally symmetric.

To say that east-west pressure gradients are needed to maintain the observed flow is quite a different thing from saying that they must be present in any case. Exner did not try to show that asymmetries must exist. His important contribution was the identification of east-west pressure gradients rather than turbulent mixing as the means by which excessive upper-level winds are precluded; this contribution has been generally overlooked as a result of the misinterpretation of his remarks.

In retrospect, it is hard to understand why zonally symmetric flow should have been considered dynamically impossible. If one chooses an initial condition of complete zonal symmetry, the time-dependent solution which develops must remain zonally symmetric. This is of course true only for an idealized uniform Earth, but it was for such an Earth that the arguments had been given. The initial-value approach has become familiar with the advent of numerical weather prediction; possibly it did not often enter the dynamical thinking of an earlier era.

An alternative school of thought maintained that a symmetric general circulation might be possible, but that it would in some way be unstable, and that cyclones would develop. The proper formulation of this idea developed from the work of V. Bjerknes and his collaborators.

The early work of this group was a natural outgrowth of the meteorological work of Helmholtz. Years later, in discussing this work, Bjerknes (1933, p. 784) expressed his doubts that Helmholtz actually regarded cyclones as the disturbances which would form on unstable surfaces of discontinuity, but acknowledged that Helmholtz's work was sufficient to guide his thoughts in this direction. The eventual identification by J. Bjerknes (1919) of an observed cyclone as a wave on the polar frontal surface is a familiar chapter in meteorological history.

Although much of the earlier work was concerned with the cyclone problem, the concept of a cyclone as a disturbance growing upon a pre-existing flow pattern required a consideration of the pre-existing pattern itself. Ultimately it led to a close examination of the general circulation for its own sake.

In one discussion of the general circulation, V. Bjerknes (1921) noted the difficulties involved in deducing a zonally symmetric circulation from pure theory, and based his picture upon a combination of theory and observation; it is shown in Figure 36. He then noted that this circulation appeared to be unstable, and that cyclones should develop, leading ultimately to an unsymmetric general circulation, as shown schematically in Figure 37.

Bjerknes did not apply any specific test for stability, and his conclusion that the symmetric circulation was unstable may have been based upon his conviction that instability must be the cause of cyclones. In any event, he had not yet distinguished between the zonally averaged circulation and the symmetric circulation which would prevail in the absence of disturbances, as he was to do in a later paper.

Somewhat later, in a study of the development of fronts, Bergeron (1928) found it necessary to introduce a model of the general circulation, and he proposed a three-cell meridional circulation somewhat similar to the one which Ferrel had introduced and subsequently discarded. It is shown in Figure 38. It has been widely reproduced, and it seems to mark the beginning of the general acceptance of a three-cell pattern. The fact that it does not by itself satisfy the balance requirements is irrelevant, since Bergeron regarded the superposed eddies as a further essential part of the circulation. The middle-latitude indirect cell has since come to be known as the Ferrel cell.

Somewhat later J. Bjerknes presented a new scheme of the general circulation (V. Bjerknes *et al.*, 1933). He first described a zonally symmetric circulation which could satisfy the energy balance requirements. However he rejected the idea that the actual circulation was symmetric, and proposed an unsymmetric circulation, which is by no means a copy of Dove's of Bigelow's. In this circulation each particle travels in a circuit with an equatorward branch below and a poleward branch above, as in Hadley's theory, thus fulfilling the energy balance requirements, but the circuit is different at different longitudes. As in the symmetric theories, the Coriolis force will tend to deflect the equatorward flow westward and the poleward flow eastward, but the pressure will build up between converging deflected currents and prevent further deflection. Thus, in agreement with Exner's ideas, air can continue to flow poleward without acquiring excessive angular momentum.

Bjerknes did not describe the zonally averaged meridional motion, and went so far as to say that it would be without interest, since it would merely be a statistical average which would be close to zero. It is to be noticed that his subtropical high pressure cells have a typical ENE-WSW orientation. Possibly he had introduced this configuration simply as a result of careful observation of the real atmosphere. Nevertheless, such a pattern leads to the positive correlation between eastward and northward motion which is needed for a poleward transport of angular momentum, and it foreshadows some of the contributions which Bjerknes was to make some years later.

The explanation for the existence of cyclones which we believe to be the correct one was provided in a later paper by V. Bjerknes (1937). Here he concluded that the circulation which would prevail in the absence of departures from zonal symmetry was essentially the one given by Ferrel and Thomson, with a large direct cell occupying most of either hemisphere, and a shallow indirect cell at low levels in middle latitudes. This circulation differs considerably from his earlier picture, which was based mainly upon observations. He then maintained that this circulation was unstable with respect to small zonally unsymmetric disturbances; hence the observed circulation would contain fully developed disturbances, which would assume the form of cyclones and anticyclones. This remarkable paper, published in his seventy-fifth year, makes a fitting culmination to his contributions to this problem.

The most difficult aspect of the general circulation problem is however neither the explanation of the existence of cyclones and other disturbances nor the determination of their role, but the explanation

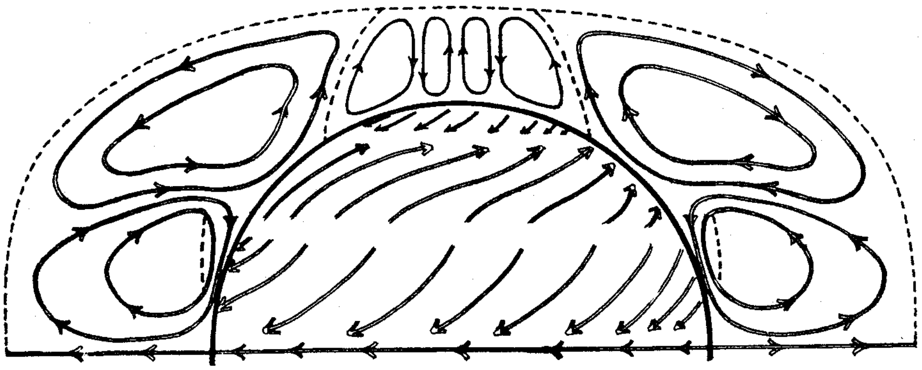


Figure 36. — A schematic representation of the zonally averaged circulation according to Bjerknes (1921)

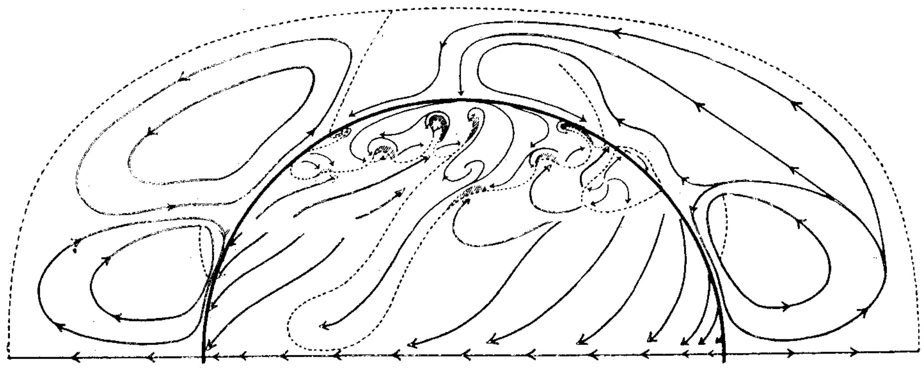


Figure 37. — A schematic representation of the general circulation of the atmosphere according to Bjerknes (1921)

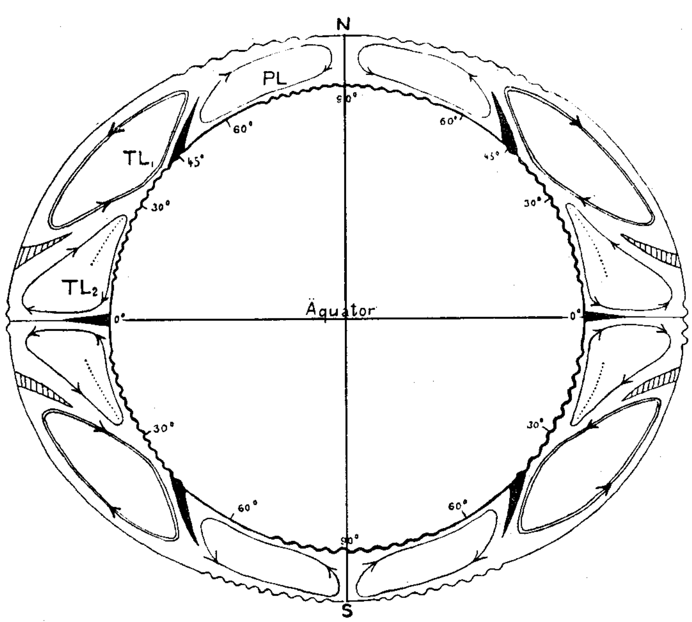


Figure 38. — A schematic representation of the meridional circulation according to Bergeron (1928)

of why cyclones behave as they do, and particularly why they transport angular momentum and energy as they do. Defant's application of turbulence theory seemed to offer a partial explanation for the heat transport, although one may legitimately question whether turbulence theory explains turbulence or merely describes it. Following the contribution of Jeffreys, it was natural to try to explain the angular-momentum transport by turbulence theory also.

In discussing the work of Jeffreys, Douglas (1931) noted that Defant's value of the Austausch coefficient applied directly to the gradient of absolute angular momentum would yield a transport a hundred times greater than that demanded by the balance requirement. He concluded that angular momentum did not diffuse in any such simple manner.

His conclusion was not universally heeded. The fact that turbulence theory appeared to yield the right sign for the transport of angular momentum in most of the atmosphere encouraged workers to pursue the matter, using a smaller Austausch coefficient than mixing-length concepts would seem to have demanded. At the time of Oberbeck's work it had appeared possible to present a complete description of the general circulation in mathematical form by solving the appropriate equations. With the realization that departures from zonal symmetry played a significant role, the possibility seemed more remote. Now, by representing the effects of cyclones and other disturbances through appropriate Austausch coefficients, it might again be possible to work with a system of equations with only latitude and elevation as independent variables.

