CHAPTER V

THE ENERGETICS OF THE ATMOSPHERE

In his famous account of the trade winds and monsoons, Halley (1686) identified the sun as the cause of the motions of the atmosphere. Although the bulk of Halley's theory is no longer regarded with favour, it is still generally accepted that the ultimate source of atmospheric energy is the sun. The direct effect of solar radiation is to heat the atmosphere and the underlying ocean and land, and thereby produce internal energy. The motions of the atmosphere, on the other hand, represent a great supply of kinetic energy. This supply is being continually dissipated by friction. One of the main problems in general-circulation theory concerns the manner in which some of the internal energy produced by solar heating is ultimately converted into kinetic energy to replenish the supply thereof.

In the previous chapter we examined in some detail the maintenance of the spatial distributions of wind velocity, temperature, and moisture. In the present chapter we shall examine in further detail the manner in which the total amounts of kinetic, potential, and internal energy represented by wind, temperature, and moisture fields are maintained, but we shall be only incidentally concerned with the geographical distributions of these fields. In all probability we cannot completely explain the maintenance of the total amounts without explaining the geographical distributions as well. Nevertheless, by considering the production, transformation, and dissipation of energy separately from the remaining aspects of the circulation, we may gain further insight into the roles played by some of the physical processes.

Two fundamental quantities to be considered are the rate at which solar energy reaches the extremity of the atmosphere, and the rate at which new kinetic energy must be produced to offset the dissipative effects of friction. The former rate is observed to be about 1.8×10^{17} watts, or on the average about 350 watts per square metre of the Earth's surface. Various estimates place the latter rate at about one hundredth of the former. If the atmosphere is regarded as a heat engine, producing kinetic energy, the ratio η of these rates, about one per cent, is a measure of its efficiency. The determination and explanation of the efficiency η constitute the fundamental observational and theoretical problems of atmospheric energetics.

Basic energy forms and conversions

Since the bulk of the incoming solar radiation heats the underlying ocean or land instead of heating the atmosphere directly, we need to examine the energetics of the atmosphere-ocean-Earth system, or at least that part of the system which directly or indirectly exchanges significant amounts of energy with the atmosphere. We may disregard the hot interior of the Earth, since the heat received from it is negligible, except locally in regions of volcanic activity. We may likewise disregard the deep oceans, although we should recognize the possibility that heat stored there may reach the surface years later through slow overturning, and influence the long-period atmospheric fluctuations (see Rossby 1957).

The forms of energy which play a significant role are kinetic energy (KE), potential energy (PE), and internal energy (IE). Thermodynamically both thermal internal energy and the latent energy of condensation and fusion of water are forms of IE, but sometimes they are more conveniently treated as

separate forms of energy. Some writers prefer to treat the kinetic energy of small-scale turbulent motions as a form distinct from KE. In the present treatment, where the small-scale motion is not regarded as a part of the circulation, and where no distinction is made between turbulent and molecular friction, it seems most logical to treat turbulent kinetic energy as neither KE nor a form by itself, but as a portion of the IE, thus effectively grouping the kinetic energy of small-scale motions with the kinetic energy of molecular motions.

Other forms of energy are not directly or indirectly converted into KE, PE, or IE in large amounts, although they may be important on a local scale. The electrical energy converted into IE through lightning discharges may, for example, play an important part in the dynamics of thunderstorms and tornadoes, but the total amount of electrical energy in the atmosphere is minor. By contrast, there is a vast supply of nuclear energy, but, fortunately for humanity, the natural processes for releasing it are virtually absent.

The atmosphere-ocean-Earth system exchanges total energy with its environment only through radiation. In so doing, the system gains or loses only IE. Since the system does not undergo any net long-term change in total energy, the heating by incoming radiation must in the long run balance the cooling by outgoing radiation.

Within the atmosphere-ocean-Earth system, IE may be transferred from one location to another, and in particular from the atmosphere to the underlying surface or vice versa, through radiation and conduction. Again, the net heating of the system by these processes is zero.

Only those processes involving a force can produce or destroy KE. Motion of the atmosphere (or the ocean) with or against the force of gravity, and hence downward or upward, converts PE into KE, or KE into PE. The process is adiabatic and thermodynamically reversible. KE is the only immediate source or sink for PE.

Likewise, motion of the atmosphere with or against the pressure-gradient force, and hence across the isobaric surfaces toward lower or higher pressure, converts IE into KE, or KE into IE. Again the process is reversible and adiabatic. Motion of the atmosphere against the force of friction, and the frictional heating which accompanies it, also convert KE into IE. By its very nature the process is irreversible, since friction must on the average oppose the motion. The frictional heating produces the necessary increase in entropy. The only remaining force, the Coriolis force, acts at right-angles to the motion and does not add or remove KE.

It follows that the conversion of IE into KE by the pressure-gradient force, although thermodynamically reversible in that it can proceed equally well in either direction, does not proceed to the same extent in either direction. In the long run it produces as much KE as is dissipated by friction.

It also follows, since there is no long-term net heating by radiation and conduction, and since the remaining energy-conversion processes other than friction involve no heating at all, that the net total heating of the atmosphere-ocean-Earth system equals the net frictional heating. The total heating of the system is therefore positive, not zero.

If a distinction is made between the thermal and latent forms of IE, the processes of evaporation and melting and the reverse processes of condensation and freezing convert thermal IE into latent IE, and vice versa. In particular, evaporation from the ocean surface removes thermal IE from the ocean and adds latent IE to the atmosphere. It is possible, however, not to include latent IE as a form of atmospheric energy, provided that the release of latent heat, which must inevitably occur regardless of

the attitude which one takes toward latent energy, is treated as a special form of heating by the environment, rather than an internal quasi-adiabatic process. If this convention is adopted, the atmosphere will be assumed to gain IE not when water evaporates from the ocean, but when the water subsequently condenses within the atmosphere.

In this manner one may treat the energetics of the atmosphere by itself. Aside from surface friction, the total influence of the environment upon the atmosphere may be treated as the addition and removal of equal amounts of thermal IE by heating and cooling. Surface friction need not be distinguished from internal friction, since both processes convert KE into IE irreversibly. The only remaining conversions from one form to another are then the reversible processes within the atmosphere, involving KE and PE, or KE and IE. Throughout the remainder of this chapter we shall consider the energetics of the atmosphere alone, from this point of view.

The fact that the atmosphere remains very nearly in hydrostatic equilibrium places certain constraints upon the energy conversion processes which may actually occur. When, for example, heating adds IE to the atmosphere, upward expansion occurs, and the upward motion converts some IE into KE and an equal amount of KE into PE. It is easily shown that under hydrostatic equilibrium the PE contained in a vertical column extending throughout the depth of the atmosphere is proportional to the IE, in the ratio R/c_v, or approximately 2/5, although the result is strictly true only for a dry atmosphere extending upward from sea-level. The amounts of PE and IE per unit mass are given by gz and c_vT, while an element of mass of a column of unit cross-section is ρdz; thus

$$\int_0^\infty gz\rho dz = \int_0^{p_0} z dp = \int_0^\infty p dz = \int_0^\infty RT\rho dz, \tag{97}$$

where p_0 is the surface pressure. Thus PE and IE increase or decrease together, and it is convenient to regard them as a single form of energy, called total potential energy (TPE) by Margules (1903). It is meaningless to speak of the TPE at a particular point, but within a vertical column the average amount of TPE per unit mass is given by the average value of c_pT, which is simply the sensible heat per unit mass.

It follows that whenever the horizontal motions by themselves convert seven units of IE into KE, the vertical motions which must accompany them in order to maintain hydrostatic equilibrium convert two units of KE into IE, and two units of PE into KE. The net result is a conversion of five units of IE and two units of PE, or seven units of TPE, into KE. Thus the vertical motions alone do not alter KE or TPE. Effectively, horizontal motion of the atmosphere with or against the pressure-gradient force, and hence across the isobars toward lower or higher pressure, converts TPE into KE, or KE into TPE. This is now the only conversion process which we need to consider, in view of the convention concerning latent energy which we have adopted.

Mathematical expressions for the conversion processes may be obtained from the equations presented in Chapter II. If $\{X\}$ denotes the total amount within the atmosphere of a quantity whose value per unit mass is denoted by X, and if, as in the previous chapter, $\overline{X}$ denotes a long-term time average of X, it follows that $\{dX/dt\} = \partial\{X\}/\partial t$. Hence, from (40) and (41),

$$\partial\{\Phi + I\}/\partial t = H - C, \tag{98}$$

$$\partial\{K\}/\partial t = C - D, \tag{99}$$

where

$$H = \{Q\} \tag{100}$$

is the total heating of the atmosphere,

$$D = - \{ \mathbf{U} \cdot \mathbf{F} \} \tag{101}$$

is the total dissipation, and C is the rate of conversion of TPE into KE by reversible adiabatic processes, given by the alternative expressions

$$C = - \{ \omega \alpha \} = g \{ \omega \partial z / \partial p \} = - g \{ z \partial \omega / \partial p \} = g \{ z \mathbf{V} \cdot \mathbf{U} \} = - g \{ \mathbf{U} \cdot \nabla z \}. \tag{102}$$

It is to be observed that no matter which expression is used for C, only the divergent part of the wind is involved.

It follows that the long-term averages $\overline{H}$, $\overline{C}$, and $\overline{D}$ are equal, and, as previously noted, are positive, since D is always positive. Further restrictions upon the field of Q follow because there are no net long-term changes of entropy and potential temperature. From (15), (14), and (100),

$$\overline{\{Q/T\}} = 0, \quad \overline{\{Q/p^\kappa\}} = 0, \quad \overline{\{Q\}} = \overline{H} > 0. \tag{103}$$

Since T and p^κ are necessarily positive, it follows that Q is negatively correlated with $1/T$ and $1/p^\kappa$.

This statement applies equally well if Q is replaced by the non-frictional heating $Q_n = Q - Q_f$, since the frictional heating Q_f, is positive everywhere. Thus

$$\overline{\{Q_n/T\}} < 0, \quad \overline{\{Q_n/p^\kappa\}} < 0, \quad \overline{\{Q_n\}} = 0. \tag{104}$$

Within the limited sense of these inequalities, the heating must occur at a higher temperature and a higher pressure than the cooling.

The process which converts TPE into KE, represented by $C = - \{ \omega \alpha \}$, is often colloquially described as a sinking of colder air and a rising of warmer air at the same elevation. This interpretation seems reasonable in view of the strong negative correlation between ω and w. Certainly it is correct for the case of a simple Hadley circulation. Yet $\{ \omega \alpha \}$ cannot be converted without further approximation into an expression involving w and T alone, and the interpretation cannot be rigorously defended.

We have previously identified the measurement of the efficiency η, or equivalently the determination of $\overline{H}$, $\overline{C}$, or $\overline{D}$, as the fundamental observational problem of atmospheric energetics. Early estimates of the efficiency, which ranged as high as 20 per cent were estimates of the classical thermodynamic efficiency, which is considerably greater than η. The thermodynamic efficiency may be defined as the ratio of the net heating to the heating at the heat source. In this ratio the numerator may be identified with the net heating of the atmosphere, but the denominator is not the net solar heating. It is probably best identified with the difference between the incoming and outgoing radiation, summed over only those regions where this difference is positive. It follows that the classical thermodynamic efficiency exceeds η by a factor of perhaps 4 or 5. The thermodynamic efficiency cannot exceed the ratio of the temperature difference between the heat source and the heat sink to the temperature of the heat source, whence an upper bound can be estimated from the temperature field alone.

Direct evaluation of $\overline{D}$ is difficult, in view of our inadequate knowledge of friction, especially at higher elevations. An early estimate by Sverdrup (1917) was 1.3×10^{15} watts, or 2.55 watts per square metre of the Earth's surface, equivalent to a value of η of 0.007. This value was based upon empirically determined coefficients of viscosity in the relatively steady trade winds.

Brunt (1920) estimated a value of 5 watts m^{-2}, which would make $\eta = 0.014$; this he obtained by computing a value of 3 watts m^{-2} in the surface friction layer, and assuming that the remainder of the atmosphere ought to contribute nearly as much. For some time Brunt's value was regarded as the best

estimate attainable. Subsequent estimates were substantially lower, and in a comprehensive review of previous estimates, Oort (1964a) chose 2.3 watts m⁻² as the most reasonable value.

Direct evaluation of $\overline{C}$ requires measurement of the divergent part of the wind in one manner or another, which is also difficult to perform accurately. Nevertheless, it would appear to offer an independent method of determining. η.

Examination of Sverdrup's procedure reveals, however, that he based his coefficients of viscosity on observations of cross-isobar flow in the trade winds. Likewise, Brunt's estimate was based upon typical cross-isobar flow in the friction layer. It thus appears that both Brunt and Sverdrup were actually evaluating $\overline{C}$ rather than $\overline{D}$.

The very recent estimates of $\overline{D}$ by Holopainen (1963) and Kung (1966) are also based upon evaluation of the cross-isobar flow — $g\mathbf{U}\cdot\nabla z$. The procedure is of considerable interest. Holopainen used only a few weeks of winter data at a few stations in England. Kung used a year of data at a fairly dense network over North America, but his study was still far from global. Since the conversion — $g\mathbf{U}\cdot\nabla z$ varies widely and even changes sign from one point to another, its average over a limited region would not likely represent a global average, even if errors in observation could be eliminated. The less easily computed dissipation $-\mathbf{U}\cdot\mathbf{F}$ can be expected to be positive everywhere, at least when vertically integrated, and an estimate from a limited region might be fairly acceptable. Accordingly the remaining term $d\,(\mathbf{U}\cdot\mathbf{U}/2)/dt$ in the kinetic-energy equation, which would automatically vanish in a long-term global average, was also evaluated over the limited region, and was subtracted from — $g\mathbf{U}\cdot\nabla z$ to yield the estimate of $-\mathbf{U}\cdot\mathbf{F}$.

Holopainen found a value of 10.4 watts m⁻²; Kung's more extensive data indicated an annual mean of 7.1 watts m⁻² and rather little seasonal variation, thus raising η to 0.02. In Kung's computations the atmosphere was divided into twenty layers. The greatest dissipation was found in the lowest 100 mb, but, after a relative minimum near the 500-mb level a secondary maximum was reached near the tropopause. Undoubtedly the final estimate of $\overline{D}$ has yet to be made, but it is not unlikely that generally accepted values will have to be revised upward.

Direct evaluation of $\overline{H}$ might appear to offer a third method of determining η, but this method proves to be useless. Heating of all kinds includes heating by friction, and the evaluation cannot be independent of the evaluation of $\overline{D}$. The total non-frictional heating must be zero, and there is nothing further to learn about this total by computing any of its parts.

It is nevertheless possible to estimate $\overline{H}$ from the spatial distribution of Q by taking advantage of the restriction that heating does not in the long run alter the mean entropy. From (103) it follows that for any constant T_1,

$$\overline{\{Q\,(1 - T_1/T)\}} = \overline{H}. \qquad (105)$$

If T_1 is chosen so that $1/T_1 = \overline{\{1/T\}}$, $\overline{\{1 - T_1/T\}}$ vanishes, and $\overline{H}$ is seen to depend only upon the covariance of heating and temperature. It may therefore be estimated moderately well from estimates of Q which are not sufficiently accurate to estimate $\overline{\{Q\}}$. That is, errors in Q will largely be cancelled by errors in $Q\,T_1/T$.

This is in essence the method of evaluation used by Lettau (1954), who thereby obtained a value of 2 watts m⁻². Dividing a hemisphere into six zones, each covering fifteen degrees of latitude, Lettau estimated average values of Q and T for each zone, and obtained an estimate of $\overline{H}$. Only horizontal variations of Q and T entered his computations, and he pointed out that his value was probably an underestimate on this account.

It would be equally possible to take advantage of the restriction that heating does not in the long run alter the mean potential temperature. Hence, again in view of (103), for any constant p_1,

$$\overline{\{Q\,(1 - p_1{}^\kappa/p^\kappa)\}} = \overline{H}. \tag{106}$$

To use this equation effectively, a knowledge of the vertical but not the horizontal variations of Q would be needed. Apparently neither equation (105) nor (106) makes full use of the known restrictions upon the distribution of Q. For this purpose the concept of available potential energy has proven advantageous.

Available potential energy

In most of the recent work in atmospheric energetics, TPE has been further resolved into available potential energy (APE) and unavailable potential energy (UPE). With the conventions which we have adopted, the reversible adiabatic processes which convert TPE into KE preserve the potential temperature of each parcel of air, and therefore preserve the statistical distribution of potential temperature. Among those hypothetical states of the atmosphere possessing the same statistical distribution of potential temperature as the existing state, there is one state, commonly known as the reference state, which possesses the least TPE. In the reference state the surfaces of constant pressure and constant potential temperature are horizontal, and the potential temperature never decreases with increasing elevation.

Following Lorenz (1955, 1960), the UPE of any state of the atmosphere is defined as the TPE of the corresponding reference state, while the APE is defined as the excess of TPE over UPE. In his famous paper on the energy of storms, Margules (1903) introduced a quantity similar to APE, which he called available kinetic energy *(die verfügbare kinetische Energie)*, but he did not apply the concept to the general circulation.

Since the reversible adiabatic processes which convert TPE into KE do not alter the reference state, they do not affect the UPE. The conversion of TPE into KE, whose rate is given by C, may therefore equally well be described as a conversion of APE into KE.

In the long run the APE removed by conversion must be replaced by heating, at the same rate at which TPE is replaced by heating. However the individual modes of heating — radiation, conduction, condensation, friction — each affect the UPE, and hence need not affect APE and TPE alike.

We have observed that the net long-term production of TPE by friction alone is equal to the net production of TPE by all modes of heating; the net production by non-frictional heating is zero. In converting KE into TPE, however, friction raises the potential temperature of some portions of the atmosphere, and does not lower the potential temperature of any. It must therefore raise the TPE of the reference state, i.e. the UPE, very likely by about as much as it raises the TPE of the existing state. The APE produced by friction, if any, must therefore be less than the TPE produced, or the KE dissipated, and presumably it is much less.

Herein lies the principal importance of APE. Since as much APE as TPE is produced by heating of all kinds, there must be a net production of APE by heating other than friction. An estimate of the rate at which heating generates APE therefore affords an estimate of η which is independent of those estimates involving the concept of friction.

Since UPE cannot be altered by reversible adiabatic processes, APE is in a sense a measure of the portion of the TPE available for conversion into KE; hence the name. Nevertheless there is no requirement in the definition of the reference state that a reversible adiabatic process leading from the existing

to the reference state actually exists, since conservation of potential temperature is only one of the requirements which a dynamically possible reversible adiabatic process must fulfil. In general the reference state cannot be reached, so that APE is only an upper bound, and not a least upper bound, for the amount of energy available for conversion into KE. The fact that not all of the APE is "available" is not however of great consequence, since during any time interval when a major portion of the APE could be converted into KE, additional APE will be produced by heating.

The sum of APE and KE resembles negative entropy in that friction decreases it and internal reversible adiabatic processes leave it unaltered, while heating is needed to increase it. It is nevertheless a separate concept from entropy, since it involves the field of motion, while entropy depends only upon the thermodynamic state. Strictly speaking, internal radiative transfer can sometimes increase the APE; this is not true of negative entropy.

The generation of APE by heating, the conversion of APE into KE by reversible adiabatic processes, and the dissipation of KE by friction may be regarded as the three steps in the basic energy cycle of the general circulation. They are indicated schematically in Figure 51.