We must therefore note that no matter how conservative absolute angular momentum may be, classical turbulence will attempt to remove internal stresses by creating a state of solid rotation, which it can do only by transporting angular momentum towards latitudes of lower angular velocity — not towards latitudes of lower absolute angular momentum. For the most part a transport towards lower angular velocity is a transport towards a region of weaker westerlies or stronger easterlies. Angular velocity is certainly not conservative, yet the notion that mixing should somehow lead to a uniform distribution of some conservative quantity is hard to dispel.

The most extensive attempts to apply horizontal-mixing concepts to the general circulation are to be found not in complete solutions of the equations but in the qualitative and semi-quantitative works of Rossby. In these works collectively we find him seeking the proper explanation; he explores one possibility, and, when it proves to be untenable, he turns to another.

The idea of an upper-level poleward current across middle latitudes had by now generally been abandoned, and one of the problems was to explain the strength rather than the weakness of the upper westerlies. In an early paper devoted to the problem, Rossby (1938a) proposed that the westerlies south of the polar front (in the northern hemisphere) could be maintained through large-scale lateral mixing with the supposedly stronger westerlies above the frontal surface, i.e. by being dragged ahead. The presence of an upper-level westerly maximum as far south as 35°N was not yet generally recognized. He assumed a surface frictional drag proportional to the square of the wind speed, and a lateral drag proportional to the square of the horizontal wind shear, and after introducing other simplifications was able to represent the motion in middle latitudes by analytic functions. In his solution the westerlies decreased exponentially with distance south of the polar front.

However his solution did not extend north of the polar front. In view of the proximity of the polar regions to the Earth's axis, it is difficult to see how any surface easterly winds there could be strong enough to supply through friction the angular momentum which in turn was supposed to be supplied to middle latitudes through mixing.

In his best-known treatment of the general circulation, which was addressed to a much wider audience than the meteorological world, Rossby (1941) modified the ideas of his earlier paper by supposing that the westerlies south of the polar front were maintained by mixing with the westerlies farther southward as well as farther northward. Thus he visualized a circulation where the necessary transports of angular momentum were accomplished by large-scale eddies, in the manner proposed by Jeffreys, but he went beyond Jeffreys in relating the transports to mixing concepts.

Not being content with a mere description, he preceded his discussion of mixing by attempting the difficult task of qualitatively deducing the zonally symmetric circulation which would develop from a hypothetical initial state of meridional motion only. He showed that something more complicated than a single direct cell should develop, but his choice of a three-cell pattern seems to have been guided by observations, whereas, as we have noted, the meridional circulation to be expected in the absence of disturbances may not contain three cells at all. The difficulty in following his reasoning at this point has undoubtedly caused many readers to overlook his contributions concerning the role of the eddies.

Still, Rossby could not reconcile the transport of angular momentum from low to middle latitudes, and supposedly from weaker to stronger westerlies, with the ideas of large-scale diffusion. Thus he was ultimately led (1947) to explore the possibility that large-scale lateral mixing is characterized by a transfer of vorticity toward latitudes of lower absolute vorticity, although he carefully noted that small-scale mixing would not have this effect. Specifically, he posed the question as to what distribution of zonal motion should develop in a thin spherical shell under the influence of lateral mixing. Under the assumption that vorticity would be transferred from a reservoir of positive vorticity in high latitudes in the northern hemisphere to a similar reservoir of negative vorticity in the southern hemisphere, he obtained a Pole-to-Pole profile of westerly wind speed which bore a fair resemblance to the observed upper-level winds in the Earth's atmosphere, with easterlies in the equatorial regions and a westerly maximum in middle latitudes in either hemisphere.

Yet the mechanism for the maintenance of this profile can hardly be equated to the mechanism prevailing on Earth, for although the needed source and sink of vorticity may be present in the surface frictional drag of the polar anticyclones, there should be at least equally intense sources and sinks in the sub-polar cyclonic cells and the subtropical anticyclones. Ultimately, Rossby and some of his collaborators were led to the conclusion that some process more complicated than classical turbulence must be present.

Fulfilment of the balance requirements

The modern era in the study of the general circulation begins with the proposals by Starr (1948), Bjerknes (1948), and Priestley (1949) that routine upper-level observations should now be plentiful and accurate enough for direct evaluation of the transport of angular momentum. Such computations might settle the question of the relative importance of the eddies and the meridional circulations. It must be noted that the ideas which Jeffreys (1926) had presented some twenty years earlier had become fairly well known but were by no means universally accepted. Starr and Bjerknes both expressed the opinion that the angular-momentum transport across middle latitudes would prove to be accomplished mainly by the eddies, as Jeffreys had maintained.

Starr observed that the required northward transport could be produced by troughs and ridges with a general NE-SW orientation. As Bjerknes also noted, elongated quasi-elliptical anticyclones with their major axes oriented WSW-ENE could produce the same effect in lower latitudes. Priestley also considered the transports of water and sensible heat, and illustrated his proposal with computations from two years of upper-level data at Larkhill, England. A few words about single-station computations are in order.

We have noted that the field of northward motion may be resolved into a meridional circulation or meridional cells and superposed eddies, and that the meridional cells may be resolved into standing or time-averaged meridional cells and superposed transient or instantaneous meridional cells, while the eddies may be resolved into standing eddies, which appear on time-averaged maps, and superposed transient or migratory eddies. The resolution is given by

$$v = [\bar{v}] + [v]' + \bar{v}^\star + v^{\star\prime}, \tag{91}$$

which follows from (80). The long-term northward transport of any quantity X may then be resolved into the amounts accomplished by the separate components of v; thus

$$[\overline{X v}] = [\overline{X}]\,[\bar{v}] + [\overline{\overline{X}]'\,[v]'} + [\overline{X}^\star \bar{v}^\star] + [\overline{X^{\star\prime} v^{\star\prime}}]. \tag{92}$$

For brevity we may refer to the separate modes of transport as the standing-cell transport, transient-cell transport, standing-eddy transport, and transient-eddy transport.

Any long-term poleward mass flow past a single station will carry angular momentum, water, and energy with it, and thereby contribute to the poleward transports of these quantities, but this contribution will be largely cancelled by the necessary long-term equatorward mass flow at some other station. The observations at the first station alone cannot reveal the extent to which it is cancelled. Thus, as Priestley noted, the standing-eddy transport cannot be estimated from data at one station only.

Priestley recombined the terms in (92) representing the transient-eddy transports; thus

$$[\overline{Xv}] = [\overline{X}]\,[\bar{v}] + [\overline{X}^\star v^\star] + [\overline{X'v'}]. \tag{93}$$

He estimated the final term in (93), which he regarded as the transport by the transient eddies, by assuming the covariances $\overline{X'v'}$ to be independent of longitude. He also estimated the standing-cell transport by assuming the departure of $[\bar{v}]$ from its vertical average to be independent of longitude. He concluded that both the meridional circulation and the eddies were important in effecting the required transports. He furthermore found no indication that the eddy transports were in agreement with mixing-length concepts.

Priestley's main objective in performing the computations with data from one station was to demonstrate the feasibility of global computations, rather than to obtain definitive measurements. More recent studies have shown that the statistical properties of the transient eddies vary considerably with longitude. Nevertheless, Priestley's results are sufficient to indicate that the eddies may play an important role.

The first transport computations extending around the globe were performed by Widger (1949), who used data for the single month of January 1946. At that time upper-level wind coverage was still not plentiful, and Widger used geostrophically estimated wind components, obtained from analysed sea-level, 700-mb, and 500-mb northern-hemisphere maps, at the intersections of standard latitudes and longitudes. This procedure automatically eliminates the transport by the meridional cells, which are entirely non-geostrophic. Widger obtained angular-momentum transports of the proper sign and order of magnitude to satisfy the balance requirements, with the major contribution coming from the 500-mb observations.

Hemispheric computations extending through the depth of the troposphere were first carried out by Mintz (1951), who used geostrophic-wind data for January 1949 extending up to 100 mb. Analysed maps above 300 mb were still a rarity, and Mintz's computations had to be preceded by a painstaking analysis of a series of upper-level maps. Mintz found that the angular-momentum transport occurring near the tropopause far outweighed the transport below 500 mb, with the jet stream apparently playing a major role.

Still, the results were not entirely convincing. The computed correlations between u and v were rather low, generally between $+ 0.1$ and $+ 0.2$, and small but systematic departures from the geostrophic wind might conceivably have reduced them to zero. The results were especially unconvincing to those who held to mixing-length concepts, since south of the westerly-wind maximum the transport was toward stronger westerlies. In any event there were no direct computations of the cell transport for comparison.

Convincing evidence of the importance of large-scale eddies in the angular-momentum balance was finally provided by Starr and White (1951), who used observed winds at a chain of sixteen stations extending around the globe in the vicinity of 30°N. They found a large poleward transport by the eddies, and such a small transport by the meridional circulation that even its sign was in doubt.

Meanwhile the eddies were proving to be important in the transports of other quantities. Since Defant's famous paper it had been generally accepted that the eddies could transport heat, but White (1951) found that geostrophically measured sensible-heat transports agreed well with the balance requirements. Benton and Estoque (1954) performed a detailed study of the flux of water vapour over North America and the adjacent oceans, and found that the hemispheric eddy transport of water vapour, as estimated from this quadrant, was sufficient to satisfy the balance requirements at these latitudes.

Subsequently Starr and White extended their computations to chains of stations at other latitudes, and finally (1954) combined these computations, along with similar ones for the transports of water and sensible heat, into a complete hemispheric study. With their computation procedure it was feasible to distinguish between the transient meridional circulation and the transient eddies, but not between the standing eddies and the transient eddies; thus their form of equation (92) was

$$[\overline{Xv}] = [\overline{X}]\,[\overline{v}] + \overline{[X]'\,[v]'} + [\overline{X^{\star} v^{\star}}]\,. \tag{94}$$

Virtually all the transport of angular momentum proved to be accomplished by the eddies, except at the southernmost latitude, 13°N, where the meridional circulation also gave an important contribution.