Before making any direct estimate of the generation of APE by heating, it is almost essential to obtain analytic expressions for APE and the rate at which it is produced, in order to avoid the errors of omission and oversimplification which are so easily made when verbal arguments are applied to rather intricate phenomena. The definition of APE involves the reference state, which is most readily described in a co-ordinate system in which potential temperature θ rather than z or p is used as the vertical co-ordinate. Within any vertical column of unit cross-section, barring superadiabatic lapse rates or appreciable departures from hydrostatic equilibrium, the pressure p corresponding to a given potential temperature θ is simply the weight of the air whose potential temperature exceeds θ. This statement will be true even for values of θ less than the surface value θ_0 provided that the definition of p is extended so that $p(\lambda, \varphi, \theta) = p_0(\lambda, \theta)$ when $\theta < \theta_0$, where p_0 is the surface pressure. It is possible to define a quantity $P(\theta)$ whose value at any point in the atmosphere equals the average value of p on the isentropic surface passing through that point; thus

$$P(\theta) = S \int_S p(\lambda, \varphi, \theta) \, dS, \tag{107}$$

where $dS = a^2 \cos \varphi \, d\lambda \, d\varphi$ is an element of horizontal area, and the integration extends over the area S of the Earth. The ratio $P(\theta)/P(o)$ is then the probability that a randomly selected mass of air will
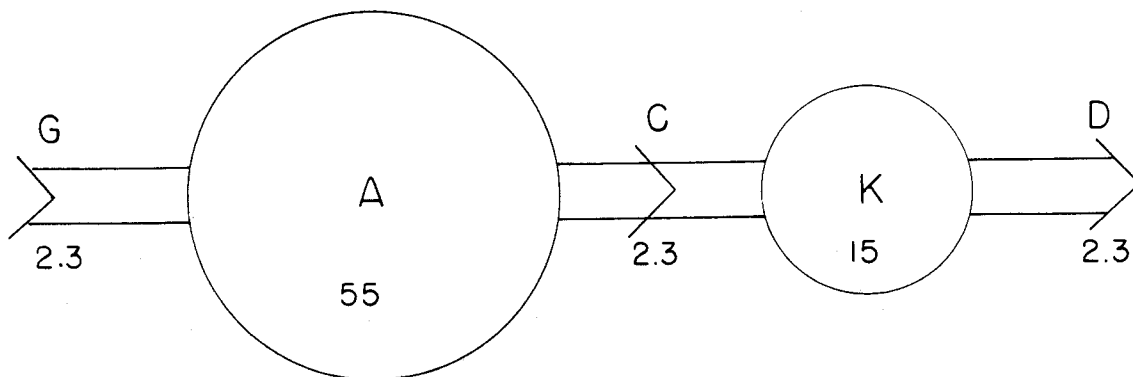

Figure 51. — The basic energy cycle of the atmosphere as estimated by Oort (1964 a). Values of available potential energy and kinetic energy are in units of 10^5 joules m^{-2}. Values of generation, conversion, and dissipation are in watts m^{-2}

have a potential temperature exceeding θ. If now any quantity characterizing the existing state is expressed in terms of $p(\lambda, \varphi, \theta)$, the same quantity for the reference state is obtained by replacing p by P.

This assumption neglects the topography. Because elevated land masses fill some of the space which would otherwise be occupied by air, pressure is not a perfect measure of total mass; the mass of air having a pressure between 1000 and 900 mb, for example, is less than the mass having a pressure between 600 and 500 mb.

Neglecting topography, the TPE of a vertical column is given by

$$c_{\mathrm{p}}\, g^{-1} \int_0^{p_2} T dp = (1 + \kappa)^{-1}\, p_{00}{}^{-\kappa}\, c_{\mathrm{p}}\, g^{-1} \int_0^{\infty} p^{1+\kappa}\, d\theta\,. \tag{108}$$

The term involving $p_0{}^{1+\kappa}\,\theta_0$ which would otherwise appear upon integration by parts has been eliminated by choosing $\theta = 0$ as the lower limit of integration and taking advantage of the extended definition of p.

The UPE may now be obtained by replacing p by P in (108) and integrating horizontally, and the APE may then be obtained by subtraction. At a particular point or even within a particular column APE is not defined, but the APE of the whole atmosphere is seen to be given by

$$A = (1 + \kappa)^{-1}\, p_{00}{}^{-\kappa}\, c_{\mathrm{p}}\, g^{-1} \int_S \int_0^{\infty} (p^{1+\kappa} - P^{1+\kappa})\, d\theta\, dS\,. \tag{109}$$

Equation (109) is the so-called exact formula for the APE, although actually it contains the hydrostatic approximation and neglects the presence of topography. It is the appropriate form to use in further theoretical developments. Nevertheless, unless one is thoroughly conditioned to thinking in a θ-co-ordinate system, the features of the atmosphere which are associated with significant amounts of APE may not be apparent. A number of approximate expressions have therefore been developed; the original approximation of Lorenz (1955) is

$$A = \frac{1}{2}\, c_{\mathrm{p}} \left\{ \Gamma_{\mathrm{d}}\, (\Gamma_{\mathrm{d}} - \widetilde{\Gamma})^{-1}\, \widetilde{T}^{-1}\, \widetilde{T''^2} \right\}\,. \tag{110}$$

Here $\Gamma = -\partial T/\partial z$ is the vertical lapse rate of temperature, and $\Gamma_{\mathrm{d}} = g/c_{\mathrm{p}}$ is the dry-adiabatic lapse rate (about $10°$ per kilometre). The tilde ($\sim$) denotes an average over an entire isobaric (or approximately horizontal) surface, and the double prime ($''$) denotes a departure from this average. Lorenz obtained the approximation by observing first that since $p > 0$, $\kappa > 0$, and P is an average of p, the integral of $p^{1+\kappa} - P^{1+\kappa}$ is positive definite for each isentropic surface, and may be approximated in terms of the variance of p on the isentropic surface. Second, provided that the isentropic surfaces are not too greatly inclined to the horizontal, the variance of p on an isentropic surface may be approximated in terms of the variance of θ, and hence of T, on an isobaric surface.

Van Mieghem (1956) obtained a somewhat similar approximation by assuming that the reference state could evolve from the existing state by a dynamically possible adiabatic process, and then using a variational procedure to compute the gain of KE during the envisioned process. The general agreement between the expressions might not have been expected in view of the usual absence of a process leading to the reference state. However, APE depends upon the field of mass alone, while the existence of the envisioned process depends also upon the field of motion. Apparently, corresponding to any existing

field of mass, at least within reasonable limits, there exists a hypothetical field of motion, in general differing from the existing field of motion, such that, if the state of the atmosphere consisted of the existing field of mass and the hypothetical field of motion, the reference state would subsequently occur. The expressions should therefore agree.

From expression (110) it follows that the APE may be approximated by a weighted average of the *horizontal* variance of temperature, the weighting function being inversely proportional to the horizontally averaged static stability, as measured by $\Gamma_d - \widetilde{\Gamma}$. The approximation may be shown to be most acceptable when $\Gamma_d - \widetilde{\Gamma}$ is large, and it is worthless if $\widetilde{\Gamma}$ is near Γ_d, since the APE does not become infinite.

This approximation is consistent with the approximate rule that KE is produced when cold air sinks and warm air rises at the same level. In order that such a process may occur at all, there must first of all be different temperatures at the same level. If the stratification is stable, the temperature at a fixed elevation will rise in the sinking air and fall in the rising air, and the process will thereby reduce the horizontal temperature contrast and finally eliminate it. Moreover, the less stable the stratification, the farther the cold air must sink and the warm air must rise in order to eliminate the temperature contrast. Thus more KE is attainable, and so more APE is present, when the horizontal temperature contrast is greater and when stratification is less stable.

According to (110), there should be two principal methods by which heating can produce APE; first, by heating the warmer regions and cooling the cooler regions at the same elevation, thereby increasing the horizontal temperature variance, and, second, by heating the lower levels and cooling the upper levels, thereby decreasing the static stability. The former process is essentially the one explicitly considered by Lettau in his estimate of the efficiency.

From the exact expression (109) for APE, it follows that

$$\partial A / \partial t = G - C, \tag{111}$$

where

$$G = \left\{ Q \left(1 - p^{-\kappa} P^\kappa \right) \right\}. \tag{112}$$

The quantity $N = 1 - p^{-\kappa} P^\kappa$ appearing in (112) may be regarded as an efficiency factor, which represents the effectiveness of heating at any point in producing APE. Where N is negative, cooling will produce APE.

Figure 52 presents a hypothetical distribution of potential temperature, which is based upon the average temperature field shown in Figure 10. Possible variations of surface pressure have been neglected. The pressure which a given potential-temperature surface would assume in the reference state is proportional to the area above this surface; these pressures are indicated by the numbers in parentheses, the pressure at the Earth's surface being taken as 1000 mb. With this numbering, Figure 52 becomes a chart of the distribution of P. From Figure 52 the distribution of the efficiency factor N has been evaluated; it is shown in Figure 53.

If this distribution of N is at all typical, the APE generated by friction could not exceed 8 per cent of the kinetic energy dissipated, even if friction were confined to low levels in the tropics. If friction were uniformly distributed throughout low levels, the amount could not exceed 3 per cent. If it is true that there is considerable dissipation near the tropopause, where N is negative, friction may generate no APE at all. The assumption that a direct estimate of $\overline{G}$ is independent of an estimate based upon friction therefore seems to be justified.
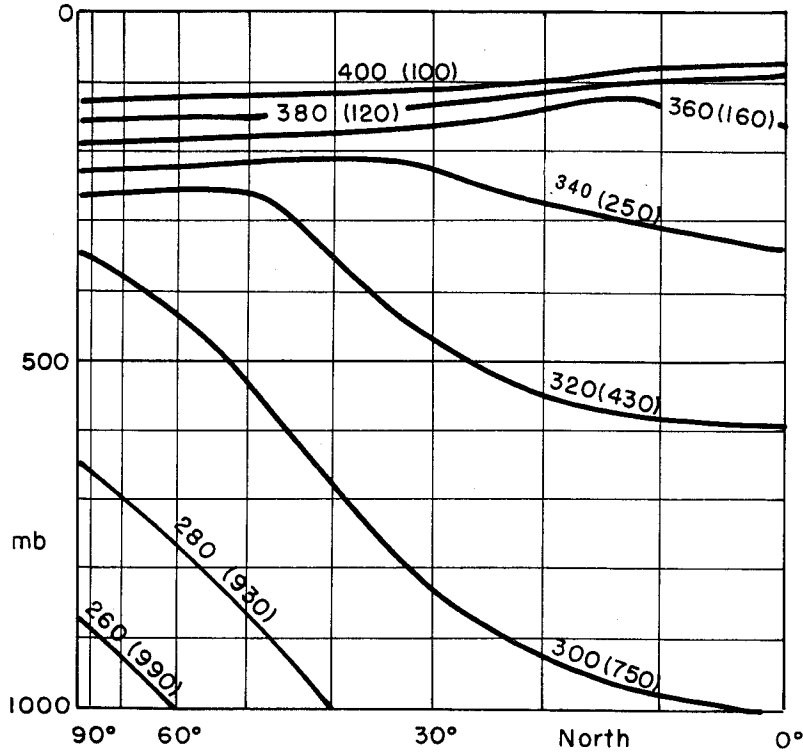
Figure 52. — A hypothetical distribution of potential temperature θ, and corresponding distribution of $P(\theta)$. Values of θ are in degrees K, and values $P(\theta)$ (in parentheses) are in millibars
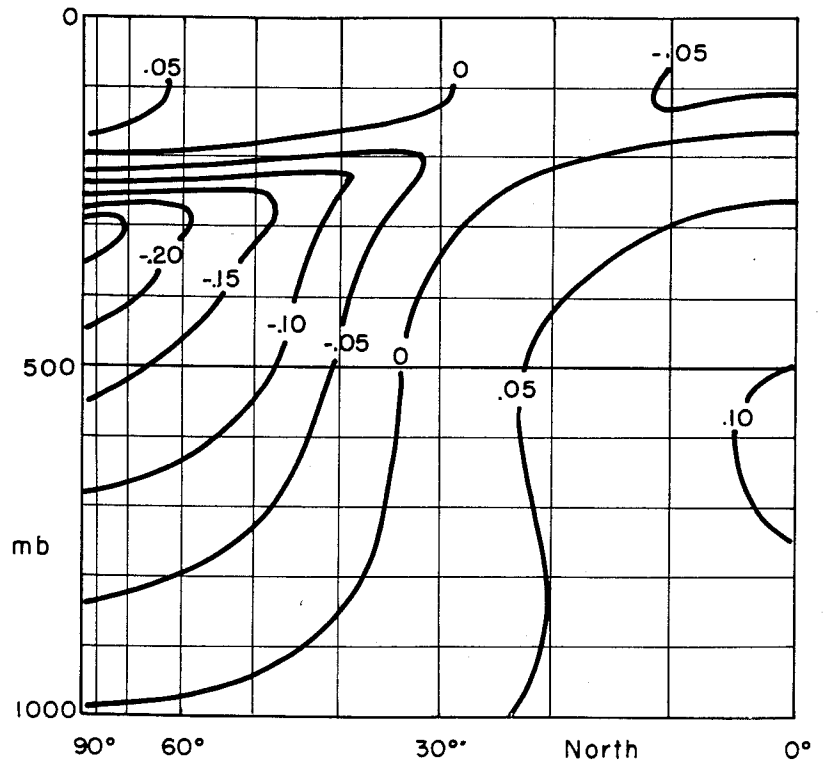
Figure 53. — The distribution of efficiency factor N corresponding to distribution of θ in Figure 52

From the approximate expression (110) for A, Lorenz derived the approximation

$$G = \left\{ \Gamma_{\rm d} \left(\Gamma_{\rm d} \, \widetilde{T} \right) \widetilde{T}^{-1} \, \widetilde{Q}'' \, \widetilde{T}'' \right\}. \tag{113}$$

This formula has formed the basis for many of the subsequent observational studies. Only the influence of heating upon the factor $\widetilde{T}''^{2}$ in (110) has been included. A closer approximation would include the influence of heating on $\widetilde{T}$ and $\widetilde{T}$, but it would still not agree with the exact expression (112).

On the basis of this expression Lorenz estimated a value of 4 watts m^{-2} for $\overline{G}$. This estimate was based upon assumed average values of Q and T as functions of latitude only. It is of interest to compare the result with Lettau's lower estimate. There was little difference in the data used, and the formulae are nearly the same except for the lapse-rate factor $\Gamma_{\rm d} \left(\Gamma_{\rm d} - \widetilde{T} \right)^{-1}$ and the restriction to the horizontal covariance in (113).

With a "normal" lapse rate of 2/3 of the dry-adiabatic, the lapse-rate factor acquires a value of 3. This more than accounts for the difference in the estimates, and seems to represent the degree of the underestimation whose existence Lettau pointed out. The heating must decrease with elevation, and this decrease must contribute to a covariance of Q and T in formula (105) if T also decreases with elevation. Formula (113) effectively takes into account the necessary vertical decrease of Q by means of the lapse-rate factor.

The fundamental theoretical question now arises as to why η should be as low as one or two per cent, or, for that matter, why it should not be even lower. In attempting to answer this question, Lorenz (1960) sought the maximum possible value of η. Since the generation of APE depends essentially upon the horizontal covariance of heating and temperature, there can be no generation with no temperature contrast. If on the other hand the contrast is so great that radiative equilibrium prevails, there is no net heating and again no generation. The maximum generation therefore accompanies a somewhat weaker temperature contrast. The thermodynamic efficiency is then not particularly great, since even the cold source is not excessively cold, while η falls considerably short of the thermodynamic efficiency, since there is considerable outgoing radiation at the heat source.

On the basis of a crude model, Lorenz found a maximum η of not much greater than two per cent. This led him to speculate that the atmosphere might be constrained to operate at nearly maximum efficiency; specifically, when several modes of behaviour satisfy the dynamic equations, the less efficient modes may be unstable and give way to the more efficient modes.

Further considerations suggest that the maximum efficiency may really be considerably higher; for example, Lorenz's model applies to a dry atmosphere, where the outgoing radiation is greatest at the warmest latitudes, thus acting to destroy APE. In the real atmosphere outgoing radiation is more nearly independent of latitude. Nevertheless, the notion that the general circulation should act so as to maximize or minimize some basic quantity is attractive, and perhaps not unreasonable. Dutton and Johnson (1967) have recently attempted to apply principles similar to the principle of least action to the general circulation. The introduction of such a principle might seem to be an overspecification, but, since the governing equations apparently possess a nearly infinite variety of sets of long-term statistics, some such principle may be just what is required to single out the set which actually prevails.

Zonal and eddy energy

The knowledge that APE is continually converted into KE at a certain average rate does not by itself reveal the types of weather patterns which are primarily responsible for the conversion. The dominant process could conceivably be a general sinking of cold air in higher latitudes and a rising of

warm air in lower latitudes, as in Hadley's circulation. It could equally well be a sinking in the colder portions of cyclones and anticyclones and a rising in the warmer portions at the same latitudes. Most of the recent studies in atmospheric energetics have been concerned with the further resolution of KE and APE, and of the accompanying generation, conversion, and dissipation processes, into the portions associated with separate modes of motion.

Most frequently KE and APE are resolved into the amounts associated with the zonally averaged fields of motion and mass and the amounts associated with the eddies. The KE may be resolved into zonal kinetic energy (ZKE), the amount of KE which would exist if the existing zonally averaged motion but no eddy motion were present, and eddy kinetic energy (EKE), the excess of KE over ZKE. Because KE is a quadratic function of velocity, EKE is also the KE which would remain if the existing eddy motion but no zonal motion were present. If the velocity field $\mathbf{U}$ is resolved into the zonally averaged motion $[\mathbf{U}]$ and the eddy motion $\mathbf{U}^\star$, the total amounts of ZKE and EKE become

$$K_z = \frac{1}{2} \left\{ [\mathbf{U}] \cdot [\mathbf{U}] \right\}, \tag{114}$$

$$K_E = \frac{1}{2} \left\{ \mathbf{U}^\star \cdot \mathbf{U}^\star \right\}. \tag{115}$$

It is to be observed that the term "zonal kinetic energy" does not refer to the kinetic energy of the zonal motion (which would be $u^2/2$) nor the zonally averaged kinetic energy (which would be $[\mathbf{U} \cdot \mathbf{U}]/2$).

Likewise, the APE may be resolved into zonal available potential energy ($ZAPE$), the amount of APE which would exist if the field of mass were replaced by its zonal average, and eddy available potential energy ($EAPE$), the excess of APE over $ZAPE$. It is not at all certain how the zonal average of the mass field is best defined. In order to use the exact formula (109), it would be best to define $[p]$ as the average pressure along a latitude circle on an *isentropic* surface, and to let $p^\star = p - [p]$. In this case the total amounts of $ZAPE$ and $EAPE$ become

$$A_z = (1 + \kappa)^{-1} \, p_{00}^{-\kappa} \, c_p \, g^{-1} \int_S \int_0^\infty ([p]^{1+\kappa} - P^{1+\kappa}) \, d\theta \, dS, \tag{116}$$

$$A_E = (1 + \kappa)^{-1} \, p_{00}^{-\kappa} \, c_p \, g^{-1} \int_S \int_0^\infty (p^{1+\kappa} - [p]^{1+\kappa}) \, d\theta \, dS. \tag{117}$$

Because the integrand in (109) is approximately quadratic in p, the $ZAPE$ is approximately equal to the amount of APE which would exist if p were replaced everywhere by $P + p^\star$.