With the importance of the large-scale eddies firmly established, the primary purpose of subsequent transport computations became the determination of more appropriate numerical values. The most extensive collection of these computations appears in the works of the Planetary Circulations Project (formerly the General Circulation Project) at the Massachusetts Institute of Technology, which has been continually directed by V. P. Starr. Some of the earlier works of this project have already been mentioned (Widger 1949, White 1951, Starr and White 1951, 1954). The author considers himself fortunate to have been associated with this project since its inception in 1948, and is pleased to take this opportunity to present its most recent estimates of the various transports.

In the newer computations the transports have been evaluated by a uniform procedure first used by Buch (1954) in the study mentioned in Chapter III. In this study Buch used all available upper-wind data for the year 1950, at a network of 81 stations over the northern hemisphere. For each of these stations, at each of the six pressure levels 850, 700, 500, 300, 200, and 100 mb, he evaluated the statistics $\bar{u}$, $\bar{v}$, and $\overline{u'v'}$, the time-averaging being for the whole year. He also evaluated $\bar{u}$ and $\bar{v}$ separately for the summer and winter halves of the year, and computed certain other statistics, including standard deviations of u and v.

He then constructed a map of each statistic for each level, by recording the computed statistics on the map and drawing isopleths. From the analysed maps he interpolated the values of the statistics at the intersections of standard parallels and meridians, and then summed over longitude to obtain estimates of $[\bar{u}]$, $[\bar{v}]$, and $[\overline{u'v'}]$, and hence the separate terms in equation (93), the form used by Priestley. He then multiplied the terms by the appropriate latitude factor to obtain his estimates of the angular momentum transport by the meridional cells, standing eddies, and transient eddies.

The map analyses were necessarily subjective. With an average of only one station per three million square kilometres, there were some large continuous regions with no data at all. Only pilot-balloon observations were available between 60°E and 110°E, and the 200-mb and 100-mb maps could not be analysed in that sector. In fact, Buch regarded his procedure as experimental. Since that time upper-level wind measurements have become routine at many more stations, and it would be possible today to perform a similar study with several hundred stations, although some large areas with scanty data still remain.

A similar study for the southern hemisphere using 1950 data would have been out of the question, but with the inauguration of the International Geophysical Year a reasonable number of stations became established. Using all the available upper-wind data for the year 1958 at a network of 145 stations, Obasi (1963) applied the computational procedure used by Buch to the southern hemisphere.

Figure 39 compares the annual mean northward transport of angular momentum, as computed by Buch and Obasi, with the balance requirement as shown in Figure 23. In view of the various approximations involved in both curves, the agreement is remarkable. The feasibility of computing angular-momentum transports directly would appear to have been established.

Figure 40 compares the annual mean eddy transport of angular momentum with the meridional-cell transport. The general predominance of the eddies is evident; the computed eddy transport alone fulfils the estimated balance requirement as closely as does the total computed transport. It is gratifying to find that the computed cell transport conforms to the three-cell pattern in each hemisphere, but beyond this result very little reliance can be placed in the estimated values. The data are simply not adequate to give a reliable picture of the meridional circulation.

Obasi determined the transports separately for each season; the average of these is shown. Buch computed the transports only for the whole year, but from summer and winter values of $[\bar{u}]$ and $[\bar{v}]$ we have determined the cell transport for each season, and have shown the average of these. Without this modification the Hadley cell, whose position shifts with the seasons, would appear mainly as a transient meridional circulation, and the computation procedure would include the transport which it accomplishes as part of the transient-eddy transport.

Figure 41 shows the vertical distribution of the horizontal eddy transport of angular momentum. The most conspicuous feature is the extreme concentration near 200 mb and 30° latitude in either hemisphere, suggesting the great importance of the jet streams in maintaining the balance of angular momentum. Nearly half of the total transport occurs within a 200-mb layer. Both Buch and Obasi also determined the transient-eddy and standing-eddy transports separately. In the northern hemisphere the latter also shows a concentration near the region of maximum westerlies, and accounts for about twenty per cent of the total eddy transport. In the southern hemisphere, where geographical influences tend to be less pronounced, the standing-eddy transport is weaker and less regularly distributed.

Comparing Figure 41 with Figures 1-8, we see that throughout approximately half the atmosphere — the tropics and the lower temperate latitudes — the eddy transport of angular momentum is directed

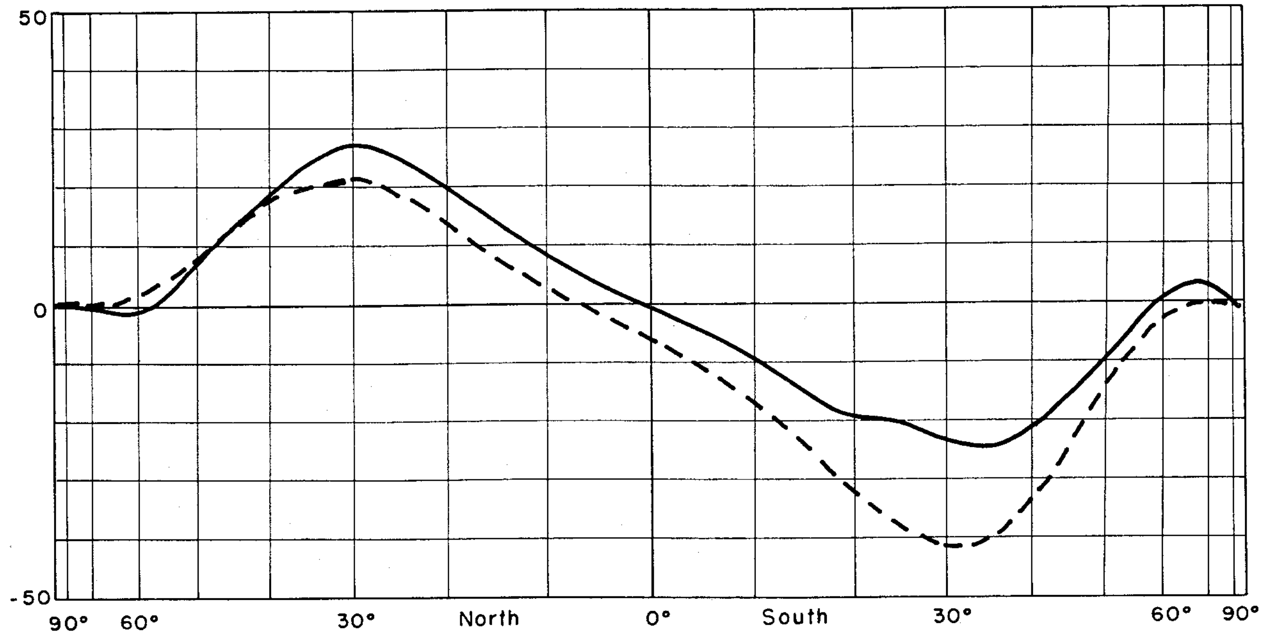


Figure 39. — The average northward transport of angular momentum (solid curve) as estimated by Buch (1954) (northern hemisphere) and Obasi (1963) (southern hemisphere), and the required transport (dashed curve) as given in Figure 23. The unit is 10^{25} g cm^2 sec^{-2} (scale on left)

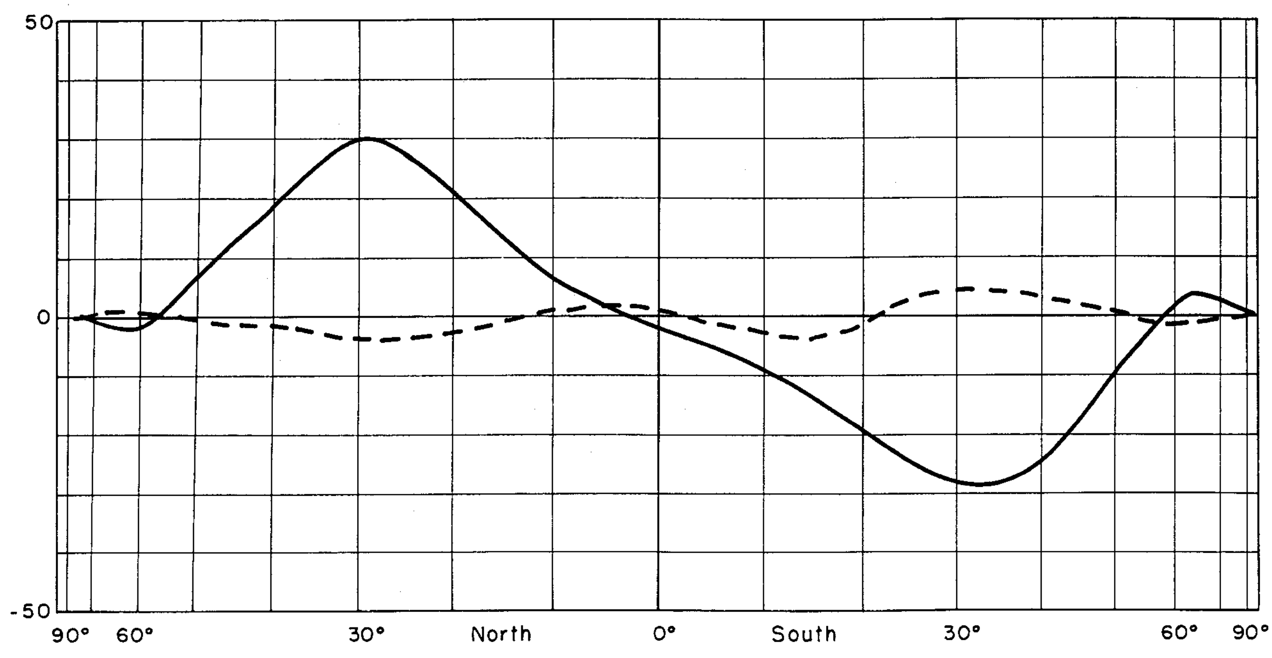


Figure 40. — The average northward transport of angular momentum by the eddies (solid curve) and by the meridional circulation (dashed curve) as estimated by Buch (1954) (northern hemisphere) and Obasi (1963) (southern hemisphere). The unit is 10^{25} g cm^2 sec^{-2} (scale on left)

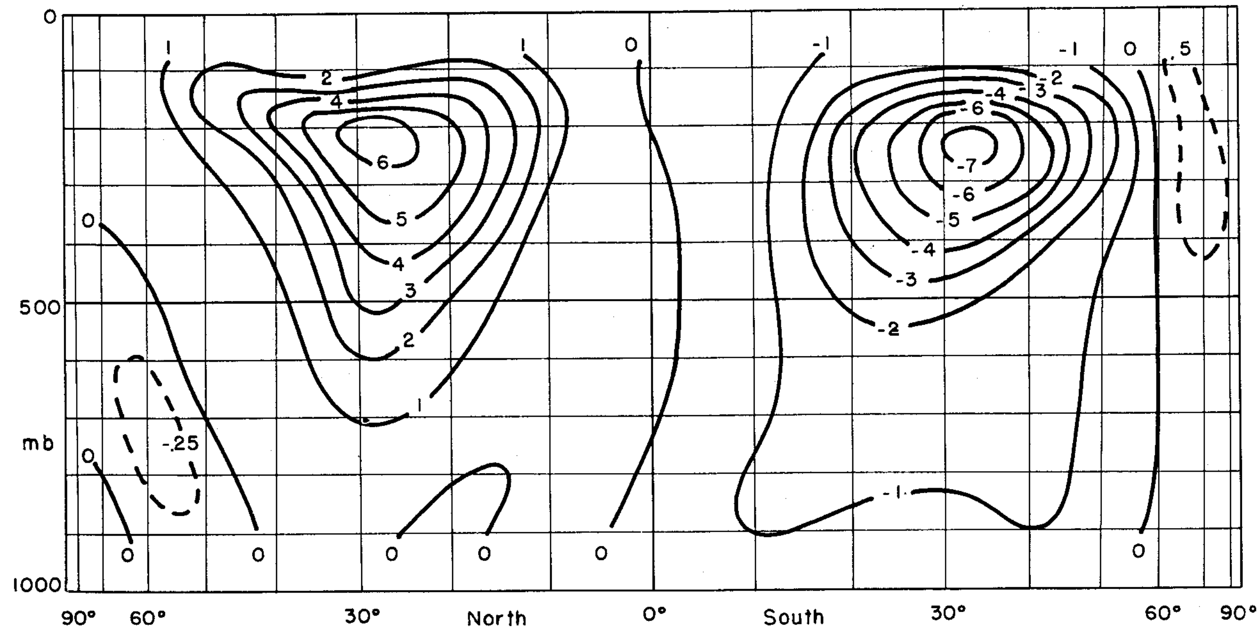


Figure 41. — The vertical distribution of the average northward transport of angular momentum by the eddies as estimated by Buch (1954) (northern hemisphere) and Obasi (1963) (southern hemisphere). The unit is 10^{25} g cm^2 sec^{-2} per 100-mb layer

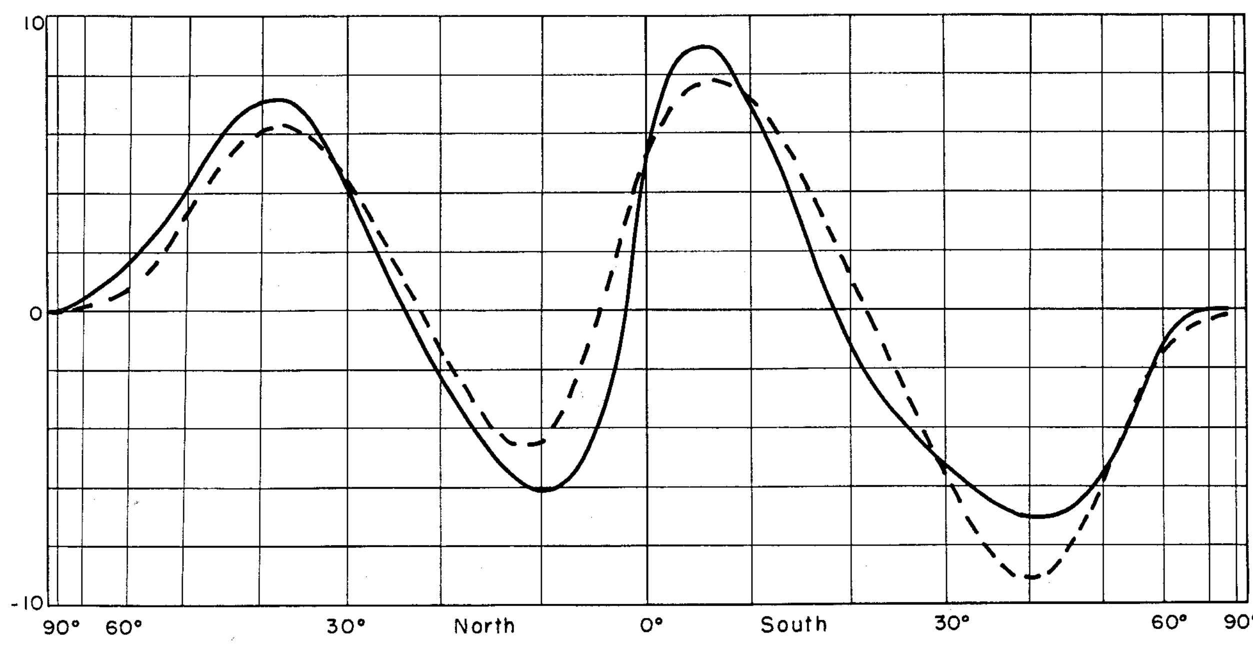


Figure 42. — The average northward transport of water (solid curve) as estimated by Peixoto and Crisi (1965) (northern hemisphere) and Peixoto (southern hemisphere), and the required transport (dashed curve) as given in Figure 21. The unit is 10^{11} g sec^{-1} (scale on left)

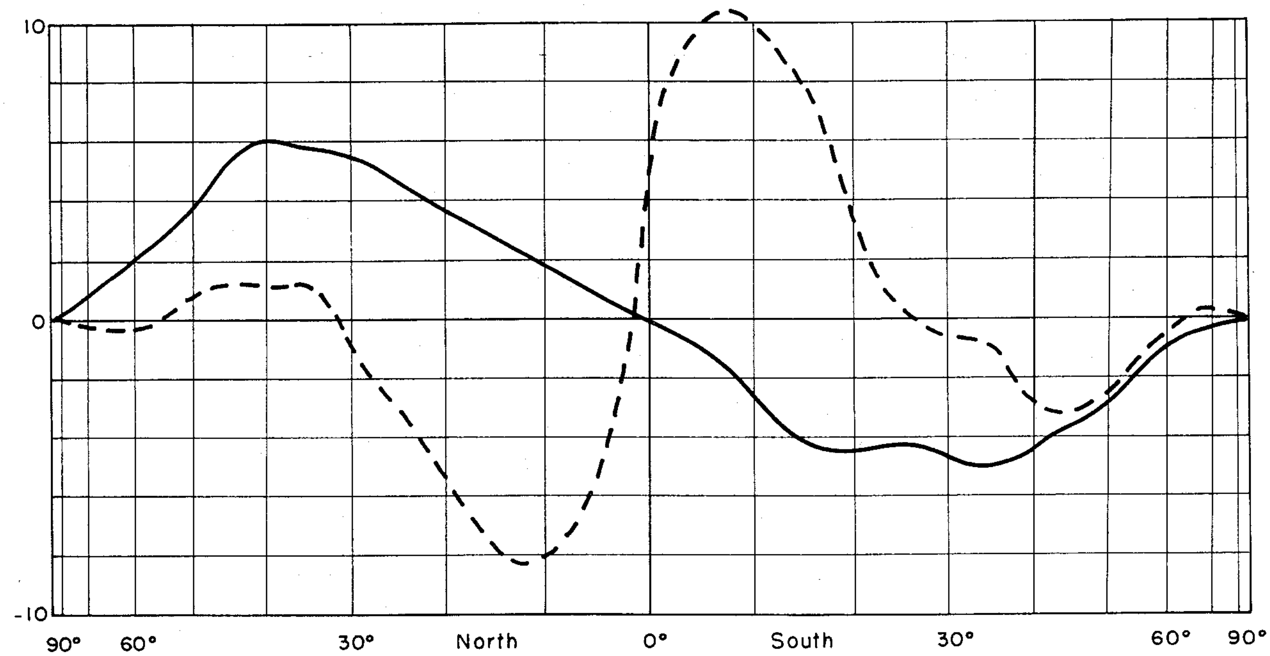


Figure 43. — The average northward transport of water by the transient eddies and transient meridional circulation (solid curve) and by the standing eddies and standing meridional circulation (dashed curve) as estimated by Peixoto and Crisi (1965) (northern hemisphere) and Peixoto (southern hemisphere). The unit is 10^{11} g sec^{-1} (scale on left)

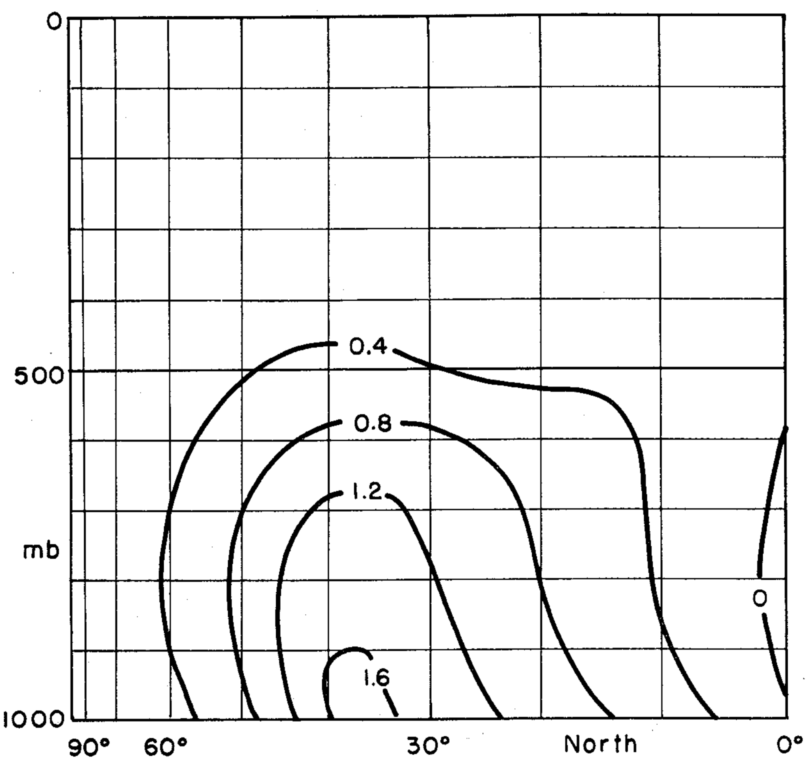


Figure 44. — The vertical distribution of the average northward transport of water by the transient motions as estimated by Peixoto and Crisi (1965). The unit is 10^{11} g sec^{-1} per 100-mb layer

toward latitudes of higher angular velocity. The weaker equatorward transport in the polar region is also countergradient. This is precisely opposite to what would be predicted by ordinary mixing-length theory. To obtain the right result, one would have to assume a negative eddy viscosity. Yet choosing a negative coefficient of viscosity for the entire atmosphere would not yield much improvement, since in the temperate latitudes the transport is directed toward latitudes of lower angular velocity. We must conclude that we are dealing with a phenomenon quite different from classical turbulence.