In most computations the approximate expression (110) for APE has been used in place of (109). The corresponding approximations for $ZAPE$ and $EAPE$ are

$$A_z = \frac{1}{2} \, c_p \left\{ \Gamma_d \, (\Gamma_d - \widetilde{\Gamma})^{-1} \, \widetilde{T}^{-1} \, [T]''^2 \right\}, \tag{118}$$

$$A_E = \frac{1}{2} \, c_p \left\{ \Gamma_d \, (\Gamma_d - \widetilde{\Gamma})^{-1} \, \widetilde{T}^{-1} \, T^{\star 2} \right\}, \tag{119}$$

where, according to the usual convention, $[T]$ is the average temperature along a latitude circle on an isobaric surface, and $T^\star = T - [T]$.

The recognition of additional forms of energy makes it possible to investigate a more detailed energy cycle. We must first observe that when a complicated physical process affects several forms of energy, it is not always possible to define the rate at which one form is converted into another by this process. If however the process can be resolved into simpler processes, each of which affects only two forms of energy, the conversion rate by each process can be defined. The resolution of the original process into simpler processes may at times seem somewhat arbitrary, but ordinarily it will be no more arbitrary than the resolution of energy which made it necessary.

Accordingly, the process which generates APE may be resolved into a heating at warmer latitudes and a cooling at colder latitudes, which generates $ZAPE$, and a heating of warmer regions and cooling of colder regions at the same latitude, which generates $EAPE$. Likewise, the conversion process may be resolved into a sinking in colder latitudes and rising in warmer latitudes, converting $ZAPE$ into ZKE, and a sinking of colder air and rising of warmer air at the same latitude, which converts $EAPE$ into EKE. Finally, the dissipation may be resolved into dissipation of ZKE by the zonally averaged friction and dissipation of EKE by the departure of friction from its zonal average.

With sufficient resolution of the physical processes, there is no process which converts $ZAPE$ into EKE, or $EAPE$ into ZKE (see Lorenz, 1955). However, there remain the processes which can convert $ZAPE$ into $EAPE$ without affecting KE, and ZKE into EKE without affecting APE.

The latter process consists of a horizontal or vertical transport of absolute angular momentum by the eddies toward latitude circles of lower angular velocity, in the direction which would be expected if the large-scale eddies behaved in the manner of classical turbulence. Likewise, the former process consists of a horizontal or vertical transport of sensible heat by the eddies toward latitude circles of lower temperature (actually lower $[T]''$), again in the manner suggested by classical turbulence theory.

Presumably both ZKE and EKE are dissipated by friction and therefore require sources. There must be a net conversion of $ZAPE$ into ZKE or $EAPE$ into EKE, but it is not necessary that both conversions proceed in this direction, since one form of KE could serve as the needed source for the other. Likewise there must be a net generation of $ZAPE$ or $EAPE$ by heating, but it is not necessary that both forms be generated, since one form of APE could serve as the source for the other, or KE could serve as the source for one form of APE. Thus, unlike the basic energy cycle, the directions in which the various steps of the more detailed energy cycle proceed cannot be deduced in any simple manner from existing theory, and have been ascertained only from observations.

The numerous observational studies of the energy cycle are in fair qualitative agreement. First, the heating at low latitudes and the cooling at high latitudes are very effective in generating $ZAPE$. Whether $EAPE$ is produced or destroyed by heating is less certain. Next, the eddy transport of heat is mainly toward colder latitudes, as classical turbulence theory would suggest, so that $ZAPE$ is converted into $EAPE$. On the other hand, the eddy transport of angular momentum is on the average toward latitudes of higher angular velocity, so that EKE is converted into ZKE. This result is just the opposite of what would be predicted by classical turbulence theory. Well before the introduction of the concept of APE, it was deduced by Kuo (1951) on the basis of observed winds over North America.

It follows that $EAPE$ must be converted into EKE, since there is no other source for EKE. Whether $ZAPE$ is converted into ZKE or vice versa is less certain. The low-latitude Hadley cells must act to convert $ZAPE$ into ZKE, while the middle latitude Ferrel cells have the opposite effect. It seems likely that the effect of the weak Ferrel cells is greater than that of the stronger Hadley cells, because the former occur in regions of stronger horizontal temperature contrast.

The conversion of EKE into ZKE implies that in a sense the large-scale eddying motion is an unmixing process. Any attempt to deduce the circulation by treating the over-all mechanical effect of the eddies as large-scale turbulent friction would fail unless it assumed a negative coefficient of turbulent viscosity. The phenomenon of negative viscosity has been one of the most unexpected and perhaps one of the most important recent meteorological discoveries; some evidence for it has subsequently been found in other physical systems, ranging in size from small laboratory models to Jupiter and the Sun.

Figure 54 illustrates the detailed energy cycle. The directions of the arrows indicate the directions in which the various processes proceed, according to the consensus of investigators.
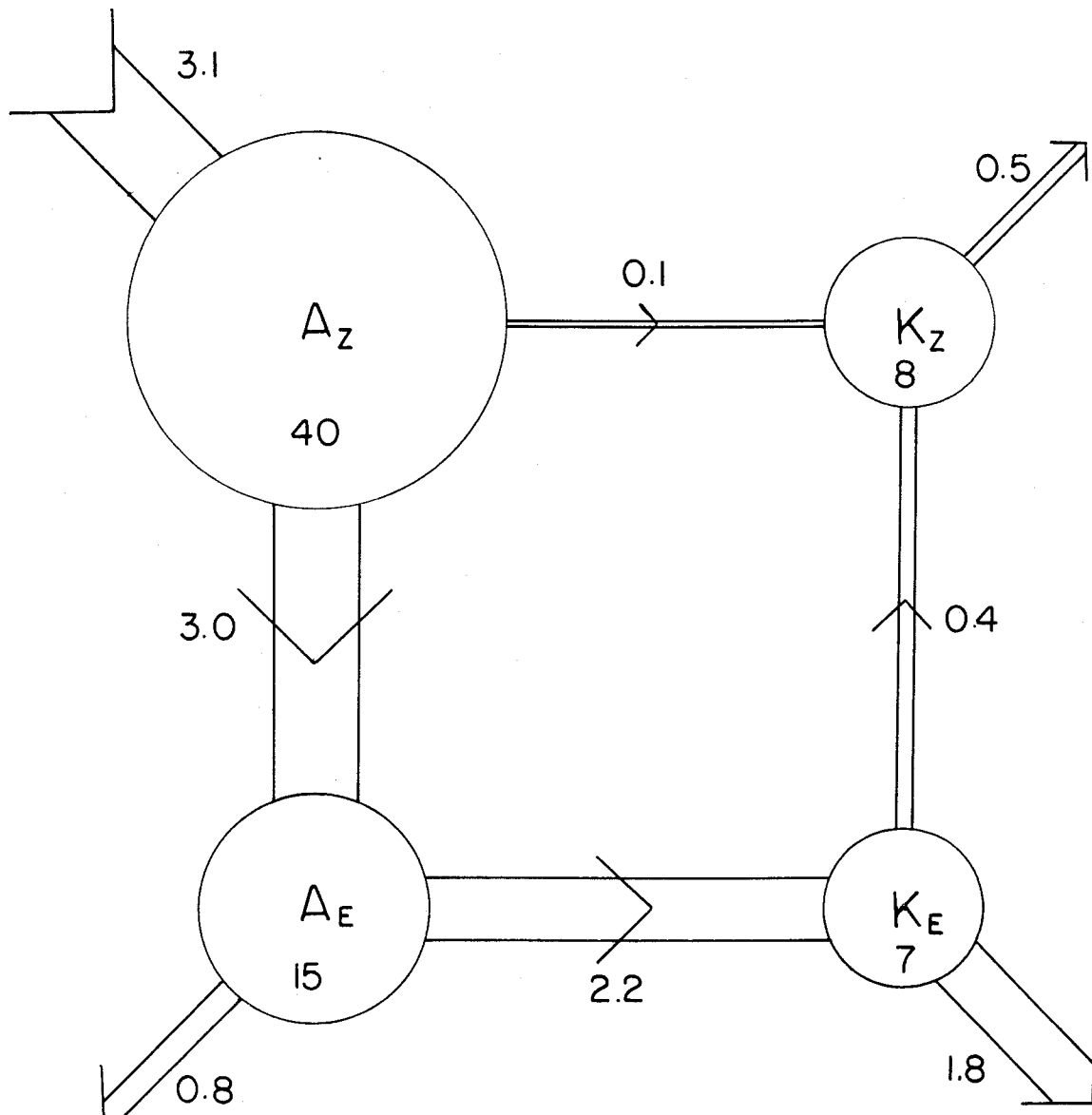

Figure 54. — The energy cycle of the atmosphere as estimated by Oort (1964a). Values of energy are in units of 10^5 joules m^{-2}, and values of generation, conversion, and dissipation are in watts m^{-2}. The estimated value of conversion from A_Z to K_Z is smaller than the probable error of the estimate

To compute the numerical values we require suitable mathematical expressions. Most of the estimates so far performed have been based upon the approximate expressions given by Lorenz. We first let

$$\partial A_z / \partial t = G_z - C_z - C_A , \tag{120}$$

$$\partial A_E / \partial t = G_E - C_E + C_A , \tag{121}$$

$$\partial K_z / \partial t = C_z - C_K - D_z , \tag{122}$$

$$\partial K_E / \partial t = C_E + C_K - D_E . \tag{123}$$

Equations (113), (102) and (101) express G, C, and D as covariances; the corresponding zonal and eddy generations, conversions, and dissipations are simply covariances of zonally-averaged quantities or eddy quantities. Thus

$$G_z = \left\{ \Gamma_d \, (\Gamma_d - \widetilde{\Gamma})^{-1} \, \widetilde{T}^{-1} \, [Q]'' \, [T]'' \right\} , \tag{124}$$

$$G_E = \left\{ \Gamma_d \, (\Gamma_d - \widetilde{\Gamma})^{-1} \, \widetilde{T}^{-1} \, Q^\star \, T^\star \right\} , \tag{125}$$

$$C_z = - \left\{ [\omega] \, [\alpha] \right\} , \tag{126}$$

$$C_E = - \left\{ \omega^\star \, \alpha^\star \right\} , \tag{127}$$

$$D_z = - \left\{ [\mathbf{U}] \cdot [\mathbf{F}] \right\} , \tag{128}$$

$$D_E = - \left\{ \mathbf{U}^\star \cdot \mathbf{F}^\star \right\} . \tag{129}$$

The two new processes are the conversion from ZKE to EKE,

$$C_K = - \left\{ a \, \cos \varphi \left([u^\star \, v^\star] \frac{1}{a} \frac{\partial}{\partial \varphi} + [u^\star \, \omega^\star] \frac{\partial}{\partial p} \right) \left[\frac{u}{a \, \cos \varphi} \right] \right\}$$
$$- \left\{ a \, \cos \varphi \left([v^{\star 2}] \frac{1}{a} \frac{\partial}{\partial \varphi} + [v^\star \, \omega^\star] \frac{\partial}{\partial p} + \frac{\tan \varphi}{a} [u^{\star 2} + v^{\star 2}] \right) \left[\frac{v}{a \, \cos \varphi} \right] \right\}, \tag{130}$$

and the conversion from $ZAPE$ to $EAPE$

$$C_A = - c_p \left\{ \widetilde{T}^{-1} \widetilde{\theta} \left([T^\star \, v^\star]'' \frac{1}{a} \frac{\partial}{\partial \varphi} + [T^\star \, \omega^\star]'' \frac{\partial}{\partial p} \right) \left(\Gamma_d \, (\Gamma_d - \widetilde{\Gamma})^{-1} \, \widetilde{\theta}^{-1} \, [T]'' \right) \right\} . \tag{131}$$

Since $[v]$ tends to be small, the second line in (130) may ordinarily be disregarded. In deriving (131), we have considered the influence of the eddies only on the factor $\widetilde{T''^2}$ in the approximate expression (110) for A.

The numerical values in Figure 54 are those given by Oort (1964a). They are based upon a comprehensive assessment of all previously published numerical values.

Undoubtedly Oort's figures will not be the final word. Dutton and Johnson (1967), for example, have noted the dangers in using the approximate formula (113) to evaluate G. In particular, they note that surface heating in higher latitudes should produce APE, since N is slightly positive, whereas according to (113) surface heating will destroy APE, since T'' is negative. Accordingly they have estimated G from the exact formula (112), and their value is 5.6 watts m^{-2}. Since their estimates of N and Q are based upon seasonal averages, they assume that their result represents the contribution of G_z, and, on the basis of Kung's estimate of G, which exceeds Oort's by a factor of 3, they assume that G_E is positive.

Figure 53 also indicates the long-term average amounts of the various forms of energy. The numerical values are again those of Oort. The total KE, 15×10^5 joules m^{-2}, is equal to the amount which would exist if the wind had a speed of 17 m sec^{-1} everywhere. Even on such a basic quantity there is a lack of agreement; some studies suggest that it is nearly twice as large, while Holopainen's computations (1966) based upon Crutcher's charts (1959) indicate that it is 20 per cent smaller.

A number of investigators have recently subjected APE and KE to still further resolutions, in order to gain further evidence as to how the circulation operates. The number of possible resolutions seems to be almost limitless. Oort (1964a) has investigated the resolution of KE and APE into the amounts associated with the time-and-longitude averaged fields of motion and temperature and the amounts associated with the remaining fields, noting that this resolution facilitates the computation procedure. Qualitatively the energy cycle is much the same, but quantitatively it is different. Wiin-Nielsen (1962) has resolved KE into the amounts associated with the vertically averaged flow and the departure from the vertical average, and has found that APE is converted primarily into the latter form of KE.

A number of studies have been concerned with the resolutions of $EAPE$ and EKE and the processes affecting them into the amounts associated with each wave number. The wave numbers are defined by means of a Fourier analysis with respect to longitude. The appropriate equations have been presented by Saltzman (1957).

The most complete study of this sort in terms of length of record has been performed by Saltzman and Teweles (1964), who have used nine years of daily 500-mb data to resolve the conversion C_K into the amounts accomplished separately by wave numbers 1 to 15 inclusive. They find that all wave numbers contribute negatively to C_K, with a peak contribution at wave numbers 2 and 3 and another peak at wave numbers 6 and 7. The former peak is the dominant one in winter and is nearly absent in summer, while the latter peak appears at every season of the year.

Wiin-Nielsen, Brown and Drake (1964) performed similar computations at five levels, using a total of eight months of data, and found that the great majority of conversions by individual wave numbers for individual months were positive, although there were some notable exceptions. Their results indicated that the conversion processes might not be very reliably determined from data at only one level. In addition they performed a resolution of the conversion C_A, and found that all wave numbers contributed positively.

Measurements of the conversion C_E are hindered by our lack of knowledge of the true vertical-motion field, but Saltzman and Fleisher (1961) used six months of daily values of ω for the 850–500 mb layer, as evaluated by the ω-equation, and obtained a resolution of the conversion process. Again all wave numbers contributed positively to a conversion of $EAPE$ into EKE, with the peak contribution occurring at wave number 6.

The study of the energetics of a particular portion of the atmosphere is complicated by the possibility of a flow of mass as well as energy across the boundary. Much speculation has arisen concerning the maintenance of the circulation of the stratosphere following the discovery by White (1954) of the counter-gradient flux of sensible heat, which acts in the proper direction to maintain the equatorward temperature decrease. Oort (1964b) has studied the energy cycle of the layer of the stratosphere between 100 and 30 mb, using one year of data, and has found that within this layer the processes contribute to a net conversion of EKE into ZKE, EKE into $EAPE$, and $EAPE$ into $ZAPE$. There is therefore no source within the region for EKE, and Oort concludes that the stratospheric eddies must be mechanically forced by the air above or below, and presumably by the tropospheric eddies. Radiation also serves as a sink for $ZAPE$. The lower stratospheric energy cycle therefore seems to differ considerably from

that of the atmosphere as a whole, and the stratosphere appears to act as a heat engine in reverse, or a thermodynamic refrigerator, converting KE into APE.

In considering the basic energy cycle we faced the problem of explaining its intensity. With the recognition of the more detailed energy cycle there is the further problem of accounting for the directions in which the various steps proceed. Here we may again invoke the hypothesis that the general circulation is constrained to operate at nearly maximum efficiency.

The energy cycle can operate at its maximum rate only if the cross-latitude temperature contrast is considerably less than the contrast which would occur under thermal equilibrium — perhaps half as great. As we noted in the previous chapter, in the absence of eddies the meridional circulation would have to be extremely weak in order not to lead to upper-level westerly winds in excess of those permitted by the thermal wind relation. It could therefore transport only enough energy from warm to cold latitudes to reduce the temperature contrast slightly below its thermal-equilibrium value. If the energy cycle is to proceed at nearly maximum efficiency, eddies must therefore occur.

We do not offer this suggestion as an alternative to the hypothesis that the presence of eddies is attributable to instability. It is more properly an alternative statement of the same hypothesis. Instability is still the mechanism through which the eddies would develop and allow the cycle to proceed more efficiently.

It would seem reasonable to pass to the conclusion that the eddies, whose presence is demanded because the unaided meridional circulation cannot accomplish the needed energy transport, will now form the mechanism for producing the transport, and will therefore convert $EAPE$ into $ZAPE$. Such a conclusion does not necessarily follow. As we also noted in the previous chapter, as long as eddies are present the meridional circulation need not be weak. Conceivably the meridional circulation could accomplish the needed heat transport, and the function of the eddies could be to prevent the angular momentum aloft from reaching excessive values. The conversions would then be from $ZAPE$ to ZKE to EKE to $EAPE$, just the opposite of those shown in Figure 54. It is only through observations that we know that this is not the case. We are led to conclude that the problem of explaining the direction of the energy cycle is more complicated than it might at first seem to be.

CHAPTER VI

LABORATORY MODELS OF THE ATMOSPHERE

The theoretical study of the Earth's atmosphere is often facilitated by introducing various ideal-izations. Some of these consist of suppressing certain supposedly irrelevant details, so that the important processes may be more readily examined. In other instances certain features which may not be irrelevant at all are nevertheless omitted to render the theoretical treatment less awkward. In the present chapter we shall consider certain real fluid systems other than the Earth's atmosphere which share certain properties with it, and which may serve in one way or another as still further idealizations.

Perhaps the most obvious systems of this sort are the atmospheres of other planets. Unfortunately, definitive measurements of the motions of other planetary atmospheres are hard to obtain. Our ideas have come mainly from tracking identifiable features, which may or may not actually move with the large-scale flow. Clouds have been followed on Mars, and on Jupiter there have been abundant observations of the motions of spots, which themselves appear to be large atmospheric disturbances. The available observations are sufficient to reveal that the circulations of other atmospheres in our solar system do not resemble that of our own atmosphere very closely.

Since the planets differ so greatly in their solar distances and angular velocities and in the compo-sitions of their atmospheres, it is not surprising that the resulting circulations also differ considerably. Nevertheless it is of interest to observe that many of the earlier theories of the general circulation of the Earth's atmosphere made no use of any numerical values. Taken at face value, these theories would then have predicted similar circulations in all of the planetary atmospheres, provided simply that the planets were rotating, with their equatorial planes not too greatly inclined to their orbital planes. Whatever else we may learn from a brief consideration of the atmospheres of other planets, we are force-fully reminded of the importance of quantitative considerations.

There is much to be learned about our own atmosphere from studying our oceans. The analogy between the Gulf Stream and the jet stream is especially revealing. Nevertheless the oceanic circulation as a whole is not a particularly good model of the atmospheric circulation.

In the absence of other easily observed natural systems resembling the atmosphere, further analogues must be confined to man-made systems. Chief among these are the laboratory models which have been specifically designed to simulate the atmosphere. These models consist of differentially heated rotating cylinders containing a fluid. The cylinder represents a hemisphere of the Earth, the fluid represents the atmosphere, and the motion of the fluid is intended to represent the atmospheric circulation.

The primary importance of the models stems from the fact that the experiments, being performed in the laboratory, are subject to a certain degree of control. The shape and size of the container, the nature and amount of the fluid, the distribution and intensity of the external heat sources, and the rate of rotation can all be chosen in advance. Such internal parameters as the average speed of the fluid relative to the container can often be set to desired values by adjusting the controllable parameters on a trial-and-error basis.

The earliest experiments appear to have been those of Vettin (1857), whose apparatus was a rotating cylinder about 30 centimetres in diameter and 5 centimetres deep, containing air. In one experiment ice was placed at the centre. The resulting motion of the air was in its essential features like the atmospheric circulation envisioned by Hadley. Apparently Vettin's work had no immediate successors.

If the arguments advanced by the early writers on the general circulation should be applicable to the atmospheres of other planets, they should be equally applicable to suitably designed laboratory experiments. This fact was recognized by Thomson, who in his Bakerian Lecture (1892) proposed some experiments with an apparatus very much like Vettin's, but containing water. He also noted the desirability of varying some of the controllable parameters. Any plans he might have had to carry out his suggestions were halted by his death soon afterward.

A few more experiments were performed in the early twentieth century. For a more detailed account the reader is referred to Fultz (1951), or to the monograph of Fultz et al. (1959).

The dishpan experiments

The modern era of laboratory simulation began shortly after World War II with the experiments performed at the Hydrodynamics Laboratory of the University of Chicago (see Fultz et al. 1959). The experiments most nearly duplicating the atmosphere are the dishpan experiments. The central piece of apparatus is a cylindrical container — often an ordinary aluminium dishpan — containing water. It is mounted on a rotating turntable. A heat source is provided near the rim, and in the more refined experiments there is a cold source near the centre. The apparatus may easily be duplicated if only qualitative results are desired, but extreme care must be used if useful numerical data are to be obtained. Among the requirements in the latter instance are an almost exactly constant rate of rotation. The motion at the free surface can be made visible for an indefinite length of time by particles of aluminium powder or some other tracer, while internal motions can be temporarily followed by injecting a dye.

Velocities at the free surface are readily measured photographically. If the camera rotates with the apparatus, and a time exposure is made, the tracer particles will appear as streaks. The lengths of the streaks will indicate the speed of the flow. Temperature measurements may be obtained by means of thermocouples.

The various details of the experiments are conveniently designated by their atmospheric counterparts; the centre of the dishpan becomes the North Pole, the rim becomes the Equator, the direction toward the centre becomes north, and the direction toward which the dishpan rotates becomes east. A period of revolution becomes a day. A typical day is actually a fraction of a minute, a typical radius is about 15 cm, and the water is typically about 2 cm deep.

The external parameters which are readily altered without changing the apparatus or adding or removing water are the rate of rotation and the contrast between the heat and cold sources. Over a considerable range of these parameters, the flow which develops in the dishpan is perfectly symmetric about the axis of rotation, within the limits of observational error. In other experiments differing only in the values of the external parameters, a set of large-scale wave-like disturbances develops, and the pattern at the free surface bears considerable resemblance to an upper-level weather map. There are thus two qualitatively different régimes of flow, a zonally symmetric régime and a zonally asymmetric régime, which Fultz has called the Hadley régime and the Rossby régime. Typical circulation patterns occurring in the Hadley and Rossby régimes are shown in Figures 55 and 56, which are photographs of the free surface.
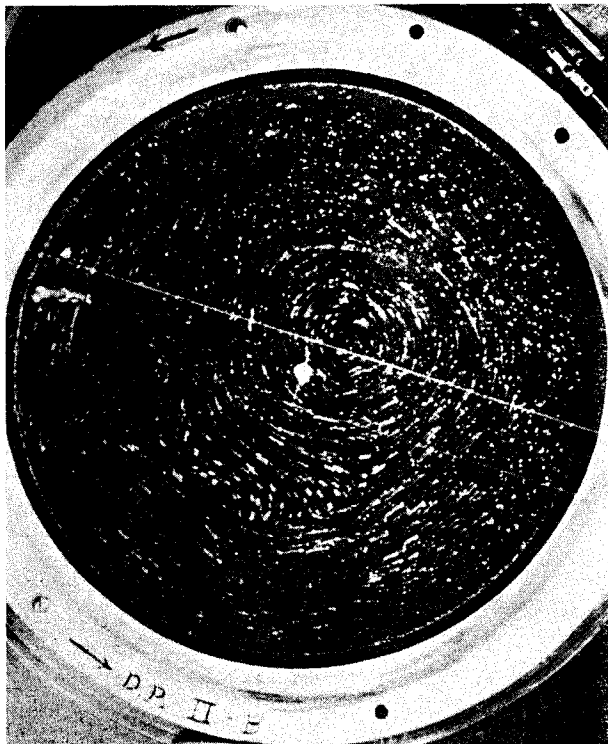
Figure 55. — A photograph of the upper surface of fluid in a rotating dishpan, showing a nearly symmetric circulation pattern. The photograph is a time exposure, so that particles of tracer on the upper surface appear as streaks indicating direction and speed of flow (from Starr and Long, 1953)
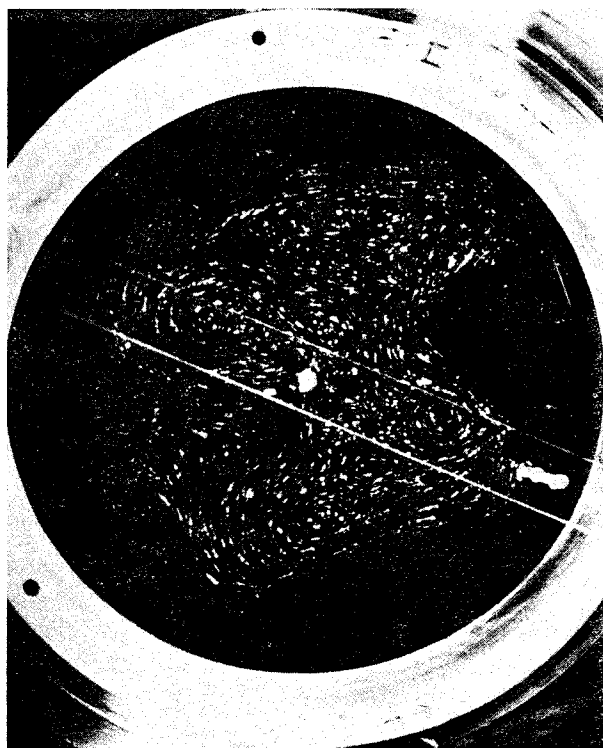

Figure 56. — A photograph of the upper surface of fluid in a rotating dishpan, showing an unsymmetric (Rossby-régime) circulation pattern. The rate of rotation of the dishpan is higher than in Figure 55(from Starr and Long, 1953)

In the earlier experiments it was found that with sufficiently slow rotation only Hadley flow would develop. With faster rotation, Hadley flow would also develop under a strong heating contrast, but with a weak heating contrast, Rossby flow occurred. Later it was found that Hadley flow could also be produced with extremely weak heating.

We have already noted that the arguments presented by Hadley (1735) and some of the nineteenth century meteorologists ought to be equally applicable to the dishpan. It is therefore of considerable interest to note that the dishpan may contain a flow bearing considerable resemblance to the one described by Hadley. Perhaps more than any other discovery, the dishpan has exonerated Hadley's ideas from some of the criticisms raised against them. Hadley's paper is a nearly correct account of what sometimes occurs in the dishpan, although it can never be a demonstration that this type of flow must occur in preference to some other one.

From the point of view of the general circulation, perhaps the most important question to be answered is whether the resemblance of the Rossby-régime flow in the dishpan to the circulation of the atmosphere is more than superficial. Certainly under proper conditions the free surface looks like a weather map at the tropopause level. There is generally a large irregular circumpolar vortex, surrounded by a narrow meandering jet stream. The wave-like disturbances in the westerly flow progress about the centre in much the same manner as the upper-level waves in the atmosphere.

Further similarities appear in the flow below the free surface. By injecting a dye, Fultz (1952) found evidence for the existence of fronts and migratory cyclones, which moreover appeared to be properly located with respect to the upper-level waves. Using a much larger apparatus, Faller (1956) was able to observe extensive frontal surfaces, which possessed families of wave disturbances whose structure and evolution closely resembled the classical Norwegian cyclone model.

Evidence that the dishpan resembles the atmosphere in other respects was provided by Starr and Long (1953). Using velocity measurements at the free surface obtained from a sequence of 108 photographs, they evaluated the northward transport of angular momentum at six different latitudes. They found an average northward transport at all latitudes, with a peak value in low latitudes occurring near the latitude of maximum westerly "wind". Furthermore nearly all of the transport was accomplished by the eddies. It thus appears that eddies in the dishpan and in the atmosphere play similar roles in the angular-momentum balance.

The production of kinetic energy in the dishpan must be accomplished by the pressure forces, yet the differences in pressure are so minute that direct measurements would be difficult to obtain. The hydrostatic and geostrophic relations therefore cannot be directly confirmed. Temperature measurements are easily made, however, and one can confirm the geostrophic relation by confirming the thermal wind relation and assuming that hydrostatic equilibrium must prevail in any case. In the dishpan the thermal-wind relation assumes the form

$$\partial \mathbf{U}/\partial z = \frac{1}{2}\, \varepsilon g\, \Omega^{-1}\, \mathbf{k} \times \nabla T\,, \tag{132}$$

where $\varepsilon = d\,(1n\,a)/dt$ is the coefficient of thermal expansion. Measurements in a dishpan with a radius of 15 centimetres, where the rim-to-centre contrast may be about $10°$, show that nearly half of this contrast may occur across a jet stream about 1 cm wide; furthermore, under the assumption that $\mathbf{U} = 0$ at the bottom, the thermal-wind relation is found to be very closely satisfied.

The circulation in the dishpan therefore resembles the atmospheric circulation in many important respects. Some features of the atmosphere of course are not reproduced. Notable among these is the

tropical circulation; the dishpan has a constant Coriolis parameter, and the tropical and temperate-latitude circulations cannot be modelled simultaneously. The stratosphere, which depends upon radiative heating in the ozone layer, is not present in the dishpan, and phenomena depending upon changes of phase of water are not reproduced.

According to the usual conditions for dynamic modelling, the transition between the Hadley and Rossby regimes should depend upon the values of certain dimensionless ratios involving the rotation and the heating contrast, rather than directly upon dimensional values of these quantities. The governing equations in dimensionless form contain a number of dimensionless parameters; among these, the values of the Taylor number T_a and the thermal Rossby number R_{OT} seem to have the greatest influence on the results of an experiment.

The Taylor number is defined as

$$T_a = 4 \, \Omega^2 \, h^4 \, \nu^{-2} \, , \tag{133}$$

where h is the depth of the fluid and ν is the kinematic viscosity. The square root of T_a is a measure of the ratio of Coriolis forces to viscous forces, and has been called the "rotation Reynolds number". Because of the great depth and low molecular viscosity of the atmosphere, the atmospheric Taylor number cannot be conveniently duplicated in the dishpan, and it had been argued that for this reason the dishpan could not simulate the atmosphere. When the Chicago experiments were undertaken despite this warning and proved successful, an explanation was needed. The most plausible explanation seems to be that the effective viscosity in the atmosphere is the turbulent viscosity, which is generally at least 10^5 times as great as molecular viscosity. Defined in terms of turbulent viscosity, a typical atmospheric Taylor number is about 10^7, which is readily obtained in the dishpan.

The thermal Rossby number may be defined as

$$R_{OT} = \frac{1}{2} \, g\varepsilon \, h \, \Omega^{-2} \, a^{-2} \, \Delta T \, , \tag{134}$$

where ΔT is the rim-to-centre difference of the vertically averaged temperature. It is evident from (134) that R_{OT} depends upon both heating and rotation; the dimensionless parameter which depends upon temperature contrast alone is the product $T_a R_{OT}$.

In a more general rotating fluid system the Rossby number R_0 is often defined as the ratio of a suitably chosen relative velocity to a suitably chosen absolute velocity. Such a ratio was first used in connection with the atmosphere by Kibel (1940). In the dishpan it is convenient to let

$$R_o = u_1 \, \Omega^{-1} \, a^{-1} \, , \tag{135}$$

where u_1 is the average value of $[u]$ with respect to latitude, $[u]$ being the average of u with respect to longitude, at the free surface. The thermal Rossby number R_{OT} has been defined in such a way that it will reduce to the Rossby number R_0 if the thermal wind relation is valid, and if the flow near the bottom is negligibly small; this may be seen by comparing (132), (134), and (135).

It should be noted that R_{OT} is strictly speaking an internal parameter; it is used because of the obvious difficulties in measuring a meaningful external temperature contrast when the heating element may be a Bunsen burner or an electrical coil. Thus a pre-chosen R_{OT} cannot be directly entered into an experiment, although it can generally be established by a trial-and-error procedure. Nevertheless it is often convenient to think of an experiment as being varied by varying the thermal Rossby number.

A typical atmospheric value of R_0 is about 0.03. Except for low values of T_a, Hadley flow is generally not observed in the dishpan for values of R_{OT} below 0.3. The experiments therefore definitely imply that the atmospheric circulation should lie within the Rossby régime.

The annulus experiments

At the time that the early experiments at Chicago were in progress, Hide (1953) was performing somewhat similar experiments at Cambridge University, using a deep annular container rather than an open dishpan. Hide was interested in simulating the Earth's core, but he recognized the meteorological significance of some of his results, and ultimately his experiments influenced the work of all others in the field.

From the meteorological point of view Hide's most significant discovery was probably that of the occurrence of flow patterns containing chains of identical waves, which progressed about the axis at a uniform rate without altering their shape, in sharp contrast to the patterns in the open dishpan, which seemed no more regular or periodic than the atmosphere itself. Theoretical meteorologists had been using such waves as mathematical idealizations since Rossby (1939) had introduced them in his famous paper, and the idea was sometimes expressed that since the patterns in the atmosphere were always irregular, Rossby's sinusoidal waves were no more than fantasy. Hide's experiments definitely showed that Rossby's ideas could apply to real fluid systems, which, although lacking the unpredictability of the atmosphere, were at least driven by an analogous mechanism.

More important from the practical point of view was the opportunity provided for detailed experimental measurements. Although motions at the free surface of the dishpan can be measured photographically, it is generally not feasible to scatter thermometers or "anemometers" throughout the interior, and the temperatures are measured at only a few interior points at once, while the internal flow may not be measured at all. When the flow is non-periodic, only the long-term statistics can be measured in detail, and these require an experiment of extended duration, sufficient to allow measurements during a representative collection of "weather situations". There is a slight advantage over real atmospheric studies in that the long-term statistics should not vary from one longitude to another.

Waves moving without changing their shape form a steady-state flow in a co-ordinate system moving with the waves. Aside from experimental errors, a single photograph is sufficient to measure the free-surface velocities, while an instantaneous temperature field can be constructed from temperature measurements taken a few at a time. This possibility has been exploited by Riehl and Fultz (1958), who have determined three-dimensional distributions of temperature and motion in considerable detail. Even when only the statistics are desired, the labour is greatly reduced by the elimination of the major sampling fluctuations.

The flow patterns discovered by Hide form a sub-régime of the Rossby régime. This may be called the "steady Rossby régime", although it must be noted that this term has also been used to describe an ordinary Rossby régime which has attained a statistically steady state following the modification of some external parameter.

When all the waves are identical, there must be a definite number of waves. The wave number is then one characteristic of the flow. It is possible within the same annular apparatus to change the number of waves by changing the external conditions. In general, lowering the thermal Rossby number increases the wave number. There are well-marked transitions from one wave number to another, so that the steady Rossby régime may be further divided into sub-régimes, one for each wave number.

Figure 57 shows the Hadley-Rossby transition, and the transitions between wave numbers, as determined experimentally by Fultz *et al.* (1964) using an annular apparatus like the one first used by Hide. (We shall presently consider the somewhat similar Figure 58.) To obtain the transitions, the rotation rate was held fixed, while the heating contrast, starting at zero, was increased in steps until the Hadley régime was re-established. The procedure was then repeated for different fixed rotation rates. It is noteworthy that the wave-number transition curves are marked by nearly constant thermal Rossby numbers. (For an annulus, the factor a^{-2} in (134) must be replaced by $a^{-1} (a - b)^{-1}$, where b is the inner radius.)

A further striking discovery of Hide's was a phenomenon which he named "vacillation". Here the waves are not steady in a moving co-ordinate system, but they alter their shape and speed of progression in a regular periodic manner, returning to their original configuration at the completion of a vacillation cycle. This phenomenon aroused immediate interest among meteorologists because of its resemblance to the fluctuations of the zonal index, a quantity introduced, incidentally, by Rossby in the paper just cited. Like the steady waves, the idea of an index cycle had been criticized as an over-idealization. It was pointed out, for example, that the variations of the circulation were more nearly random than cyclic. Hide's experiments clearly showed that regular predictable fluctuations in real fluid systems were by no means preposterous.

Since a vacillating pattern repeats at regular intervals, it is possible in this case also to measure the three-dimensional temperature field at any phase of the vacillation cycle, and thence to obtain long-term statistics for a non-steady circulation, without the usual sampling difficulties. The three-dimensional wind field is most readily determined by measuring the wind at the free surface, and deducing the wind below from the thermal wind equation.

Figures 59-62 show the appearance of the free surface at four phases of a vacillation cycle in an experiment recently performed by Fultz. (A somewhat similar experiment is illustrated by Fultz *et al.*, 1959, pp. 94-95.) The vacillation period in this case was $16\frac{1}{4}$ revolutions; the successive photographs are separated by 4 revolutions. Almost as striking as the pronounced change in the shape of the waves from one phase to another is the nearly identical shape possessed by the five separate waves at each phase.

In Figure 59 the nearly due-north winds to the left of the troughs, together with the south-westerly winds to the right, indicate a strong northward transport of angular momentum. By the time of Figure 60 this transport has ceased and reversed its sign. The increased westerlies at lower latitudes and decreased westerlies at higher latitudes, which result from the southward momentum transport, manifest themselves in the transformation of the open troughs into closed cyclonic centres in Figure 61. By this time the southward transport of momentum has ceased, and the transport has become decidedly northward in Figure 62, so that four days later, when the pattern is again as in Figure 59, the troughs have opened up again. Measurements have revealed that the poleward transport of heat undergoes similar fluctuations during the vacillation cycle.

Implications of the experiments

Entirely aside from any resemblance which they may bear to the atmosphere, the experiments pose a basic theoretical problem which demands an explanation. This problem concerns the reasons for the existence of separate régimes of flow, and the abrupt transitions between them. Of main interest is the transition between the Hadley and Rossby régimes. For definiteness the "upper transition" appearing in the upper portion of Figure 57 may be considered. The following discussion must be regarded as largely descriptive rather than truly explanatory.