The water balance has been investigated by the same procedure. The computations were first carried out by Peixoto (1958) with the 1950 data, and the procedure was subsequently repeated by Peixoto and Crisi (1965) with the vastly more complete data for 1958, when 321 stations were available. Very recently, in a study not yet published, Peixoto has extended the computations to include the southern hemisphere. Fig. 42 compares the computed northward transport with the balance requirements as shown in Figure 21. Again the agreement is remarkably good at most latitudes.

Figure 43 compares the transient-eddy transport of water with the cell transport. As in the case of angular momentum, the transient eddies predominate in middle latitudes, but in the tropics the situation is quite different. Actually Peixoto and Crisi did not separate the standing-eddy transport from the cell transport, but the strong equatorward transport in the tropics appears to be due to the Hadley cell, whose lower branch is concentrated near the surface where the water vapour content is high. In fact, the entire curve is consistent with a three-cell pattern.

Figure 44 shows the vertical distribution of the horizontal transient-eddy transport of water. A notable feature is the appreciable transport at 700 mb, despite the concentration of water vapour closer to the surface. Contrary to the case of angular momentum, no countergradient flow is evident.

In considering the energy balance we should note that only the meridional circulation can transport potential energy, since the latter varies only with elevation. The direct transport of kinetic energy by either the meridional circulation or the eddies appears to be rather small. The transport of latent energy is proportional to the water transport. There remains the transport of sensible heat.

Peixoto (1060) has evaluated the eddy sensible-heat transport, again using the data for 1950. He did not compute the cell transport of either sensible heat or potential energy, since the data were quite inadequate for directly evaluating the meridional circulation.

It is nevertheless possible to estimate the long-term meridional circulation by an indirect procedure, which we shall presently describe, which makes use of previously determined values of the eddy transport of angular momentum. We have carried out the procedure, using Buch's transport values as shown in Figure 41, to estimate the meridional circulation (shown in Figure 50). We have then computed the transports of sensible heat and potential energy accomplished by this meridional circulation, using Peixoto's temperatures.

For the latter computations it is convenient to use the stream function Ψ for the mass flow. With the aid of (82) we find that the transport of any quantity X by the meridional circulation is given approximately by

$$\int_0^{p_0} 2\pi a \cos\varphi_1 \, [X] \, [v] \, g^{-1} \, dp = \int_0^{p_0} [X] \, \partial\Psi / \partial p \, dp \,. \tag{95}$$

To cope with the difficuty which arises because potential energy becomes infinite at the top of the atmosphere, we note that

$$\int_0^{p_0} [gz]\,(\partial\Psi/\partial p)\,dp = \int_0^{p_0} [RT]\,(\Psi/p)\,dp\,. \tag{96}$$

We have assumed for computation that Ψ/p remains constant above 100 mb, as it would if the northward velocity above 100 mb were uniform.

Figure 45 compares the computed total transport of sensible heat and potential energy with the balance requirement, as shown in Figure 29, while Figure 46 compares the eddy transport of sensible heat with the cell transport of sensible heat plus potential energy. Peixoto computed the standing-eddy transport for winter only; in preparing the figure we have assumed the standing-eddy transport in summer to be half as large. The meridional circulation could not be evaluated south of 10°N, but we have assumed that the Hadley cell terminates at 2°N, where Peixoto found the water transport by the cell to vanish. The transport of water across 10°N by our meridional circulation, incidentally, agrees very closely with Peixoto's value.

As in the case of the water transport, the eddies dominate in high latitudes while the Hadley cell dominates in the tropics. Although the curves in Figure 45 have certain features in common, the general agreement is no better than fair. Certainly one year of data with less than 100 stations is inadequate for transport measurements. Nevertheless, the task of determining heat exchanges between the atmosphere and the Earth is a difficult one, and the major discrepancy near 30°N might also be due to inadequate estimation of the balance requirement.

Figure 47 shows the vertical distribution of the horizontal eddy transport of sensible heat. In addition to the pronounced concentration near the surface, there is a secondary maximum in the upper

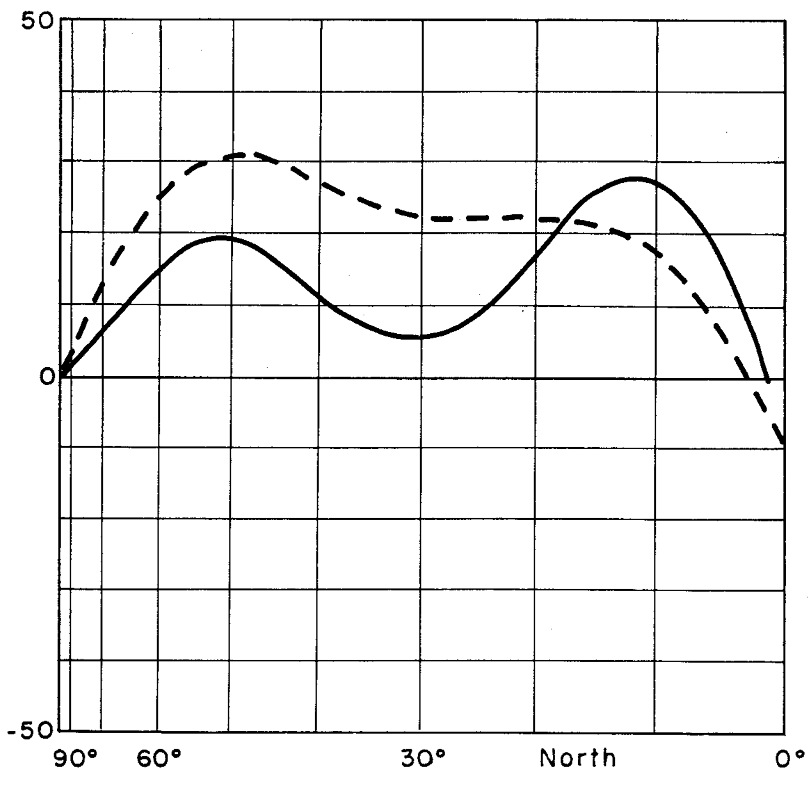
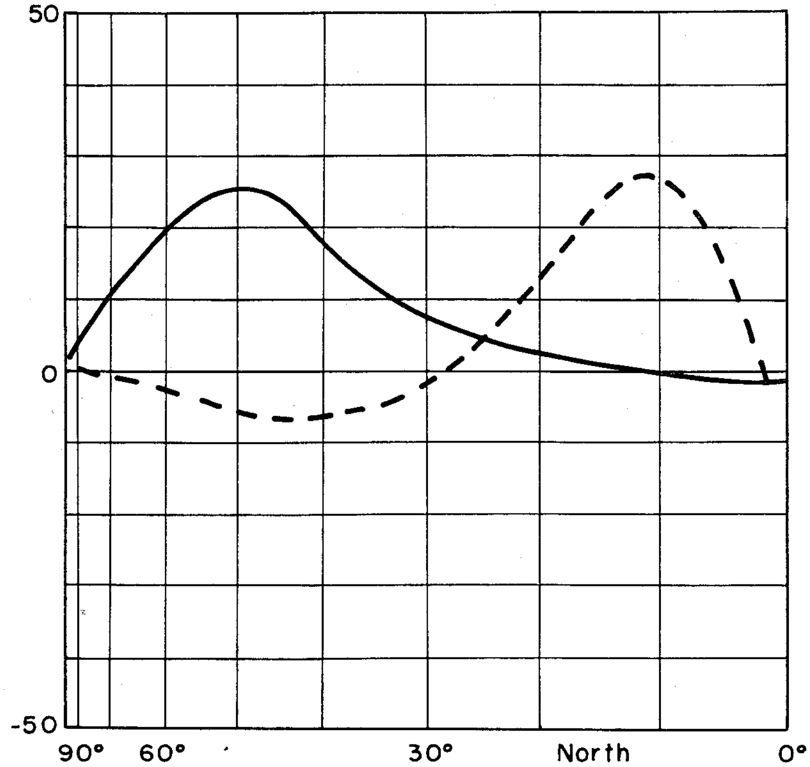


Figure 45. — The average northward transport of sensible heat plus potential energy (solid curve) as given by the sum of the curves in Figure 46, and the required transport (dashed curve) as given in Figure 21. The unit is 10^{14} watts (scale on left)

Figure 46. — The average northward transport of sensible heat by the eddies (solid curve) as estimated by Peixoto (1960), and the average northward transport of sensible heat plus potential energy by the meridional circulation (dashed curve) as determined from the meridional circulation given in Figure 50 and the temperature field estimated by Peixoto (1960). The unit is 10^{14} watts (scale on left)