Figure 57. — Transitions between Hadley (symmetric) and Rossby régimes, and transitions between wave numbers within the Rossby régime, occurring in rotating-annulus experiment of Fultz *et al.* (1964), as the thermal Rossby number R_{OT} is *increased* while the Taylor number remains fixed. Dashed curves indicate that the location of the transition is somewhat uncertain. Parameter $\Omega^2\, a\, g^{-1}$ is proportional to the Taylor number for a given apparatus with a given amount of fluid
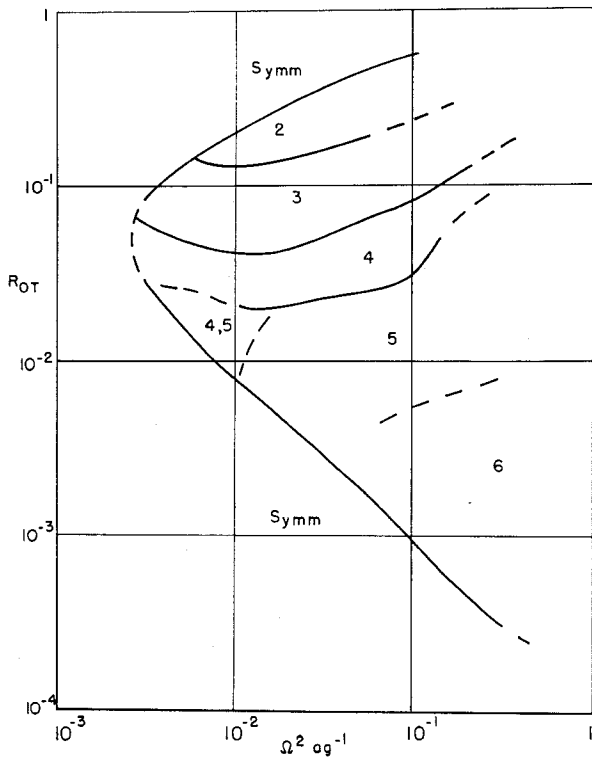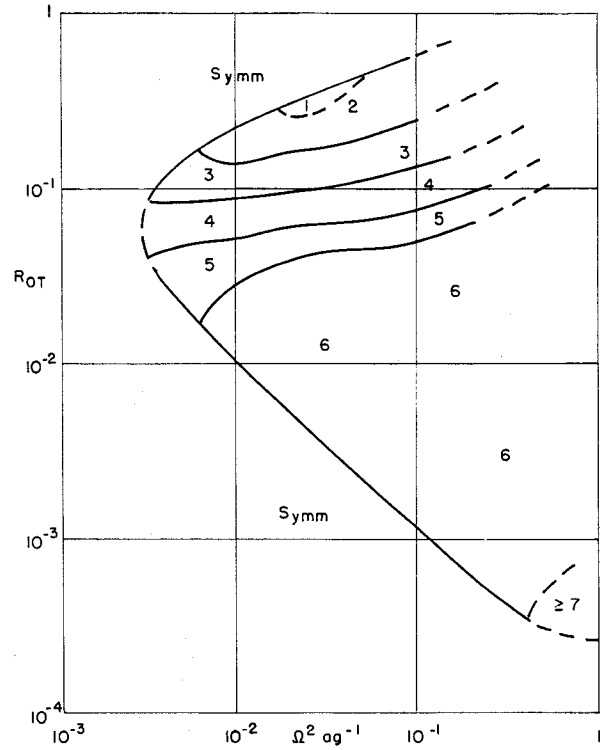

Figure 58. — Transitions between Hadley (symmetric) and Rossby régimes, and transitions between wave numbers within the Rossby régime, occurring in same rotating-annulus experiment as in Figure 57, as the thermal Rossby number is *decreased* while the Taylor number remains fixed
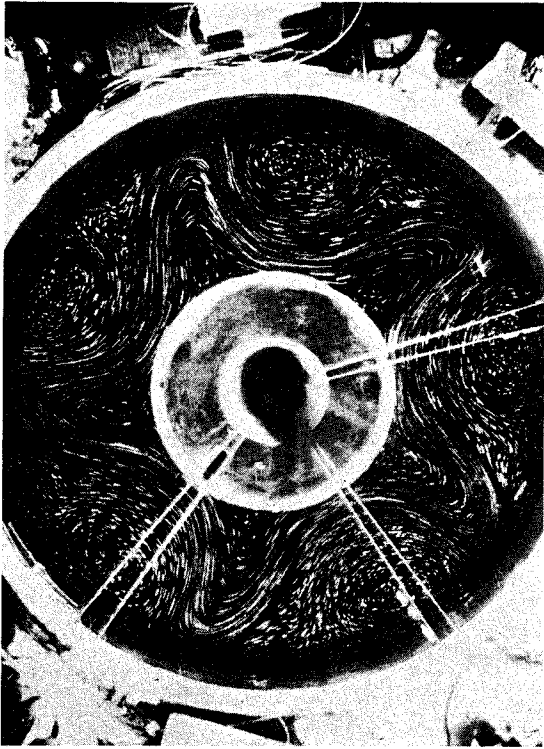
Figure 59. — Photograph of upper surface of fluid in a rotating annulus, showing a five-wave Rossby-régime circulation pattern. The photograph is a time exposure, so that particles of tracer on the upper surface appear as streaks indicating the direction and speed of flow. The circulation pattern is vacillating with a period of $16\frac{1}{4}$ revolutions (photo by Dave Fultz)
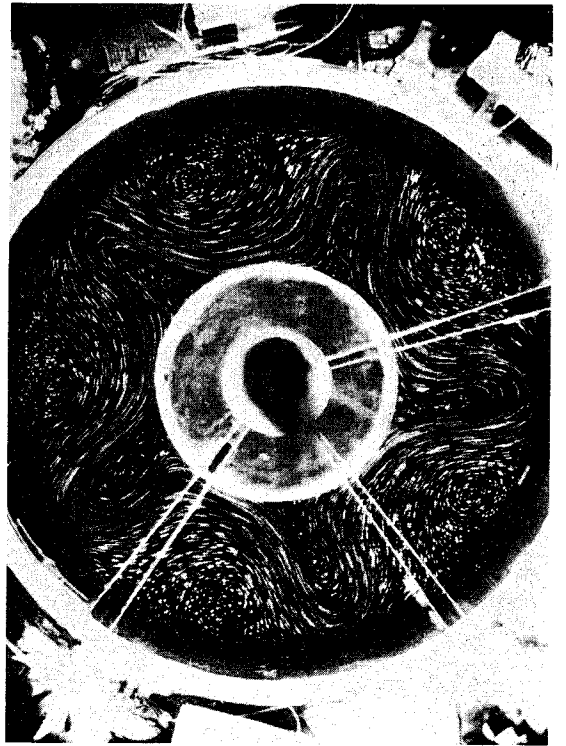

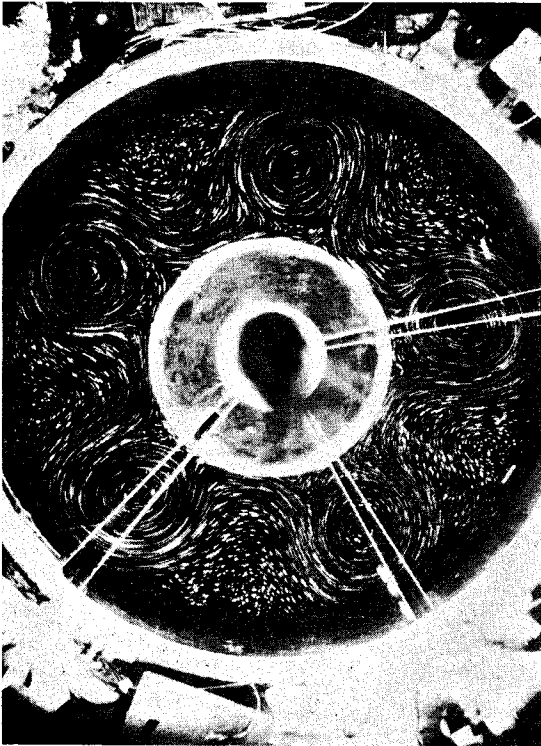Figure 60. — The same as Figure 59, four revolutions later

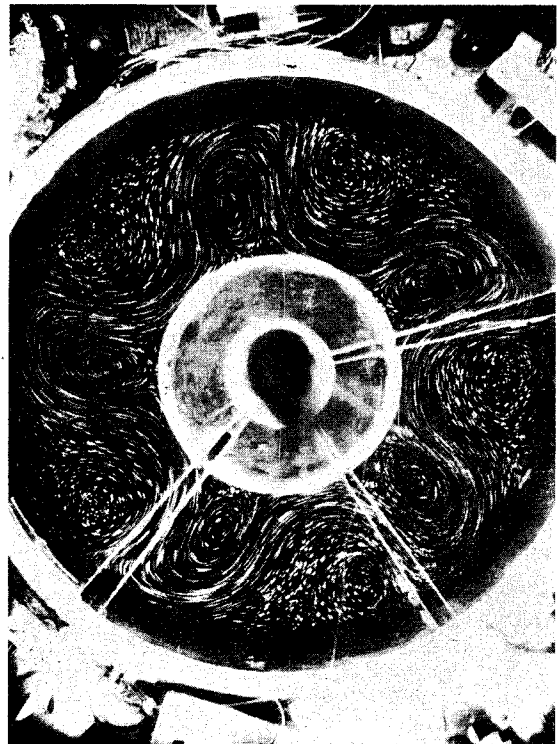Figure 61. — The same as Figure 59, eight revolutions later

Figure 62. — The same as Figure 59, twelve revolutions later

It is evident that for any set of conditions under which zonally symmetric flow is observed, the flow must constitute a symmetric solution of the mathematical equations, and that moreover this solution must be stable with respect to sufficiently small perturbations. For any set of conditions where the Rossby régime is observed, there appear to be three different possibilities:

1. Symmetric flow is mathematically impossible; there is no steady-state symmetric solution of the equations.

2. Steady symmetric flow is mathematically possible, but it is unstable with respect to small asymmetric perturbations; these perturbations therefore develop into finite waves when the critical conditions are exceeded.

3. Steady symmetric flow is mathematically possible and is also stable, but the system is intransitive, possessing at least two "general circulations"; unsymmetric flow is possible also.

The first of these possibilities had been suggested during the course of the early experiments, but the arguments in favour of it do not seem very strong. It may be recalled that once Hadley flow was found not to be present in the atmosphere, the arguments advanced to show the impossibility rather than the instability of Hadley flow proved fallacious. In any event either steady or oscillatory symmetric flow must be mathematically possible; one need but choose zonally symmetric initial conditions and the symmetry will be preserved.

The second possibility, which is much like the argument presented by Bjerknes (1921) to account for the large disturbances in the atmosphere, was proposed in connection with the dishpan experiments by Lorenz (1956). He noted that for symmetric flow the primary effect of stronger heating would be to produce a stronger temperature contrast, with accompanying stronger vertical wind shear. According to the usual criteria for baroclinic instability (which is discussed in more detail in Chapter VIII), this should favour instability rather than stability. He therefore sought a second-order or non-linear effect through which stronger heating could increase the stability. Such an effect appears to be the transport of heat by the meridional circulation, which conveys warm fluid across the top and cold fluid across the bottom, and thereby creates a stable stratification, which favours baroclinic stability. Since the vertical stability is proportional to both the strength of the meridional circulation and the temperature contrast, it is proportional to the square of the heating contrast, neglecting still higher-order effects, and for sufficiently strong heating it should offset the destabilizing effect of the vertical shear.

However correct this explanation may be in some instances, more recent experiments have shown that for certain combinations of rotation and heating the third explanation is the proper one. Fultz et al. (1959, p. 78) describe one case in detail. In the particular apparatus, the Rossby régime ordinarily occurred when R_{OT} fell below about 0.5. However, when R_{OT} was decreased very carefully by increasing the rotation rate in small steps, allowing time for adjustment of the flow between steps, the Hadley régime was preserved until R_{OT} fell to about 0.3. Between these two values, the symmetric circulation was stable in the ordinary sense, since moderate disturbance would not destroy it. Yet when the water was stirred violently with a rod for about one second, a pattern of three waves developed and remained. This was evidently the same flow which would have developed in any case if less care had been taken in decreasing the rotation rate.

Initial conditions consisting of a symmetric flow plus small but not infinitesimal disturbances have a finite rather than a zero probability of being selected by chance. More irregular initial conditions leading to the Rossby régime also have a finite probability of being selected by chance. The experiment therefore provides concrete evidence that thermally forced rotating fluid flows may be intransitive, and incidentally suggests that one is not necessarily on safe ground in assuming that the atmosphere is transitive.

Rossby flow is therefore not restricted to those instances when the mathematically possible Hadley flow is unstable. It may occur mainly when Hadley flow is unstable, but for some external conditions Rossby flow is simply an alternative to Hadley flow. The instability of Hadley flow is a sufficient condition for Rossby flow, but not a necessary condition.

In the open dishpan the principal abrupt transition is from the Hadley to the Rossby régime. The Rossby flow tends to be irregular and non-periodic, and transitions from one set of statistics to another, as the external conditions change, seem to be slow and continuous rather than abrupt. In the annular experiments, where many qualitatively distinct regular periodic flows occur, the transitions may be rather sharp. Here also, intransitivities have been experimentally observed.

The curves in Figure 57 show the transitions to lower wave numbers which occur in one particular apparatus as the heating is increased, while the rotation rate remains fixed. The transitions to higher wave numbers as the heating is decreased again are shown in Figure 58. These invariably occur at lower values of R_{OT}; thus there are external conditions under which either of two consecutive wave numbers may occur.

Like the Hadley-Rossby transition, the transitions between wave numbers may be described in terms of stability and instability. It is not sufficient in this case to consider only the stability of a steady-state symmetric flow; the stability of a time-dependent flow with respect to still further disturbances is involved. Consider two adjacent wave numbers, say numbers three and four. An equilibrium Hadley flow may be unstable with respect to disturbances having wave number three only, or four only, or it may be unstable with respect to both disturbances, or neither. If it is unstable with respect to both wave numbers, a Rossby flow containing three waves, and another one containing four, will each be mathematically possible. These Rossby flows, as opposed to the mathematically possible Hadley flow alone, may be unstable with respect to further disturbances having respectively four or three waves. If the three-wave flow is unstable with respect to four-wave disturbances, and vice versa, neither three-wave nor four-wave flow can exist by itself experimentally, and (barring still further wave numbers) the resulting flow will be somewhat irregular, as in the open dishpan. If the three-wave and four-wave flows are each stable with respect to disturbances of the other wave number, either pattern can occur and persist, and the system is intransitive. This is evidently what happens in the annulus near the wave number transitions. If only one flow is unstable with respect to disturbances having the other wave number, only the stable flow will be observed. This evidently occurs in the annulus away from the transitions.

Similar considerations seem to characterize the phenomenon of vacillation. When a wave pattern is observed to move without changing its shape, the pattern is stable with respect to further disturbances. Under other conditions, steady waves may still be mathematically possible but unstable. If the pattern is unstable with respect to further disturbances having the same wave number but a different shape, vacillation may be mathematically possible. In that event it will be possible to observe the vacillating flow, unless it is also unstable with respect to still further disturbances, in which case the resulting flow will presumably be rather irregular.

The flow in the dishpan and the annulus has received its most extensive theoretical treatment from Kuo (1957) and Davies (1959). Rather complicated mathematics is involved. That the pertinent physical processes may nevertheless be rather simple is indicated by a study by Lorenz (1962), who was able to establish a highly simplified system of equations giving a crude description of the experiments. The system consists of only eight ordinary differential equations, and it can be solved analytically for the Hadley flow under all conditions, and for the Rossby flow under those conditions where such a flow exists. He obtained a Hadley-Rossby curve looking very much like Fultz's, although it was difficult to assign

an absolute scale. As he had postulated earlier, the vertical stability was the controlling factor in rendering the Hadley flow stable for strong heating. Moreover, the intransitivity of the system near the Hadley-Rossby transition was reproduced.

With the addition of four more variables, Lorenz also obtained curves for the transitions between pairs of consecutive wave numbers. These bore a fair resemblance to the experimentally determined transitions shown in Figures 57 and 58. The intransitivity near the wave-number transitions was not reproduced; instead there were conditions where two wave numbers occurred simultaneously. Most likely the Rossby flow was too crudely represented for its stability with respect to further disturbances to be properly described.

The laboratory experiments presumably tell us more about planetary atmospheres in general than about the Earth's atmosphere in particular. They indicate the variety of flow patterns which can occur, and the conditions favourable to each of these. Since regular flow patterns, other than Hadley flow, occur mainly in the annular experiments, and since an open dishpan is presumably a better planetary analogue than an annulus, the experiments suggest that large-scale planetary flow patterns should be confined to Hadley flow and irregular Rossby flow.

Among the controlled experiments which deserve special mention is one designed to explore the original hypothesis of Halley (1686) to the effect that the trade winds are produced by the diurnal progression of the most strongly heated region about the Equator, rather than by any deflective effect of the rotation. Fultz *et al.* (1959, pp. 36-39) describe an experiment in which a flame was moved in a circular path underneath the rim of a stationary dishpan. In due time westerly winds, i.e. motion opposed to the direction of the flame, developed at the upper surface. At the bottom these were easterlies near the rim and westerlies near the centre, while a single direct meridional cell occupied the entire dishpan. Presumably the corresponding effect in the atmosphere is insignificant by comparison with the deflective effect of the rotation, but qualitatively the effect hypothesized by Halley seems to be verified.

Perhaps the most important contribution of the laboratory experiments to the theory of the atmosphere has been the separation of the essential considerations from the minor and the irrelevant. Condensation of water vapour, for example, may yet play an essential role in the tropics, where the circulation has not been well modelled, but in temperate latitudes it appears to be no more than a modifying influence, since systems occurring in the atmosphere, including even cyclones and fronts, occur also in the dishpan, where there is no analogue of the condensation process. Similar remarks apply to the topographic features of the Earth, which are intentionally omitted in most of the experiments. The so-called β-effect — the tendency for the relative vorticity to decrease in northward flow and increase in southward flow because of the variability of the Coriolis parameter — now appears to play a lesser role than had once been assumed. Certainly a numerical weather forecast would fail if the β-effect were disregarded, but the β-effect does not seem to be required for the development of typical atmospheric systems.

On the other side of the ledger, the experiments emphasize the necessity for quantitative considerations; at the very least these must be sufficient to place the atmosphere in the Rossby régime. The most that a completely qualitative treatment can do is to establish the separate properties of the Rossby and Hadley régimes, and then state that the atmosphere conforms to one or the other of these.

CHAPTER VII

NUMERICAL SIMULATION OF THE ATMOSPHERE

The recent development of laboratory models, which has provided a new tool for the investigation of the atmosphere, has been followed closely by the development of a further tool — the numerical model. Actually a numerical model is a system of mathematical equations, designed to resemble the equations governing the atmosphere, and arranged in a form suitable for solution by numerical methods. A numerical experiment consists simply of the determination and examination of a particular time-dependent solution of the equations, starting from some chosen initial conditions. Yet the procedure for performing a numerical experiment has become highly specialized, and it has become customary to regard a numerical model as a new physical system whose behaviour simulates that of the atmosphere, rather than simply an approximate mathematical means for studying the atmospheric system itself.

Numerical experiments are almost invariably performed with the aid of high-speed digital computing machines. The required amount of computation in most meaningful experiments is so great that slower methods are wholly inadequate.

Size and time limitations

Although it may be argued that the atmosphere is really a finite system containing about 10^{44} molecules, and that it may therefore be represented by a finite collection of numbers, the usual representation of the atmosphere as a continuum is far more realistic than any approximation which attempts to represent the atmosphere by a small finite set of numbers. The equations governing the continuous atmosphere may therefore be regarded as the exact equations. On the other hand a digital computer has a finite capacity, and operates at a finite speed. It therefore cannot describe the atmosphere as either a continuous or a continuously varying system.

The state of the atmosphere at any one time t must somehow be represented by a finite set of numbers, say m numbers $X_1, \cdot \cdot \cdot, X_m$, if the equations are to be solved numerically as an initial value problem. In most models these numbers have been the values of the meteorological variables at a previously chosen three-dimensional grid of points, with the values between the points being implied by some interpolative or extrapolative scheme. The interpolations are not actually carried out, but, as a part of the model, the exact equations are replaced by approximate expressions for the time derivatives $dX_1/dt, \cdot \cdot \cdot, dX_m/dt$ at the grid points, in terms of the values $X_1, \cdot \cdot \cdot, X_m$ at the grid points.

Other schemes afford more economical representations of the state of the atmosphere, but the corresponding modified equations are generally more awkward to handle. In the most common alternative scheme the field of each variable is expressed as a linear combination of a set of previously chosen functions, such as spherical harmonics, and the m numbers are the coefficient appearing in these combinations.

The m numbers cannot be varied continuously during an experiment but must be changed in a finite number of steps, say n steps. Generally a time increment Δt is chosen, and the values of the m numbers at time $t + \Delta t$ are approximated by a formula such as

$$X_j(t + \Delta t) = X_j(t - \Delta t) + 2 \Delta t \, dX_j/dt , \tag{136}$$

or often by a more sophisticated scheme. If an average of k arithmetic operations is required to compute one time derivative, the complete experiment requires a total of kmn arithmetic operations.

The mn numbers so generated may then be treated as observational data. They may be used to construct a series of simulated weather maps, or to compute any desired statistic such as a long-term average. For the latter purpose they have one obvious advantage over real data in that there should be no missing observations.

If an investigator wished to perform only one numerical experiment, he might be willing to let the machine compute for a year. Certainly comparable times have been spent in analysing the numerical data after they have been generated. Yet greater benefits are to be anticipated from a series of experiments, where certain factors may be varied from one experiment to another. The day when large computers will become so inexpensive that a meteorological centre can afford to own several of them and perform several experiments simultaneously does not seem to be near at hand. More likely there will be additional demands upon a single computer. A few weeks of computation therefore appears to be a practical upper limit for most numerical experiments.

The fastest computers in use today can perform a single arithmetic operation, such as the addition or multiplication of two several-digit numbers, in about 10^{-6} seconds, and can thus perform about 10^{12} operations in the course of two weeks, if they are kept running continuously. With further improvement in computer technology this figure is likely to increase. By comparison, a person working unaided would be unlikely to perform more than 10^4 operations during the same period.

The practical limit for the product kmn is therefore about 10^{12}. In a typical experiment about 100 operations are needed to compute a single time derivative of a single quantity, so that mn is limited to about 10^{10}. Assuming that an experiment must simulate at least a few months of atmospheric behaviour — an interval generally needed to obtain meaningful statistics from real observations — and assuming tentatively that the time increment Δt may be a few hours, a lower limit for n is about 10^3, whence an upper limit for m is about 10^7.

In the most elaborate numerical model so far investigated, Manabe et al. (1965) have represented the state of the atmosphere by about 50 000 numbers. If one can imagine using the upper limit of 10^7 numbers, these might reasonably be chosen as the values of five atmospheric variables at each of 20 elevations at each point in a horizontal grid of 10^5 points. There would then be one grid point for each 5000 square kilometres of the Earth's surface.

Undoubtedly there are particular solutions of the exact equations in which the fields of the variables are so smooth that they would be adequately described by interpolation between the 10^5 grid points, but observations show that these are not the solutions chosen by the real atmosphere. The air is filled with such disturbances as thunderstorms, cumulus clouds, and smaller turbulent eddies. The detailed structures of these systems cannot be described by the values of the variables at a coarse grid. Any numerical solution performed today must therefore apply to an atmosphere which has been idealized to the extent of omitting all motions of thunderstorm scale or smaller.

It might appear that with an eventual increase in the speed of computing machines by a factor of 10^3, which is not out of the question, it would become possible to introduce one grid point for every five square kilometres, and include at least the larger cumulus clouds. There is a further reason, however, aside from the large number of dependent variables demanded, why these features must be omitted. In any realistic solution each variable at each grid point will undergo oscillations about some mean value.

An instantaneous time-derivative affords a good approximation to the average time-derivative during a small fraction of a period of oscillation, but it gives a very poor approximation for a full period, let alone several periods. Even under the more sophisticated computation schemes, small inconsequential irregularities which should rapidly die out will be described as intensifying, and ultimately dominating the circulation, if Δt is as great as one fourth of the period of oscillation of these irregularities. The time increment must therefore be made small enough to accommodate the oscillations of all the systems which are retained, and it becomes as essential to eliminate rapidly oscillating systems, to keep n small, as it is to eliminate small-scale systems, to keep m within bounds.

The oscillations at an individual point may result from the propagation of waves or wave-like motions through the air, or from the simple displacement of some irregularity by the air motion. Local fluctuations due to turbulence are largely of the latter type; the period of oscillation is at least comparable to the time required for the smoothed wind-field to carry a turbulent element through its own length. Inclusion of even the crudest representation of cumulus clouds would therefore limit Δt to about a minute, while smaller-scale turbulence could limit it to a fraction of a second.

With Δt reduced to a minute or less, n would be raised to 10^5 or more. If even the largest cumulus clouds are to be carried as part of the global circulation in a numerical model, the speed of computation must increase by a factor of at least 10^5 above its present maximum; smaller cumulus clouds would require an even greater increase.

Nevertheless, cumulus clouds and smaller-scale eddies are instrumental in the vertical transfer of angular momentum, water, and energy, and the numerical solution must somehow incorporate their effects if it is to serve its purpose. This is ordinarily accomplished by relating the effects of these systems to the large-scale motions on which they are superposed, through the use of coefficients of turbulent viscosity and conductivity. Since the intensity of the smaller-scale systems is in turn affected by the larger-scale fields, the coefficients are better approximated by functions of the numbers at the grid points than by constants. Determination of suitable approximations is a problem which is still far short of solution.

Since cyclones and other disturbances of similar size, which are instrumental in the horizontal transports of angular momentum, water, and energy, generally do not follow one upon another by less than a day, it might appear that without the smaller-scale systems the solution could proceed in six-hour time increments. In practice this is not the case. Very small random errors, such as those introduced by round-off, will be interpreted by the computational scheme as small-amplitude disturbances, to be either carried along with the motion of the air or propagated through the air as waves. Once introduced, these disturbances will remain as part of the numerical solution. If they fail to grow they will cause no difficulties, but if Δt is greater than about one fourth of the period with which these fictitious disturbances ought to oscillate, according to the governing equations, the errors will amplify and finally dominate the field.

In general the errors will be interpreted as superpositions of simpler disturbances (normal modes: see Chapter VIII) having lengths as short as four grid intervals. (Waves with lengths between two and four grid intervals will appear in a spectral analysis, but under the usual differencing processes they will not oscillate as rapidly as those which are just four grid intervals long.) It follows that the maximum allowable value of Δt is the time required for the air to move one grid interval, or for a wave to travel one grid interval. This restriction is the well-known Courant-Friederichs-Lewy criterion for computational stability. For a more rigorous treatment the reader is referred to the book of Thompson (1959) or a shorter summary article by Phillips (1960).

The limitation upon Δt is dictated first of all by those waves which move much more rapidly than the speed of the wind. The most rapidly propagating waves are sound waves and external gravity waves. These waves are of questionable importance in the total circulation, but they soon become very important in an unstable computational scheme. It is possible to introduce further approximations which effectively modify the equations so that these waves are incapable of being propagated.

The most troublesome waves are vertically travelling sound waves, since the grid interval in the vertical is necessarily small compared to the horizontal interval. Even with minimum vertical resolution (two layers) they would limit Δt to about half a minute. As noted in Chapter II, they are completely eliminated by using the hydrostatic equation in place of the exact vertical equation of motion. This approximation is in general use in any case.

Gravity waves may be eliminated in a number of ways, the simplest of which is the use of the geostrophic approximation, in the form which equates the vorticity of the wind to the vorticity of the geostrophic wind, in place of the divergence equation. The more cumbersome but more realistic equation of balance would have a similar effect. Only the external gravity waves move at the most extreme speeds, and they may be eliminated, while internal gravity waves are retained, by simply requiring the vertically averaged divergence to vanish.

Whereas the use of an extremely short time-increment would suppress only the spurious sound and gravity waves and leave the ones which should actually occur, the hydrostatic and geostrophic approximations, with or without a very short time-increment, remove all sound and gravity waves. The approximations can therefore be justified only if these waves have very little effect upon the component of the circulation which is being studied. This indeed appears to be the case for sound waves; for gravity waves it has been claimed that this is the case, but the conclusion is less certain.

Even with the hydrostatic and geostrophic approximations, a six-hour time-increment is not possible in practice. Cyclones ordinarily travel more slowly than the wind, while fictitious disturbances other than sound and gravity waves, but not having the typical structure of cyclones, can be carried with the wind. Equally important, the minimum horizontal resolution needed to represent the presence and propagation of cyclones is considerably coarser than that needed for a good representation. Smagorinsky et al. (1965) found that a 250-kilometre grid interval gave noticeably more realistic results than a 500-kilometre interval. This was apparently not so much because somewhat smaller-scale features were admitted, but because the larger-scale features were more accurately depicted. With a reasonable horizontal resolution a one-hour time-increment appears to be near the upper limit.

Other frequently used idealizations afford minor savings in computation time, but their main purpose is simplicity rather than economy. One of these is the treatment of air as an ideal gas. Water in its various phases, with its consequent thermodynamic and radiational effects, is completely omitted. Another approximation treats the Earth's surface as completely homogeneous. Oceans and continents with their contrasting thermal capacities, and mountains and valleys with their contrasting mechanical influences, are eliminated. A still further approximation regards the solar heating as a function of latitude only. A hypothetical average sun is introduced, and the diurnal and annual variations about this average are disregarded. In many of the earlier models, the beta-plane approximation (see Chapter II) has been used.

Numerical weather prediction and the first experiment

The earlier stages of the development of numerical simulation depended almost completely upon the prior development of numerical weather prediction — the direct application of the governing equations to the problem of weather forecasting. The procedures for numerical simulation are almost the

same, with two principal differences. First, in numerical weather prediction the initial conditions must be chosen to represent the current weather situation, while in numerical simulation they may be chosen for maximum convenience. Second, in numerical weather prediction the solution is usually not extended beyond a few days, while in numerical simulation it must cover a few months at the least.

The latter distinction has considerable bearing upon the forms of the equations which may be used to advantage. There is no reason why a system of equations which gives no meaningful results at long range cannot give rather good short-range predictions. Consequently, for several years following the first reasonably successful numerical weather forecast, heating and friction were omitted altogether from the equations. To be suitable for simulating the global circulation, these equations would have to be modified at least to the extent of adding terms representing heating and friction.

The first serious attempt at numerical weather prediction was the remarkable work of Richardson (1922). Using a rather complete formulation of the primitive equations, Richardson performed a single six-hour forecast for the European area. His forecast completely failed to agree with the observed development, and he attributed the failure to inaccuracies in the initial wind measurements. It is true that the wind measurements were inadequate, but Richardson was also unaware of the phenomenon of computational instability.

Digital computers were unknown at that time, and Richardson spent many months preparing his single forecast. He visualized the establishment of a meteorological centre where 64 000 persons working together would be able to predict the weather as fast as it occurred. It is only recently, incidentally, that a single machine has attained the speed of 64 000 persons.

For a number of years afterward potential investigators were discouraged by Richardson's lack of success. It even appeared that sufficiently accurate initial wind-measurements might be impossible. A promising procedure in which accurate wind measurements were not required was eventually proposed by Charney (1947), and soon afterward Charney, Fjørtoft, and von Neumann (1950) produced the first moderately successful numerical weather forecast with a digital computer.

The forecast was based on a "one-level" model (see equation 58) in which the wind field appeared at only one level, and the temperature did not appear as a dependent variable at all. It would have been possible to modify the model by adding friction, but with no thermodynamic equation it would not have been impossible to add thermal forcing. With the introduction by Phillips (1951) of a "two-level" model, in which the wind field appeared at two levels, while a single temperature field was related to the difference of the wind fields through the thermal wind relation, the stage was set for numerical experiments of extended duration.

The original numerical simulation of the global circulation was the famous experiment of Phillips (1956). Phillips used a two-level model which contained the hydrostatic and geostrophic approximations and all of the other idealizations cited above. He chose a region bounded by parallel walls 10 000 kilometres apart, and within the region he constrained each variable to repeat itself every 6000 kilometres in the east-west direction. The external heating was simply a linear function of latitude, and friction was a linear function of the wind field extrapolated to the surface.

Phillips first suppressed all variations with longitude, and allowed a Hadley-type circulation to develop. The circulation lacked the contrasting trade winds and prevailing westerlies, and instead possessed weak easterlies everywhere at the bottom. These also would have disappeared if the solution had been allowed to attain full equilibrium, since, in conformity with the geostrophic approximation, the transport of momentum by the divergent component of the wind had been suppressed, and nothing was left to balance the surface frictional stress.

As initial conditions for the main experiment, Phillips chose the Hadley circulation plus small random perturbations which varied with latitude and longitude. In the course of about five days a system of organized waves developed, and then gradually increased in intensity. During the course of the integration the extreme wind speed also increased, and to maintain computational stability Phillips had to reduce the time-increment, which had at first been two hours, to one hour and then half an hour. After about thirty days the errors introduced by the computational procedure seemed to render the solution rather meaningless.

The experiment was remarkably successful in reproducing certain features of the circulation. At the surface easterlies soon appeared in low and high latitudes, with westerlies in between, and persisted throughout the experiment. Aloft the westerlies showed a distinct tendency to culminate in a jet stream. Moreover, the detailed energy cycle was qualitatively like the one observed in the atmosphere, and the various conversion processes possessed the right orders of magnitude, even if not the precise numerical values.

Unlike the real atmosphere there was no essential difference between the trade winds and the polar easterlies, but actually this similarity was demanded by the simplicity of the model. Certain symmetries had been built into the equations. Specifically, corresponding to any time-dependent solution, there exists another time-dependent solution in which the field of the eastward wind component is the mirror image, in the line midway between the extreme latitudes, of the same field in the former solution. Hence, if the equations are transitive, the trade winds and the polar easterlies must have equal average intensities. If they are intransitive, and one set of statistics possesses stronger trade winds, another set will possess stronger polar easterlies.

Yet the geometry alone does not demand easterlies and westerlies where they occur; the westerlies might have formed at high and low latitudes, with easterlies between. Thus, to the extent allowed by the constraints, the model duplicates the trade winds and prevailing westerlies, and suggests that their basic physical cause may not have been eliminated by the various approximations.

The experiment revealed a form of computational instability which had not been suggested by the shorter-range numerical forecasts. If the system of equations had been linear, a disturbance could have been carried along by a preassigned flow, but not by a flow which was itself a variable of the system. A suitable time-increment could then have been chosen once and for all. For a non-linear system the appropriate Δt decreases whenever the maximum wind increases. If for computational reasons the system continues to gain kinetic energy, even at a slow rate, a computation which has appeared to be stable for perhaps many days will suddenly become unstable as the critical wind speed is exceeded.

Before numerical experiments could be carried out over indefinitely long intervals, this non-linear instability had to be eliminated. Phillips (1959) has discovered one way in which the horizontal differencing procedure can produce a spurious gain in kinetic energy, and has found that the instability may be eliminated by periodically subtracting all disturbances having wavelengths of less than four grid intervals. A somewhat similar method consists of introducing a non-linear viscosity, which is very effective in damping the smaller-scale systems while having rather little effect on the larger ones. Another successful solution to the problem has been provided by Arakawa (1966), who has chosen a finite-difference formulation of the horizontal advective processes which strictly conserves the kinetic energy. Leith (1965) has solved the problem by a Lagrangian formulation of the advective processes, which seeks the location at time $t - \Delta t$ of a point which will be carried by the flow to a standard location at time t.

Recent numerical experiments

Some of the more recent simulations have attempted to remove the less realistic simplifications used by Phillips. Smagorinsky (1963) returned to the primitive equations, although still excluding "external" gravity waves. He used a spherical geometry, but restricted the flow to a region bounded by the Equator and 64.4° north (= arcsin 0.9). He extended the integration for 60 days in 20-minute time increments.

This model yielded a considerably more realistic representation of the trade winds and zonal westerlies. Irregular systems of five or six well-developed waves appeared in middle and higher latitudes, while the tropics contained many more disturbances of much smaller amplitude and horizontal scale.

A considerable advance was made by Leith (1965), who integrated a six-layer primitive-equation model containing evaporation, condensation, and precipitation. A novel feature of his numerical solution was a computer-produced motion picture, where the growth, modification, and decay of individual disturbances could readily be followed.

By far the most detailed numerical simulations to date are the experiment by Smagorinsky et al. (1965) with a dry atmosphere, and an accompanying experiment by Manabe et al. (1965) with a moist atmosphere. The former study permits external as well as internal gravity waves, and covers an entire hemisphere, but its principal refinement is the representation of the vertical structure by nine levels, which, with respect to pressure, are most closely packed at the bottom and top of the atmosphere. This allows a more realistic treatment of the surface friction layer, and also allows the effects of radiation, including ultra-violet absorption by ozone in the highest layers, to be incorporated in a more sophisticated manner. The latter experiment contains, in addition to all the features of the former, a simplified hydrological cycle, including evaporation, advection of water vapour, and precipitation, although the radiational heating is still based upon the normal rather than the current distribution of clouds and water vapour.

The experiments were remarkably successful in duplicating many features of the real circulation. The model develops its own tropopause, and the complete temperature distribution is not unlike that actually observed, including a poleward increase in the lower stratosphere. The energy cycle is in qualitative and reasonable quantitative agreement with the real atmosphere, and, as appears to be the case in reality, the kinetic energy of the stratosphere is maintained through mechanical interaction with the troposphere. The latter model yields a realistic over-all precipitation rate, although the maximum in the tropics is too intense. Altogether the energetics are satisfactory, although there seems to be too little eddy kinetic energy. The improvements to be expected from further refinements are largely quantitative, although the refinements themselves will necessarily have to be qualitative.

Another outstanding large numerical simulation is that of Mintz (1964). In conjunction with the models of Smagorinsky et al. and Manabe et al. it is especially valuable in that it includes many basic features which the other models omitted. Mintz used only two layers in the vertical, but he included the large-scale topography of the Earth, and also the oceans and land surfaces. The oceans were assumed to have infinite heat capacity, so that the ocean surface temperatures were prespecified quantities. The land, and also the sea ice, were assumed to have zero heat capacity, so that any heat received by them was immediately transferred to the atmosphere.

Mintz first performed an experiment without the mountains, and then at a certain point suddenly introduced them. According to Mintz (1964, p. 146), "As soon as this was done the air began to pour down the mountain sides (as would water down the sides of an island emerging from the sea), producing large gravity waves. After some days these large external gravity waves died out and only the familiar meteorological motions remained..."