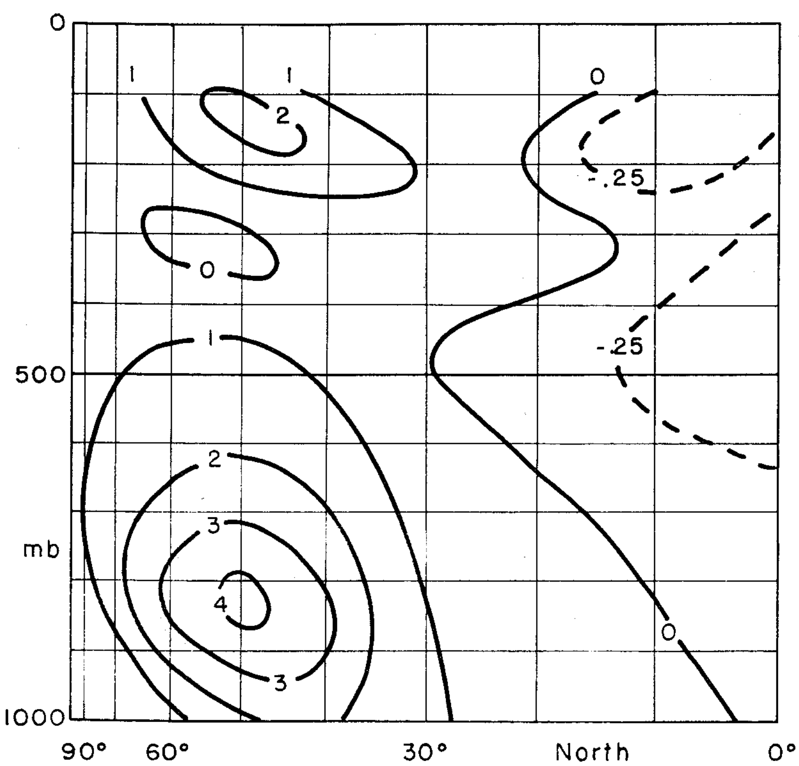


Figure 47. — The vertical distribution of the average northward transport of sensible heat by the eddies as estimated by Peixoto (1960). The unit is 10^{14} watts per 100 mb layer

troposphere. An outstanding feature is the countergradient flux throughout middle latitudes in the lower stratosphere, previously detected by White (1954) from geostrophic computations. There is also a decided countergradient flux in the tropics in the middle troposphere. These fluxes are further indications of the inadequacy of mixing-length concepts in explaining the transport processes. We are led to ask whether it is simply coincidence that turbulence theory yields the proper sign for the sensible-heat transport as often as it does.

Unlike some of the earlier studies, the ones we have just described make no use of geostrophically estimated winds. In this respect they have the obvious advantage of not depending upon an approximation which, while rather good on a point-by-point basis, could yet lead to systematic errors in the correlation of v with u, T, or q. Nevertheless, observed-wind studies seem to be more seriously affected by missing data than geostrophic-wind studies. At nearly every station some observations are missing at higher levels. As noted in the previous chapter, one of the principal reasons for missing upper-level reports is strong winds, which carry the balloon beyond the range of the observing instrument. Thus most collections of upper-level observed-wind data are biased in favour of light winds.

This bias affects the computed transports of angular momentum most seriously, since the missing observations are likely to possess extreme values of u, the transported quantity, as well as v. Recently Priestley and Troup (1964) investigated the effect of this bias by evaluating $\overline{u'v'}$ at a few stations where there were virtually no missing observations, and then determining how these values would have been altered if a few observations with the strongest winds had been missing. They found that omission of even ten per cent of the observations could drastically alter the computed value of $\overline{u'v'}$, and perhaps even reverse the sign. Their study incidentally reveals the importance of the jet stream in accomplishing the necessary transport.

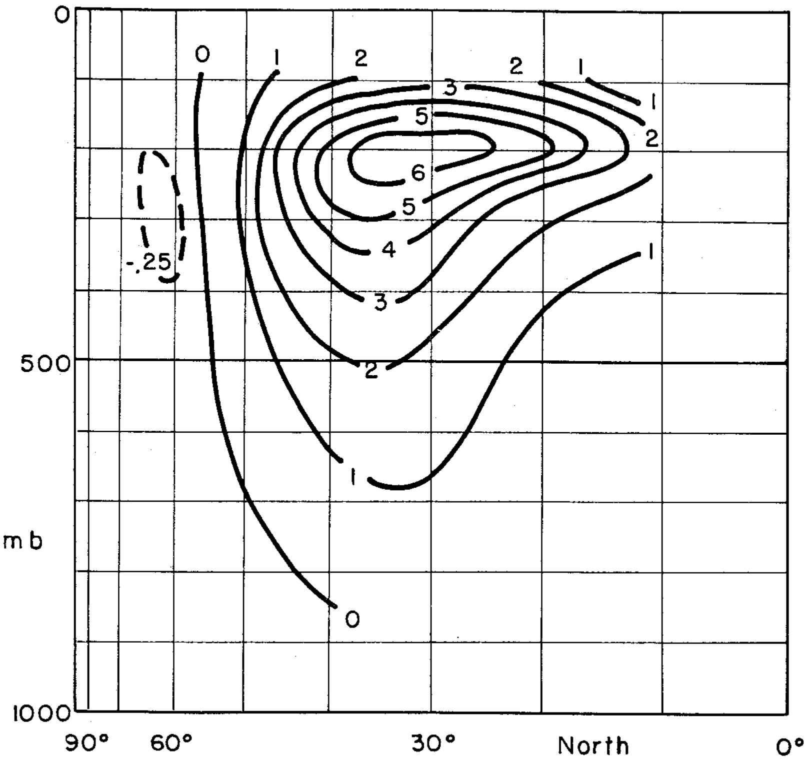
We shall therefore compare the computed transports of angular momentum with those of other investigations which may be less subject to the light-wind bias. The study using the largest sample of data is the recent one by Holopainen (1966); it is again based upon the charts compiled by Crutcher (1959). As we have noted, these charts include maps of $\bar{u}$ and $\bar{v}$ and the standard deviations of u and v at six levels; they also include maps of the correlation between u and v. From the correlations, $\overline{u'v'}$ and hence the transient-eddy transport of angular momentum may be computed, while the standing-eddy transport may be determined from the maps of $\bar{u}$ and $\bar{v}$.

Figure 48 shows the eddy-transport of angular momentum as determined by Holopainen; it is to be compared with Figure 41 showing Buch's values. There is rather good agreement; the principal difference is that Holopainen's extreme values are noticeably farther north.

Crutcher's charts were based upon observed winds wherever these were plentiful, and gradient winds where observations were scarce. On this account they may be subject to a light-wind bias, but to a lesser extent than Buch's. The computations which are least subject to the light-wind bias are those of Mintz (1955), in which all the winds have been geostrophically estimated. Figure 49 shows Mintz's results; the data are for two winter and two summer months in 1949. Again there is good qualitative agreement. Aside from the noticeably larger values, the chief feature is the absence of the much weaker transport at 100 mb than at 200 mb which appeared in the other studies. It is difficult to say whether this discrepancy results from the light-wind bias, which should be greatest at highest levels, from the use of the geostrophic-wind approximation and the inadequacy of the 1949 data, or simply because the studies used data from different years.

The principal discrepancy between the estimated transports and the estimated balance requirements occurs in the case of the energy balance (see Figure 45). Mintz (1955) obtained a curve for the eddy

Figure 48. — The vertical distribution of the average northward transport of angular momentum by the eddies as estimated by Holopainen (1966). The unit is 10^{25} g cm^2 sec^{-2} per 100 mb layer



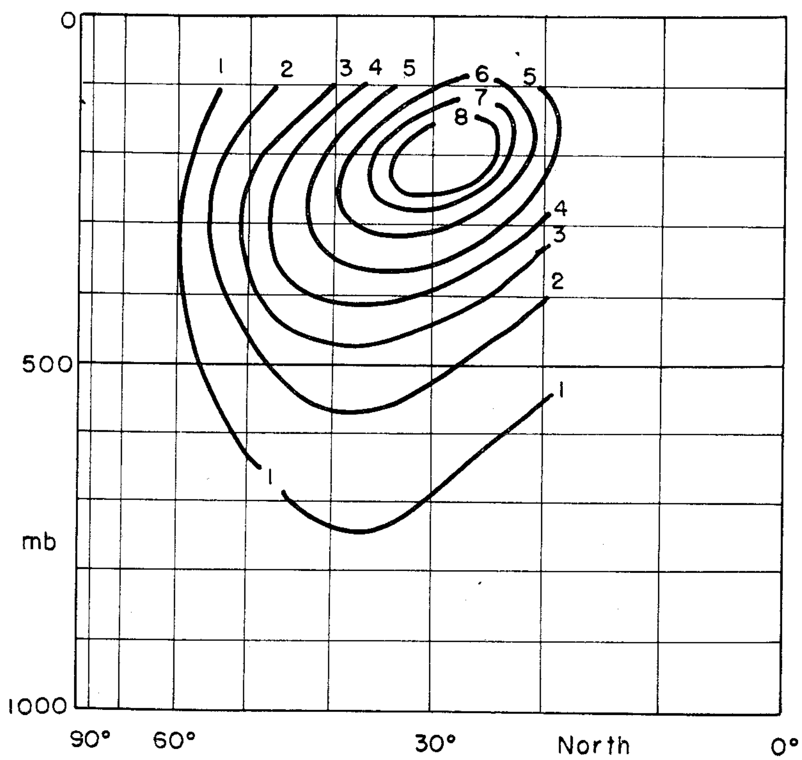


Figure 49. — The vertical distribution of the average northward transport of angular momentum by the eddies as estimated by Mintz (1955). The unit is 10^{25} g cm^2 sec^{-2} per 100 mb layer

transport of sensible heat looking very much like Peixoto's, but generally 20 per cent higher; it indicates a transport of 10×10^{14} rather than 8×10^{14} watts across 30°N. Starr and White (1954) obtained a value of 12×10^{14} watts. Likewise, the Hadley circulation deduced by Holopainen (1966) from the momentum transports shown in Figure 48 extends somewhat north of 30°N, and yields a transport of $+3 \times 10^{14}$ rather than -2×10^{14} watts. Saltzman *et al.* (1961) have estimated the direct transport of kinetic energy on the basis of 500-mb data only; it appears sufficient to add 1×10^{14} watts, and possibly more if the actual transport is concentrated near the tropopause. Nevertheless, it seems likely that the balance requirement of 22×10^{14} watts may have been overestimated; if the radiation balance and the water balance have been correctly estimated, a larger amount of energy would then be left to be transported by the oceans. Much closer agreement than indicated in Figure 45 has also been obtained by Holopainen (1965) and Palmén and Newton (1967).

The general agreement among most of the computed values is encouraging, but quantitatively we cannot regard the present pictures of the angular-momentum, water, and energy balances as the final word. When more densely distributed and more complete data finally become available, some of our estimates may be altered by as much as fifty per cent.

The vertical transports

The foregoing computations show that throughout middle and higher latitudes the required horizontal transports of angular momentum, water, and energy are accomplished mainly by large-scale eddies — the systems which were missing in the early theories of the general circulation. Only in the tropics does the meridional circulation play a dominant role. There remains the question of the vertical transports. In this respect the meridional circulation assumes an added importance.

The balance of angular momentum presents the most straightforward problem. Although the atmosphere gains or loses angular momentum only through direct contact with the ground, the horizontal transport needed to balance the exchange with the ground takes place mainly in the upper troposphere. There must therefore be a vertical transport of angular momentum within the atmosphere across intermediate elevations. This transport would appear to be upward in the tropics and downward in middle latitudes, with a slight upward flow again in the polar regions.

Here a word of caution is necessary. Angular momentum, as we have noted, may be expressed as the sum of Ω-momentum and relative momentum. In evaluating the total horizontal transport, we may disregard Ω-momentum. If, however, either the vertical transport or the vertical variation of the horizontal transport is of interest, Ω-momentum cannot be disregarded when even a weak meridional circulation is present.

Consider, for example, a direct Hadley cell confined between the Equator and 30°N. Such a cell would transport no momentum across 30°N, and would therefore not alter the need for an upward transport within the tropics. However, because Ω-momentum per unit mass decreases with increasing latitude, the vertical motion would carry a larger amount of Ω-momentum upward between the Equator and 15°N than downward between 15°N and 30°N. A cell of sufficient strength could therefore accomplish the required total upward transport. At the same time the horizontal motion would carry an intermediate amount of Ω-momentum northward across 15°N above 500 mb, and a similar amount southward across 15°N below 500 mb. The net effect would therefore be an increase of angular momentum above 500 mb, and a decrease below 500 mb, at all latitudes within the cell. Effectively, then, the cell would carry absolute angular momentum upward at all latitudes. By such means, a direct cell could accomplish a large part of the needed vertical transport at each latitude in the tropics. Likewise an indirect cell could serve a similar purpose in middle latitudes. The middle-latitude cell could be weaker, since the poleward gradient of Ω-momentum is greater there. Such cells would of course carry relative angular momentum at the same time.

The direct computation of vertical transport by the eddies, using observed vertical velocities, is impossible because vertical velocities are not observed on a global scale, even if we accept the vertical velocity deduced through the equation of continuity from the horizontal divergence as being observed. Estimates of the vertical velocity field are therefore necessarily indirect, and are based upon observations of the more readily observed non-divergent horizontal velocities and temperatures.

Three methods of deducing the vertical velocity are feasible. First there is the "adiabatic" method, based upon the thermodynamic equation. Here the field of potential temperature is assumed to be altered only by horizontal and vertical advection. The fields of horizontal advection and local change are observed, and the vertical velocity needed to achieve a balance is deduced. The procedure can be modified by introducing sources and sinks of heat, if these are known.

A somewhat analogous procedure is based upon the vorticity equation, frequently a simplified form such as (64). Again the fields of horizontal advection and local change are evaluated, and the vertical motion needed to make the remaining terms balance is deduced.

A third method uses the ω-equation, generally in a simplified form such as (73). No local changes need be evaluated, but the temperature advection and vorticity advection are both measured. These by themselves would destroy the previously existing geostrophic equilibrium. The vertical motion field and its accompanying field of horizontal divergence are assumed to be those needed to maintain geostrophic equilibrium by compensating for the effects of horizontal advection.

Using the field of vertical motion deduced by any one of these methods, it is possible to compute the vertical transport of angular momentum as a function of latitude and elevation. It is well to ask at this point what could be accomplished by such a computation.

We have noted that our direct estimates of the exchange of angular momentum between the atmosphere and the Earth are rather crude in view of our uncertainty concerning the laws of surface friction and our general failure to incorporate the effects of mountain ranges. Our estimates of the balance requirement is thus correspondingly uncertain. Direct measurement of the horizontal transport has eliminated some of this uncertainty. In addition to obtaining the intellectual satisfaction of having deduced a result by an independent method, we can place further confidence in the assumed numerical values of the surface torque.

The situation concerning the computed vertical transport is different. The deduced fields of vertical motion are at best extremely uncertain as compared to the observed fields of horizontal motion. Neglect of the effects of heating or friction will lead to incorrectly deduced vertical velocities. Additional errors can arise because the time derivatives are not well approximated by the observed 12-hour or 24-hour changes. Although it would be gratifying if the computed vertical transport should be compatible with the horizontal transport, it must be recognized that any disagreement would undoubtedly be attributed to inadequacy of the deduced vertical motions, and the vertical transport needed to balance the computed horizontal transport would still be regarded as a better estimate.

Moreover, the satisfaction of having obtained the result by independent means would be less certain, since in any event the result would not be deduced from independent data; the data used to estimate the vertical motions are the same as those used to compute the horizontal transports. It is even possible to estimate the vertical motion by a procedure based on the vorticity equation which will automatically require the computed vertical transports to agree with the computed horizontal transports.

What the computed vertical transport may indicate is the relative importance of the eddies and the meridional circulation. We mention a recent study by Starr and Dickinson (1964). Here the vertical motions were evaluated by the adiabatic method, so that there was no *a priori* reason why the computed vertical transport of momentum would have to agree with the previously computed horizontal transport. Their results indicated that the eddies were rather ineffective in transporting momentum vertically, implying that the needed vertical transport must be accomplished mainly by the cells.

Palmén and Newton (1967), on the other hand, have evaluated the vertical transport of angular momentum by the cells from the observed meridional circulation in the northern hemisphere in winter, as shown in Figure 18. They have then estimated the vertical transport by the eddies as a residual term. Again, the major portion of the vertical transport is accomplished by the cells. There appears to be a small downward eddy transport in tropical and middle latitudes. As Palmén and Newton point out, there is no way to determine from these computations what portion of the eddy transport is accomplished by cyclone-scale eddies, and what portion is accomplished by motions of much smaller scale.

The vertical transport of water requires other considerations. Since the water which returns to the Earth as precipitation falls from some intermediate elevation, rather than being transported to the ground by the motion of the atmosphere, there must be a net upward transport of water within the atmosphere as a whole.

The principal point to observe is that there is considerable precipitation even in those latitudes where evaporation exceeds precipitation, and except close to the Poles there is considerable evaporation even in those latitudes where precipitation exceeds evaporation. At each latitude the water entering the atmosphere by evaporation must be transported upward to the levels from which precipitation falls, unless it is removed by a strong divergence of horizontal transport in the lower layers. In the latter event there must be a strong convergence of horizontal transport aloft to supply the water which falls as precipitation. It does not appear that the meridional circulation can bring about this convergence, since the lower branches of the cells seem to be concentrated below the levels from which precipitation falls, while the upper branches occur above most of the water. Reference to Figure 44 reveals no convergence of eddy transport above a region of divergence, or above a region where the transport by the meridional circulation may be expected to diverge.

We are forced to conclude that there is an upward transport of water at all latitudes. This may be accomplished by the Hadley cells in the tropics, and possibly by indirect cells at higher latitudes, but between 20°N and 40°N, where the cell motion is downward, the upward transport must be accomplished by the eddies.

This eddy transport could be accomplished either by cyclones and anticyclones or by cumuliform convection, since in either type of system it is the moist air which rises and the dry air which sinks. With a knowledge of the levels from which the precipitation falls, we could deduce the vertical transport of water as a function of latitude and elevation.

The vertical transport of total energy presents further complications because of the presence of both sources and sinks at various levels in the atmosphere. Palmén and Newton have estimated the vertical transport of sensible heat across the 500-mb surface by a procedure similar to the one by which they estimated the vertical transport of angular momentum. Using the distribution of radiative heat sources and sinks as given by London (1957), the horizontal eddy-transports of sensible heat determined by Mintz (1955) and the meridional circulation of Palmén and Vuorela (1963), and partitioning the total release of latent heat into the portions above and below 500 mb by a procedure based upon a consideration of moist-adiabatic ascent of air, they have obtained the vertical eddy-transport as a residual term. They find an upward transport in lower and middle latitudes which appears to be more important than the transport by the meridional circulation. Again, there is no indication as to the scale of the eddies, but they note that, particularly in the tropics, cumuliform convection may be of major importance.

An alternative method of partitioning the total release of latent heat, based upon cloud observations, has been used by Davis (1963), who finds much smaller amounts released above 500 mb. His results would imply even larger upward eddy-transports of sensible heat.

Consequences of the transport processes

The eddy transports of angular momentum and sensible heat are the missing elements in the early theories of the general circulation. From the point of view of the zonally averaged circulation, the convergence of the eddy transport of sensible heat acts as a heat source, additional to the heating by radiation and small-scale turbulent conduction. A convergence of the eddy transport of angular momentum acts as a mechanical force, additional to surface friction and small-scale turbulent viscosity. Without these transports the three-cell structure of the meridional circulation cannot be explained.

Hadley observed that the primary effect of the low-latitude heating and high-latitude cooling would be to force a single direct meridional cell in each hemisphere. The cell would in turn necessitate eastward and westward motions, and consequently eastward and westward frictional drags at the surface. Thomson and Ferrel deduced that the frictional drag upon the surface westerlies would force an additional indirect cell in each hemisphere; Thomson and subsequently Ferrel decided that this cell should be confined mainly to the lower layers.

In an elegant treatment, Eliassen (1952) deduced the combined effects of predetermined heating and mechanical forcing upon the steady meridional motion superposed upon a general circular vortex. His equation was essentially a special case of the more recently introduced ω-equation. He found that local heating would force a flow upward along a surface of constant absolute angular momentum, while eastward mechanical forcing would force a flow outward (equatorward) along a surface of constant potential temperature.