Mintz found that the mountains had little effect upon the general type of behaviour of the atmosphere, and in particular upon the total kinetic energy, but that, together with the land-ocean contrast and the predetermined sea-surface temperatures, they had a considerable influence upon the geographical locations at which various features occurred. The time-averaged sea-level pressure field agreed rather well with observations, as did the upper-level temperatures averaged over longitude and time.

A number of other investigators have recently become engaged in numerical experiments. An excellent comparative account of the work thus far performed in this field has been given by Gavrilin (1965).

From a comparison of the large numerical experiments we gain the impression that we can duplicate the behaviour of the atmosphere as closely as we wish simply by making the equations more and more realistic. The principal remaining physical problems are the proper representation of small-scale motions and the proper treatment of water in the atmosphere; these problems are compounded in the problem of cumulus convection. There seems to be no obvious reason why acceptable solutions cannot eventually be found.

It is therefore in order to ask what would be gained, other than the satisfaction of completing a challenging task, if we should eventually reproduce the general circulation in all its relevant details. Since we know in any case, by definition, that the correct equations, solved in a correct manner, will correctly reproduce the general circulation if the atmosphere is transitive, or will reproduce the correct circulation from a wide range of initial conditions even if the atmosphere is intransitive, the immediate result would be to reassure us as to the correctness of the equations and the method of solution. Our understanding of the general circulation would be increased only because many properties of the real circulation are not easily observed and measured, whereas an essentially complete numerical solution would contain the numerical value of every relevant variable, from which any desired statistics could be evaluated. Such questions as the details of the energy cycle could be settled once and for all.

The solution would not, however, necessarily increase our physical insight. The total behaviour of the circulation is so complex that the relative importance of various physical features, such as the Earth's topography and the presence of water, is no more more evident from an examination of numerical solutions than from direct observations of the real atmosphere.

As in the case of the laboratory models, the greatest value of the numerical models should lie in the opportunity for controlled experiments. The control may be of the type which is readily introduced in the laboratory, namely, the variation of one or more parameters, such as the intensity of the heating. The added power of the method lies in the possibility of altering not only physical features, such as the Earth's topography, but also entire physical processes, such as the propagation of sound and gravity waves, since the equations are readily modified so as to describe a system which is completely unrealizable in nature. In the laboratory, sound and gravity waves may be subdued by various means, but they cannot be completely eliminated. In a numerical model the hydrostatic and geostrophic approximations eliminate them altogether. The significance of these waves for the remainder of the circulation may then be readily assessed.

The performance of controlled comparative numerical experiments appears to have only begun. Most of the effort to date has been devoted to perfecting individual experiments. Perhaps we may anticipate the time in years to come when we may as a matter of course perform several experiments of the scale of the one performed by Manabe *et al.* whenever we wish to test a hypothesis. In the meantime the effort devoted to reproducing the atmosphere rather than changing it is also well spent. The more closely we are able to duplicate the atmosphere when such is our purpose, the more confidence we can later place in controlled numerical experiments in which systematic departures from the real atmosphere have been introduced.

CHAPTER VIII

THEORETICAL INVESTIGATIONS

The physical laws which govern the behaviour of the atmosphere, and the mathematical equations which represent these laws, form the basis for every theoretical study of the circulation. The mathematical techniques for handling these equations are numerous. With the recent outburst of interest in numerical simulation, and the growing anticipation that this method may eventually supply an answer to almost any question which may be asked of it, it is easy to lose sight of the fact that theoretical studies were an established part of meteorology long before the advent of the first digital computer.

Because numerical simulation seems to show promise of achieving a position of unique and long-lasting importance, and because, in contrast to the analytic expressions yielded by more classical mathematical techniques, the results of a numerical experiment consist of a collection of numbers to be processed further in the manner of observational or experimental data, we chose to discuss numerical simulation separately in the previous chapter, following the chapters on observational and experimental studies. In the present chapter we shall examine some of the more conventional procedures through which the governing equations may be applied to the study of the circulation, with particular emphasis upon those procedures for which a computing machine is not essential.

Analytic solutions of the dynamic equations

Perhaps the ultimate theoretical achievement would be the discovery of the general analytic solution of the exact equations. At present such an achievement is precluded by our inability to formulate such processes as friction and condensation in an adequate manner, but the discovery of the general solution of an idealized system of equations would be an almost equally satisfying accomplishment. Unless the equations have been so highly simplified that non-periodic solutions no longer exist, this accomplishment also appears to be impossible; the non-periodic functions characteristic of the atmosphere cannot be explicitly expressed in a finite number of symbols.

It is perhaps a matter of opinion as to how much simplification of the equations is permissible, but we feel that the following minimum requirements must be satisfied by the general solution of any system of equations, if this solution is to be regarded as depicting the general circulation and the processes which maintain it. First, there must be an energy cycle, in which heating produces available potential energy, reversible adiabatic processes convert available potential energy into kinetic energy, and kinetic energy is dissipated by friction. Second, there must be eddies, or departures from zonal symmetry. These eddies must for the most part transport sensible heat toward higher latitudes, and thereby act to reduce the poleward temperature gradient; they must also gain available potential energy from the zonally averaged field of mass. Likewise the eddies must for the most part transport angular momentum toward higher latitudes, and thereby contribute to the maintenance of low-level trade winds and prevailing westerlies; they must also give up some kinetic energy to the zonal circulation. To complete the angular-momentum balance, direct meridional circulations must appear in low latitudes and indirect circulations must appear in middle latitudes; these must also transport additional sensible heat and potential energy. It need

hardly be mentioned that the transports of sensible heat and angular momentum are non-linear processes, and that the equations must be non-linear.

Possibly there is a system of equations whose general solutions is periodic and yet satisfies the above requirements. If so, it might be possible to obtain the general solution in analytic form. An intensive study of the solution might then disclose the basic reasons why the processes which it depicts also prevail in the real atmosphere.

In the absence of any assurance that the general solution of any suitable system of equations can be found, a less ambitious and more realistic goal is the determination of special analytic solutions. If the atmosphere is idealized at least to the extent of omitting all geographical features and all variations of external heating with time and longitude, one of these solutions is likewise independent of time and longitude. This solution describes the Hadley flow. It is not a good representation of the flow observed in the real atmosphere, but it is of interest for its own sake, and it plays an important role in current ideas regarding the general circulation.

Oberbeck (1888) was the first to attempt to obtain the Hadley solution in analytic form. In so doing he specified the temperature field rather than the field of external heating. Qualitatively his solution resembles the flow envisioned by Hadley (1735), with a single direct cell, but, as we noted in Chapter IV, the resemblance seems to be accidental.

Many years later Kropatscheck (1935) attacked the same problem, and obtained a meridional circulation looking very much like the flow visualized by Thomson (1857) and Ferrel (1859), with a shallow indirect cell in middle and higher latitudes and a single Pole-to-Equator direct cell filling the rest of the troposphere. Like Oberbeck he specified the temperature field, but his equations also included the thermal wind relation; thus his westerly winds were of the proper intensity, and his direct cell was of the proper strength for maintaining them. He adjusted the surface zonal winds so that the surface frictional torque would balance the vertically integrated transport of angular momentum.

For the friction layer, however, he assumed in advance that wherever there were surface easterlies, or westerlies, there would be a substantial equatorward, or poleward, drift superposed upon the meridional circulation which would otherwise prevail. Thus he was forced to obtain the shallow indirect cell, and his work cannot be taken as a demonstration that Thomson's or Ferrel's picture of the meridional circulation is preferable to Hadley's.

The reader will notice that we have not yet stated whether the meridional circulation which would actually prevail if large-scale eddies could be prevented from developing, would be the circulation envisioned by Hadley or the one pictured by Thomson and Ferrel, or perhaps some other circulation. This omission is intentional; we do not know the answer.

Since the real Earth possesses geographical irregularities which would render a circulation without eddies impossible in any case, the question can only pertain to the atmosphere of an idealized Earth. At the very least, to make the answer determinate we must specify whether the Earth's surface consists entirely of land or entirely of ocean. Certainly the field of heating will be profoundly affected by this choice.

Let us note, then, that in a certain sense any meridional circulation is possible. We may choose any temperature field, and an accompanying zonal wind field which approximately satisfies the thermal wind relation. We may superpose any meridional circulation upon this. We may then solve for the fields of friction and heating.

In general the friction and heating so determined will form rather unrealistic patterns. If we demand that the friction must bear a known relation to the motion, we can modify our procedure by choosing any meridional circulation and solving a linear equation, with variable coefficients, for the zonal circulation and the accompanying temperature. Alternatively we can choose a zonal circulation, which must satisfy the constraint that the total frictional torque is zero upon any region bounded by the Earth's surface and two surfaces of constant angular momentum, and solve for the meridional circulation. In either event we can then determine the heating from the thermodynamic equation.

The remaining problem will be that of finding an initial choice of the meridional or zonal circulation which leads to a reasonable field of heating. It seems altogether possible that some circulation like Hadley's and also some circulation like Thomson's or Ferrel's will serve this purpose.

In any event, a zonally symmetric solution does not describe the observed circulation satisfactorily. Among the special solutions of the idealized equations which may offer better representations, the simplest are those which describe a flow containing waves which progress without changing their shape, as in the steady Rossby régime in the laboratory experiments. These are steady-state solutions in a moving co-ordinate system, and it may well be possible to obtain them by analytic procedures. Next in simplicity are the vacillating solutions, where the waves alter their shape and speed of progression in a regular periodic manner. These doubly periodic solutions become simply periodic in a suitably moving co-ordinate system, and it does not seem unrealistic to hope that they also may be determined analytically.

Like the Hadley solution, the steady-wave solutions and the vacillating solutions, if they exist at all, are unstable with respect to still further disturbances, and are not the solution chosen by the atmosphere. If they are to be offered as a representation of the atmosphere, they should satisfy the same minimum requirements which we have demanded of the general solution. We cannot be certain that they will do so. The real atmosphere contains many irregularly shaped eddies. Some of these transport angular momentum and sensible heat poleward across middle latitudes, while others transport these quantities equatorward. The fact that the net effect of the eddies is a poleward transport is no assurance that the eddies in the steady-wave or even the vacillating solutions will transport angular momentum and sensible heat in this direction.

Nevertheless, it seems plausible that the physical processes which control the configurations of the eddies in the irregularly fluctuating atmosphere, and thereby determine the transports which they accomplish, may also act in a similar manner upon the eddies appearing in the unstable but mathematically possible steady-wave solutions. It is even more reasonable to assume that the average transports of angular momentum and sensible heat by the eddies in the vacillating solutions will be similar in direction and even in amount to the transports accomplished by the irregular eddies in the real atmosphere. If this can be shown to be the case, the discovery of the most general vacillating solution would approach the desired goal.

In view of the difficulties involved in determining even the Hadley solution analytically, the task of obtaining steady-wave solutions would appear to be rather formidable. Some of the successful attempts to solve equations which have been simplified beyond the point of describing the maintenance of the general circulation reveal some of the problems involved.

The most celebrated analytic solution of a dynamic equation in meteorological literature is undoubtedly Rossby's (1939) solution of the two-dimensional vorticity equation on the beta-plane. This solution exhibits the propagation of steady waves in the westerly-wind belt, and bears considerable resemblance to the subsequently discovered flow in some of the laboratory experiments. Nevertheless it is not intended to offer an explanation of the general circulation, since there is no heating nor friction, and the amplitude of the waves must be prescribed in advance.

Haurwitz (1940) extended Rossby's solution to include friction and forcing, but since the solution was two-dimensional, the forcing had to be mechanical rather than thermal. Haurwitz also obtained solutions for the complete sphere. Both Rossby and Haurwitz had used a linearized form of the vorticity equation, but Craig (1945) noted that their solutions also satisfied the non-linear equation. More recently Kuo (1959) has obtained three-dimensional analytic solutions for a simplified non-linear system of equations, again without heating and friction.

A feature of these non-linear solutions is that the eddies consist of a single mode of motion, i.e. they are of such a configuration that the advection which they accomplish produces no distortion. In the more general field of motion, superposed eddies of more than one mode will distort one another and thereby produce still further modes. Solutions possessing more than one mode of motion therefore possess an infinite number.

If the eddies do not distort the field of motion, they certainly do not alter the field of zonally averaged angular momentum. It follows that the solutions of the frictionless equations containing eddies of a single mode possess no convergence of eddy angular-momentum transport.

We know of no analytic solutions of even the most simplified equations which yield the proper eddy transports of sensible heat and angular momentum. In attempting to represent some of the dishpan experiments, Lorenz (1962) found the general non-transient solution of a highly simplified system and obtained a proper transport of sensible heat, but the system admitted no transport of angular momentum at all, either by the eddies or the meridional circulation. When more degrees of freedom were added to the system and an eddy transport of angular momentum was allowed, Lorenz (1963) could obtain the solution only by numerical integration.

It would therefore greatly facilitate the theoretical study of the circulation if the statistical properties of the large-scale eddies could in some manner be represented in terms of the zonally averaged motion upon which they are superposed. The procedure which most naturally suggests itself consists of assuming that the horizontal eddy transports of angular momentum and sensible heat are proportional to the gradients of zonally averaged angular velocity and temperature, the factors of proportionality being suitably chosen Austausch coefficients. Such a procedure would reduce the system of equations to one not much more complicated than the system which must be solved if the Hadley solution is to be obtained.

The observational studies have revealed, however, that throughout about half of the atmosphere the eddies transport angular momentum toward latitudes of higher angular velocity, while in some regions they transport sensible heat toward latitudes of higher temperature. Thus a procedure based upon the introduction of large-scale Austausch coefficients would yield the wrong answer. A clear statement of the theoretical reasons why the eddy-transport of angular momentum cannot be expected to conform to classical turbulence theory has been given by Eady (1954).

We feel that an analytic solution of the dynamic equations, if it is to be offered as an explanation of the circulation, cannot by-pass the problem of explaining the configurations of the eddies. Meanwhile, most theoretical studies actually carried out, except those which have sought the Hadley circulation, have had the more modest aim of demonstrating that some observed feature of the circulation must occur after some other observed feature has been assumed to occur. With this more limited goal, it is frequently possible to simplify the equations by methods which are not allowable when a complete solution is sought.

The perturbation equations

The technique of handling otherwise intractable non-linear equations which is found most frequently in meteorological literature is that of linearization, i.e. converting the non-linear equations into linear equations by treating certain variable factors as constants. The most systematic use of linearization

occurs in the familiar "perturbation method". If two time-dependent solutions of the same system of non-linear equations differ only slightly from one another at some initial time, the departure of one solution from the other is governed to a close approximation by a system of homogeneous linear equations, during such time as this departure remains small. This principle finds its greatest use when one of the two solutions is already known, in which case the other solution may be found by solving the linear system. Usually the known solution is in some way simpler than the solution being sought, and most frequently it is because of its greater simplicity that it has become known. Consequently, the flow represented by the simpler solution is often called the "basic flow". In the majority of studies in meteorological literature the basic flow is independent of time, and in most of these cases it is also independent of longitude. The basic flow could, for example, be a complete Hadley circulation. There is no real necessity, however, for the known solution to be independent of longitude or time, or to be any simpler than the unknown solution.

In any event, the coefficients in the linear equations depend not only upon the original non-linear equations but also upon the particular known solution. In some instances where the basic flow is sufficiently simple, the coefficients are constants and the equations are readily solved. In other important cases the coefficients vary with latitude and elevation. If the basic flow is time-variable, the coefficients also vary with time. The perturbation method has received its most extensive meteorological development by Bjerknes *et al.* (1933).

The most important property of homogeneous linear equations is superposability; the sum of any two solutions is also a solution. It is this fact which has made it possible for the mathematical theory of these equations to become so highly developed. In many instances the general solution may be expressed as a superposition of simpler solutions or "normal modes". When the basic flow is independent of time and longitude each normal mode represents a pattern which progresses without changing its shape, while its amplitude may grow exponentially, remain constant, or decay exponentially.

The perturbation method is most easily justified when it is used to investigate the stability of a basic flow, i.e. to determine whether small disturbances superposed on the basic flow will amplify or decay. If for arbitrary initial conditions the solution consists of a superposition of normal modes, the basic flow is unstable if at least one normal mode amplifies. If all of the normal modes decay, it is stable. It is often considered neutral rather than stable if at least one mode retains its amplitude, while none amplify. An unstable flow may be stable with respect to certain classes of disturbance, and unstable with respect to others. A general solution of the linear equations reveals not only the stability or instability of the known solution but also the forms of the amplifying and decaying modes. When the general solution is not readily found, the stability may often be determined by examining the behaviour of the total energy of the eddies.

A disturbance or "eddy" superposed upon a steady zonally symmetric flow represents a supply of energy over and above that contained in the steady flow. The linearized equations do not conform to the law of conservation of energy since they frequently allow a disturbance to amplify without limit at an exponential rate. Nevertheless, the non-linear equations from which the linear equations are derived, and which they closely resemble during the period when the disturbances are small, require a source of energy for the growing disturbances; this source must be the energy of the basic flow, even though the equations do not explicitly alter the basic flow. When it is impossible to extract energy from the basic flow and at the same time satisfy the remaining constraints, the basic flow must be stable.

A study of stability as such is not concerned with the manner in which the basic flow is established or maintained, and in many studies the original non-linear equations may be simplified by omitting

friction and thermal forcing altogether. When this is done, virtually any field of motion which is independent of time and longitude satisfies the non-linear equations, whether or not it bears any resemblance to a circulation which could be maintained against friction by heating. There is thus a wealth of basic flows whose stability may be investigated, and many studies seek general criteria for instability, rather than testing the stability of special basic flows. On the other hand, removing friction removes a sink of energy from the eddies — sometimes the only sink. As a result, a large class of basic flows may prove to be neutral. When friction is included the eddies must also have a source of energy, and the neutral flows are generally restricted to those critical ones for which the source and sink of energy just balance.

There are a number of mechanisms through which a basic flow may transfer its energy to a disturbance, and the unstable conditions resulting from different mechanisms are generally regarded as different types of instability. The two most important types from the point of view of the general circulation are barotropic instability and baroclinic instability.

The former type occurs when the eddies receive their energy from the kinetic energy of the basic flow. It has been treated in detail by Kuo (1949) and subsequent authors. No turning of the wind direction with height is involved, and the phenomenon is most easily studied by disregarding the vertical dimension altogether, and letting the original non-linear equations describe the flow of a two-dimensional incompressible fluid. A further common simplification is the beta-plane approximation. In giving up kinetic energy the basic flow must still conserve its total angular momentum. Since, for fixed angular momentum, a flow with uniform angular velocity (or uniform linear velocity, in the case of a beta-plane) contains the minimum kinetic energy, a flow cannot be barotropically unstable unless its angular velocity varies from one latitude to another. In this event the coefficients in the linear equations vary with latitude, and the explicit determination of normal modes is often rather difficult. Energy considerations, on the other hand, show that a necessary condition for barotropic instability is a maximum or minimum of absolute vorticity somewhere other than at the Poles (or the extreme latitudes, in the case of a beta-plane). Since, when instability does occur, the growing disturbances feed upon the kinetic energy of the basic flow, they must have a structure suitable for transporting angular momentum, in the mean, toward latitudes of lower angular velocity. They need not transport any sensible heat.