Kuo (1956) applied this approach to the atmosphere, and found that a three-cell pattern was demanded. If the eddies, no matter how intense, transported no angular momentum nor sensible heat, the meridional circulations might be more or less as deduced by Thomson and Ferrel. The supposedly irrelevant disturbances would be irrelevant indeed. With the transports as shown in Figures 41 and 47 the situation is different. The strong divergence of angular-momentum transport in low latitudes, particularly at the jet-stream level, and the strong convergence in middle latitudes force converging poleward and equatorward currents at high levels. These meet in the subtropics and descend to form a direct low-latitude cell and an indirect middle-latitude cell, which are superposed upon the meridional motion which would otherwise exist. The weaker divergence of angular-momentum transport in the polar regions gives rise to a high-latitude direct cell. In addition, the divergence of sensible-heat transport extending well into middle latitudes, and the convergence closer to the Pole, force upward motion in higher middle latitudes and downward motion in lower latitudes, further intensifying the indirect cell.

Just as individual vertical velocities may be estimated from the vorticity equation or the thermodynamic equation alone instead of the ω-equation, so the meridional circulation may be deduced from the momentum transports or the energy transports alone. In short, any convergence of eddy transport of angular momentum, or energy, which is not balanced by friction, or heating, must be balanced by the cell transport. In view of the possibility of significant unknown vertical eddy transports of energy and the general uncertainty as to the distribution of heating, the most reliable results should be obtained from the angular-momentum transport data.

The procedure was first used by Mintz and Lang (1955). In brief, if the absolute angular momentum is known on each of four sides of a "rectangle", say ACDB in the upper left of Figure 50, if the horizontal eddy-transport of angular momentum through sides AB and CD is known, if there is assumed to be no vertical eddy-transport or frictional transfer through sides AC and BD, and if the flow of mass through two adjacent sides AB and AC is known, the flow of mass through sides BD and CD is easily deduced from continuity considerations. Starting at the upper corner of Figure 50, with the boundary condition that no mass flows across the top of the atmosphere or the "90th parallel", we can evaluate the mass flow across any segment if the complete field of horizontal eddy transport is known. In the friction layer, where the above assumptions are no longer valid, the circulation is deduced from mass continuity.

Mintz and Lang used the geostrophically estimated angular-momentum transport values which Mintz (1955) had previously determined, and they assumed no mass flow across the 200-mb level. Because of the crudeness of some of their assumptions they regarded their result as a model of the meridional circulation rather than an evaluation of it. Nevertheless, it appears to be as reasonable as any estimate which was then available. The same procedure has since been used by Holopainen (1966).

The procedure becomes even simpler if A and B, and also C and D, instead of lying on a vertical line, lie on a nearly vertical line of constant absolute angular momentum. One may then begin at any latitude and work downward from the top of the atmosphere. We have carried out modified procedure using Buch's values of the angular-momentum transport and assuming that the friction layer extends to 850 mb. The resulting meridional circulation is shown in Figure 50. It contains well developed Hadley and Ferrel cells, and it is very much like the winter circulation of Mintz and Lang, except that both cells appear farther south.

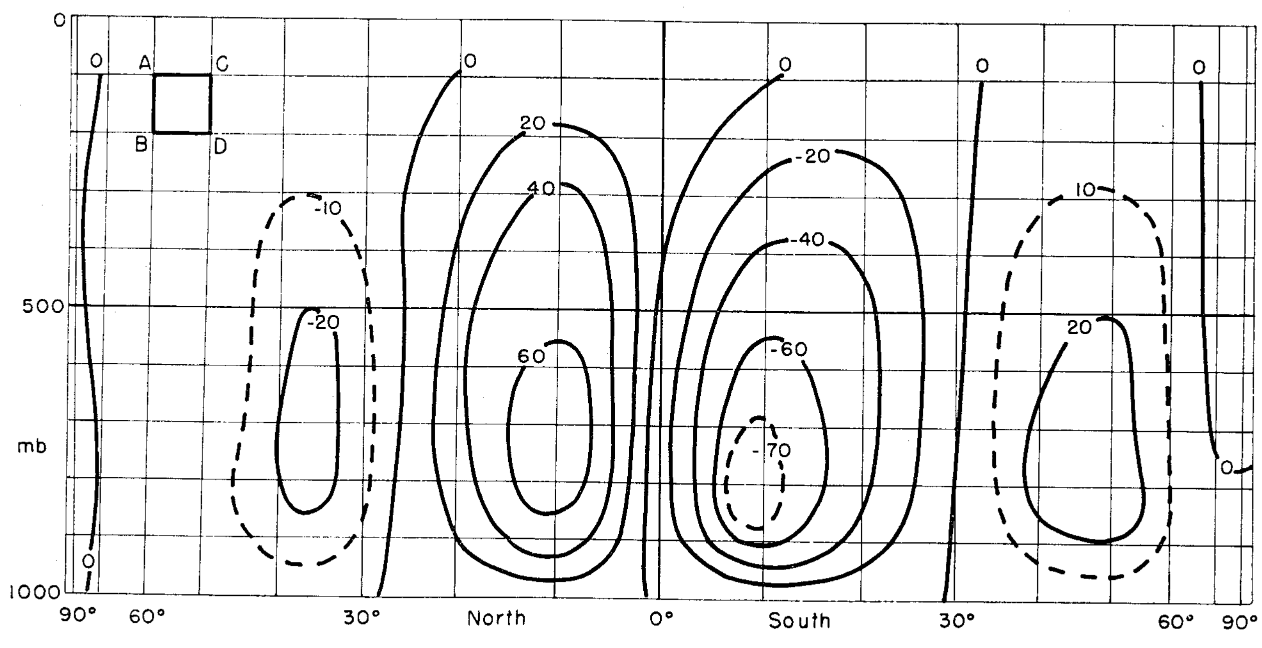


Figure 50. — The average meridional circulation as determined indirectly from the eddy-transport of angular momentum given in Figure 41. The southern hemisphere estimate is by Gilman (1965). The unit for stream function $\overline{\Psi}$ is 10^{12} g sec^{-1}

Gilman (1965) has computed the summer and winter meridional circulations for the southern hemisphere by the modified procedure, using the transport values of Obasi. The average of his summer and winter circulations is also shown in Figure 50. Southern hemisphere data are still quite inadequate for a direct computation of the meridional circulation, and Gilman's indirect computation represents the best picture so far available. Near the Equator the procedure breaks down, and we have joined the two hemispheres by simple continuity, together with the assumption that the opposing currents should meet slightly north of the Equator.

Having obtained a picture of the horizontal and vertical transports of angular momentum, water and energy by the eddies and the meridional cells, we may now ask what is implied concerning the fields of the transported quantities, or, alternatively, the fields of zonal motion, specific humidity, and temperature. It is sometimes stated that the strong upper-level westerly winds are maintained by a convergence of the horizontal transport of angular momentum. In a sense this statement is true; there is convergence of the horizontal transport where the westerlies reach their maximum. Yet there is no simple relation through which the westerly wind speed may be deduced from the field of angular-momentum transport.

Since in the long run any convergence of the horizontal or vertical transport of angular momentum must be balanced by turbulent friction, the time-averaged field of friction may be deduced from the field of angular-momentum transport. The westerly-wind field may therefore be deduced from the angular momentum transport field only to the extent that it may be deduced from the field of friction.

It is reasonable to believe that friction tends to diminish the westerly winds in the regions where they are strongest, but for the atmosphere as a whole the precise relation between wind speed and turbulent friction, if one exists, is but poorly known. The most obvious instance of a direct relation between wind speed and friction occurs at the Earth's surface, where the drag opposes the surface wind at a rate which, if not exactly determined by the wind speed, at least tends to increase with increasing wind speed. The long-term convergence of the vertically integrated horizontal transport of angular momentum in middle latitudes therefore demands a westerly surface wind in these latitudes; if no surface westerlies were present, and if the convergence of the transport persisted, angular momentum would continue to accumulate in middle latitudes. Since the thermal-wind relation would continue to be approximately satisfied, and the horizontal temperature gradient would not become infinite, westerlies would ultimately appear at the surface. The easterlies in low latitudes are similarly demanded by the divergence of the horizontal transport of angular momentum. Of course, the processes required to maintain geostrophic balance might themselves alter the transport of angular momentum. It is only if we postulate that the convergence of angular momentum transport must continue that we can deduce that surface westerlies must appear and persist.

Similar considerations apply to the transport of water. A net convergence of the total water transport does not imply a high value of specific humidity; it implies precipitation in excess of the gain of water by evaporation and turbulent transfer. To a first approximation it may imply a high average relative humidity. Without a knowledge of the temperature field as well, a complete knowledge of the horizontal and vertical transport of water would tell us rather little about the distribution of specific humidity.

The horizontal and vertical transport of energy may be more revealing. A convergence of the transport requires a net loss of energy by radiation and turbulent conduction. To some extent, at least, the amount of energy lost by radiation depends upon the temperature, although the presence of water vapour and clouds can complicate the picture. With a continued convergence of the transport of energy, the temperature should therefore rise until such a time as the increased effect of radiation and conduction can balance the effect of the transport. The air should therefore be warm, or at least warmer than would be indicated by radiation considerations alone.

A complete knowledge of the field of energy transport should therefore give a fair first approximation to the horizontal and vertical distribution of temperature. A knowledge of the field of angular-momentum transport would indicate the distribution of surface easterlies and westerlies, but would by itself give a rather poor indication of the westerly wind field aloft. With a knowledge of the transports of both energy and angular momentum, we should be able to infer the upper-level wind field from the fields of temperature and surface wind, using the thermal wind relation. The procedure would be most satisfactory in the case of an idealized dry atmosphere. For the real atmosphere we should not expect the fields so deduced to be very realistic, because of the complicating effects of water upon absorption and emission of radiation.

For the idealized atmosphere, an explanation of the transport processes would therefore amount to an explanation of the zonally averaged circulation. Nevertheless, since the transport processes are themselves affected by field of motion, an independent explanation of the transport processes which could then be used to explain the zonally averaged motion does not appear possible. On the other hand, any complete explanation of the zonally averaged circulation must contain, explicitly or implicitly, an explanation of the transport processes.