Baroclinic instability occurs when the eddies receive their energy from the available potential energy of the basic flow. There is no need for a direct exchange of kinetic energy with the basic flow; hence the basic flow need not have any horizontal shear, and the phenomenon is most easily investigated by suppressing all variations of the basic velocity with latitude. Baroclinic instability was first studied by Charney (1947), Eady (1949), and Fjørtoft (1950), and in more detail by Kuo (1952). In contrast to the barotropic stability problem, the coefficients in the linear equations become constants when the vertical wind shear is sufficiently simple, in which case the normal modes are easily determined. The various investigators have used different simplifying assumptions, and their results are not in complete agreement. Nevertheless all agree that instability is favoured by a large Coriolis parameter, large vertical wind shear, and low static stability. For vertical wind shears typical of the middle-latitude westerlies, when critical conditions are slightly exceeded, the most rapidly amplifying normal mode generally consists of a chain of six, seven, or eight waves extending around the globe. Since the growing disturbances feed upon the available potential energy of the basic flow, they must transport sensible heat, in the mean, toward latitudes of lower temperature. They need not transport any angular momentum.

The most general basic flow which is independent of time and longitude possesses both horizontal and vertical shear. Such a flow may be unstable barotropically, or baroclinically, or both barotropically and baroclinically, but separate consideration of the conditions for barotropic and baroclinic instability is not sufficient to determine whether the flow is stable or unstable. The linear equations are much more

complicated than in the special cases where either horizontal or vertical shear is absent, and in general the normal modes are not readily determined analytically, although they can be found by numerical methods. The problem has recently been treated in great detail by Pedlosky (1964), using the geostrophic approximation. When the equations are further simplified by the two-level approximation, a necessary condition for instability is the existence of both positive and negative gradients of "potential vorticity", which in this case is a linear combination of vorticity and temperature. Pedlosky has considered certain flows which satisfy this condition but do not satisfy the condition for barotropic instability, and has found growing disturbances which gain their energy from the available potential energy of the basic flow by transporting sensible heat toward latitudes of lower temperature, but give up kinetic energy to the basic flow by transporting angular momentum toward latitudes of higher angular velocity. The flows therefore behave in the manner which would be expected if they possessed independently the properties of baroclinic instability and barotropic stability, and they possess an energy cycle which is qualitatively like the one observed in the atmosphere.

The linearized equations have been used for a long time to study the development of cyclones, but their application to the global circulation necessarily came only after the realization that the eddies played a significant role in the circulation. Considerable caution is required in using the equations. In the first place, the flow in the atmosphere almost never consists of a zonally symmetric flow plus small disturbances. The existing flow may be averaged with respect to longitude, and this averaged flow may be regarded as the basic flow, but the departures from this average are seldom if ever small. Use of the linearized equations when the eddies are large omits the non-linear effects of the field of eddies upon itself; these effects would be mainly noticeable as distortions. This omission is equivalent to the hypothesis that large disturbances superposed upon a basic flow behave in the same manner as small disturbances superposed upon the same flow. One might argue, however, that such an assumption is just another simplifying approximation which, like the geostrophic approximation, is not satisfied perfectly. In any event, the assumption need not violate energy principles.

A more serious shortcoming of the linearized equations is the omission of the non-linear effects of the eddies upon the basic flow. These effects would alter the energy of the basic flow, and hence the overall intensity. Because of this omission, the linearized equations say nothing about the variations of the basic flow and, as a consequence, they are incapable of explaining the ultimate intensity of the eddies, since growing or decaying normal modes will continue to grow or decay as long as the basic flow does not vary.

It is possible for a particular solution of the linearized equations to resemble the general circulation of the atmosphere, if a neutral basic flow is chosen. The energetics of the eddies may even be qualitatively correct. But the basic flow persists only because all sources and sinks have been removed.

It would be possible at this point to acknowledge the non-linear effect of the eddies upon the basic flow, and assume that heating is just sufficient to cancel this effect. But the complete system of equations would then no longer be linear, even though the equations governing the eddies would be linear. No system of linear equations can by itself yield a complete explanation of the general circulation; at most it can explain certain features after others have been assumed. For the complete problem, linearization is not "just another approximation".

A further effect not admitted by the linearized equations with a constant basic flow is the instability of a system consisting of this basic flow plus superposed eddies with respect to still further disturbances. Here the instability of a time-variable flow is involved. Such instability appears to be responsible for the occurrence of vacillation in some of the laboratory experiments and non-periodic flow in the atmosphere, in place of the uniformly travelling waves which would appear if a single normal mode were dominant.

There are at least two ways in which the theory of stability can add to our understanding of the global circulation. As early as 1937, V. Bjerknes postulated that the Hadley flow which would prevail if no disturbances were present would be unstable. Eady (1950) re-emphasized this point, and characterized the instability as baroclinic. Simplified analytic solutions for the Hadley circulation such as Charney's (1959), and numerical solutions such as Phillips' (1956), support this assumption. For the real atmosphere the exact Hadley circulation, or the nearest circulation to it which could exist in view of the geographical irregularities, has not been determined, and its stability cannot readily be investigated.

The presence of disturbances, at least in a sufficiently idealized atmosphere, is thus explained; any circulation devoid of them would soon become unstable. This does not mean that the disturbances found in the atmosphere originated as small perturbations on a nearly symmetric circulation, or for that matter that a nearly symmetric circulation ever existed. It does mean that if the disturbances should ever for any reason temporarily disappear or nearly disappear, the remaining symmetric flow would evolve toward the Hadley circulation, which is unstable, whereupon the disturbances would regenerate.

The other approach considers the stability of the existing zonally averaged flow, rather than the flow which would prevail if no disturbances were present. This flow appears to be nearly always baroclinically unstable but generally barotropically stable. Moreover, the most rapidly growing normal modes have dimensions and structure somewhat like the observed eddies in the atmosphere, and they possess a similar energy cycle. Under the hypothesis that the normal modes which are indicated by the linear equations as amplifying most rapidly are the ones which ultimately acquire and retain a large amplitude, the typical size and shape of the eddies, and the observed energy cycle, are qualitatively accounted for in terms of the observed basic flow.

Real disturbances do not amplify forever, and it might appear that the typical basic flow should be neutral rather than unstable, in order that the disturbances should simply maintain their intensity. However, there are limitations to the assumption that large disturbances behave like small ones. Individual cyclones tend to have a life cycle. By the time that they reach occlusion their shape has changed considerably. The change of shape and the cessation of growth are not predicted by the linearized equations. They may result in part from a change in the basic flow, in which case they could be described by linear equations with prespecified time-variable coefficients. It is likely however that the occlusion of a cyclone is partly due to the non-linear distortive effects of the field of disturbances upon itself, in which case it can occur when the basic flow is still unstable rather than neutral.

The equations for the basic flow

What is mainly lacking in theoretical work based upon linearized equations is an explanation of the observed basic flow and its variations. Growing disturbances, in removing energy from a basic flow, ordinarily render it less unstable; ultimately they render it neutral and the disturbances cease growing, or, as just noted, they may cease growing while the basic flow is still unstable. Thus the disturbances act as a governor, maintaining the basic flow at nearly neutral stability. There is, however, a wide variety of neutral basic flows. The appropriate flow is not simply the Hadley flow, reduced by a constant factor. Stability considerations therefore place a constraint upon the basic flow, but do not determine it.

It is nevertheless possible in principle to determine the basic flow if the properties of the disturbances are known, not by means of the linearized equations, but by the zonally-averaged equations which govern the basic flow. These equations are identical with the ones which govern the Hadley flow, except that the convergences of the eddy-transports of angular momentum and sensible heat appear as additional

mechanical and thermal forcing. If these transports are prespecified, the procedure for solving the equations is similar to the procedure for solving for the Hadley circulation, and it should be approximately as difficult.

A solution of these equations would not by itself explain the basic flow, since the prespecified transports actually depend upon the eddies, which in turn are influenced by the basic flow. Consequently the determination of a solution would be a somewhat unrewarding accomplishment. Since it would also be a difficult task, it is not surprising that it has not been carried out. Nevertheless, in combination with a suitable solution of the linearized equations, a solution of the equations governing the basic flow might offer the best attainable approximation to an analytic solution of the complete system.

We have noted that under the hypothesis that the normal modes which grow most rapidly when small in amplitude are the ones which will remain after reaching finite amplitude, the linearized equations may be solved for eddies of an unknown amplitude but a known shape, in terms of a prespecified basic flow. Likewise, we have seen that the zonally averaged equations may in principle be solved for the basic flow, provided that the transports of angular momentum and sensible heat by the eddies are prespecified. The combined system of equations should therefore be solvable for the zonal flow and the eddies simultaneously, provided simply that the amplitude of the eddies is prespecified. The appropriate amplitude may be determined by the condition that the basic flow should be neutral, so that the eddies will undergo no net gain or loss of energy.

In reality the new system of equations does not differ too greatly from the original system of governing equations upon which it is based, and it may be regarded as "another approximation". It has been simplified to the extent of omitting the non-linear effects of the field of eddies upon itself, but it is still a closed non-linear system. The linearized equations governing the eddies still appear among the equations, but they are now no longer linear, since the coefficients, which depend upon the basic flow, are now unknowns of the system. From the point of view of duplicating the circulation, there is nothing to recommend a solution of this system over a numerical solution of the original system from which it was derived. However, the process of solving the system may yield some insight into the general circulation which is not afforded by a typical numerical experiment. The maintenance of the eddies by a basic flow which is baroclinically unstable or neutral but barotropically stable, and the control of the basic flow by the transports of angular momentum and energy accomplished by the eddies, should be clearly revealed. An added feature is that the restriction to a single normal mode will automatically yield a steady-wave solution.

An approximate method of solving a system of equations of this sort was used by Charney (1959) to construct a model of the general circulation. In this study Charney began with a simplified system of equations essentially the same as the one used by Phillips (1956) in his original numerical experiment. This system includes the beta-plane, two-layer, and geostrophic approximations. He first solved analytically for the Hadley circulation — a task made possible by the simplicity of the equations. He found this circulation to be unstable, and he determined the most rapidly amplifying normal mode. He then postulated that the growing disturbance would retain its shape while it modified the zonal flow, and he solved for a new zonal flow in terms of an unknown amplitude of the eddies, including as additional thermal and mechanical forcing the effects of the eddy-transports of angular momentum and energy. Finally he determined the amplitude of the disturbance by requiring that the available potential energy given to the disturbances by the zonal flow and by external heating should balance the kinetic energy removed from the disturbances by the zonal flow and by friction. A balancing energy cycle was thus assured.

The picture of the general circulation obtained by Charney is realistic in certain features. Easterly surface winds appear in low and high latitudes, with westerlies in between. The energy cycle proceeds

in the right direction. More significant from the point of view of method is the close resemblance to the results of Phillips, since Charney was attempting to solve by an approximate method nearly the same equations which Phillips had solved numerically.

It would seem possible to obtain an exact solution of the new system by a successive-approximation scheme in which the first few steps duplicate the procedure used by Charney. Following these steps, a second approximation to the field of eddies, again in terms of an unknown amplitude, is obtained by finding the most rapidly growing mode corresponding to the new zonal flow. The transports of angular momentum and energy by the new eddies are used to solve for the next approximation to the basic flow, and again the amplitude of the eddies is determined by requiring the energy cycle to balance. This scheme is then repeated until convergence is obtained. There is of course no assurance that such a scheme will converge at all, but in view of the general resemblance of Charney's approximation to Phillips's solution, which is indicative of the final approximation, it seems possible that the method will converge rather rapidly.

It remains to be seen whether some further extension of this method, capable of representing vacillation or perhaps some more irregular behaviour, can yield a more realistic representation of the general circulation while continuing to offer as much insight into the manner in which the circulation operates.

CHAPTER IX

THE REMAINING PROBLEMS

The large numerical experiments, in which the state of the atmosphere is sometimes represented by many thousands of numbers, constitute our closest approach to a theoretical demonstration that the circulation must assume the form which it does rather than some other conceivable form. The demonstration is by no means complete. Certain quantities which really depend upon the circulation, such as the spatial distribution of absorption of radiation by water vapour, have usually had their observed values preassigned to them. Certain features, such as the presence of tropical hurricanes, have not yet been reproduced. Moreover, since even the most realistic numerical solutions are particular solutions, the possibility of other particular solutions with rather different properties cannot be completely eliminated. Nevertheless, shortcomings of these sorts have characterized virtually all theoretical studies of the general circulation, usually to a much greater extent. One gets the impression that the experiments will ultimately duplicate the atmospheric circulation to any desired degree of accuracy.

As demonstrations that the atmosphere must behave as it does, theoretical studies employing more classical mathematical procedures have not as yet compared with the numerical experiments. One of the closer approximations to such a demonstration is the previously cited study by Charney (1959). Yet Charney found it necessary to introduce the *ad hoc* assumption that the fully developed disturbances, superposed upon the prevailing zonal flow, would possess the same shape as the most rapidly growing incipient disturbances superposed upon the Hadley flow. Altogether he succeeded in duplicating no more than the grossest features of the general circulation. Nevertheless, by following through his complete procedure one can gain a certain insight concerning the reasons why the atmosphere behaves as it does, which one might not gain from an inspection of the millions of tabulated numbers forming the complete output of a numerical experiment.

A deeper physical insight may sometimes be afforded by a simple qualitative description. In this concluding chapter we shall outline as nearly complete a qualitative explanation for some of the main features of the circulation as we feel can at present be formulated. We shall not attempt to make our presentation rigorous, and our arguments will not always be demonstrations that the atmosphere must behave as it does, rather than in some other manner. We shall however attempt to present the correct reasons for the observed behaviour, to the extent that these are known. In addition we shall indicate the areas where suitable qualitative explanations have yet to be offered.

An explanation of the circulation of the atmosphere logically begins with the driving force. There seems to be no question but that this is solar radiation, and that it is the greater intensity of this radiation in lower than in higher latitudes which enables it to produce available potential energy, which it may do either by heating the atmosphere directly, or by heating the underlying surface which in turn transmits energy to the atmosphere.

It follows that the atmosphere must possess a circulation. For in the absence of motion, each latitude would assume a state of thermal equilibrium, losing as much heat as it gained, and in order to lose more heat than the higher latitudes, the lower latitudes would have to be warmer. But a cross-

latitude temperature contrast would be incompatible with a state of no motion, for if hydrostatic equilibrium prevailed, there would be cross-latitude pressure gradients, and horizontal motions would develop, while if hydrostatic equilibrium did not prevail, vertical motions would develop.

The required circulation must transport energy from low to high latitudes, and thereby bring about a weaker poleward temperature gradient than would otherwise prevail, but it cannot destroy the gradient altogether. This follows because friction is continually dissipating the kinetic energy of the circulation, and new kinetic energy must be produced at the expense of available potential energy, whence new available potential energy must be generated by heating of the warmer regions and cooling of the colder ones. If every latitude remained in thermal equilibrium in spite of the circulation, there would be no local heating or cooling, while if the circulation destroyed the temperature contrast altogether, there would be no warmer and colder regions to be heated and cooled. If the circulation transported energy equatorward and thereby maintained a stronger temperature contrast than would otherwise prevail, the tropical regions would be cooled and the polar regions would be heated by radiation, and available potential energy would be destroyed. Likewise, if the circulation transported enough energy poleward to reverse the temperature gradient, the now warmer polar regions would be cooled and the now cooler tropical regions would be heated by radiation, and available potential energy would again be destroyed.

The arguments which we have presented are far from rigorous. First of all, available potential energy may be generated without any horizontal heating contrast if the atmosphere is statically unstable. Without going into detail, we shall simply note that radiation evidently tends to produce a stable stratification throughout much of the atmosphere; where this is not the case, small-scale convective motions tend to develop and stabilize the stratification. Even assuming a statically stable atmosphere, however, we have not considered the vertical structure of the atmosphere in sufficient detail. In the lower stratosphere, for example, the equatorial latitudes are the coldest, and heating destroys available potential energy. Our general conclusions can apply only to some sort of vertical average, but neither the outgoing radiation nor the generation of available potential energy depends simply upon vertically averaged temperatures. Finally, we have omitted the possibility that the necessary generation of available potential energy could result from cross-longitude temperature and heating contrasts, which might perhaps arise from the cross-longitude contrast between land and ocean. Nevertheless, we feel that our explanations, while not rigorous, are essentially correct.

Accompanying the poleward temperature gradient there must be a poleward pressure gradient at higher levels or an equatorward pressure gradient at lower levels, or both. Absence of these gradients would require too small a vertical pressure gradient at high latitudes or too large a vertical pressure gradient at low latitudes to be in hydrostatic equilibrium. There would then have to be net downward acceleration in high latitudes or net upward acceleration in low latitudes. The full explanation for hydrostatic equilibrium is rather complex, involving an explanation of the typical scale of motion, but at this point we are concerned only with the equilibrium of the average state over an extensive region of the atmosphere. We must nevertheless turn to quantitative considerations. A net upward acceleration over an extensive region in low latitudes requires that the air leaving the region be moving upward more rapidly than the air entering the region. In the present instance, the velocities which would be required at the boundary of the region in the absence of the stipulated pressure gradients would be greater than any which could be maintained against friction, even if solar heating produced available potential energy with the maximum possible efficiency.

At the Earth's surface as a whole neither easterly nor westerly winds can predominate. This follows because there can be no net long-term transfer of angular momentum from the Earth to the atmosphere, and hence no net surface torque. Either easterly winds must prevail at some latitudes and westerlies at others, or else there can be no systematic distribution of surface easterlies and westerlies at all.

It is natural to conclude that general westerly winds must predominate aloft. Absence of these winds would require too large a poleward pressure gradient aloft, or too large an equatorward pressure gradient at the surface, or both, to be in geostrophic equilibrium. Hence there would have to be net poleward acceleration at high levels or net equatorward acceleration at low levels. Like the explanation for hydrostatic equilibrium, the full explanation for geostrophic equilibrium is rather complex, but again we are at present concerned only with the equilibrium of an average state over an extensive region. However, in this case the possibility of non-geostrophic flow cannot be eliminated by energy considerations alone.

A highly non-geostrophic circulation, in which the pressure decreases equatorward at low levels and poleward at high levels, while there are no systematic easterly or westerly winds at any level, is apparently possible if there is a downward transport of northward momentum across all levels, attaining its maximum value in middle levels. The downward transport could be accomplished by "mesoscale" systems having horizontal dimensions of perhaps a hundred kilometres, in which the poleward-moving air sinks and the equatorward-moving air rises. We shall presently consider geostrophic equilibrium in greater detail; in the meantime we shall merely assume that no mechanism exists for maintaining the mesoscale systems which would bring about the needed downward transport of northward momentum. In that event quasi-geostrophic westerly winds must prevail at upper levels.

The preceding are the features of the circulation which are most readily deduced from basic principles, even if not in a completely rigorous fashion. They include a poleward transport of energy, which is needed to hold the poleward temperature gradient below its thermal-equilibrium value, and thereby enable the heating to produce available potential energy. No conclusions have been drawn concerning the poleward transport of absolute angular momentum; therefore no explanation for the latitudes occupied by the surface easterlies and westerlies has been offered.

One circulation scheme compatible with the properties so far deduced is the zonally symmetric circulation of Hadley, possibly with the modifications introduced by Thomson and Ferrel. In such a circulation there must be general poleward flow at upper levels and equatorward flow at lower levels in order to bring about the necessary poleward transport of energy, since sensible heat plus potential energy increases with elevation in a stably stratified atmosphere. The mesoscale eddies which in the more general case might produce a downward transport of northward momentum are certainly absent, so that upper-level westerlies must be present. Thus the direct meridional cell also brings about a poleward transport of angular momentum, and the surface winds are easterly in low latitudes and westerly in higher latitudes.

Although the Hadley circulation is consistent with the governing physical laws, it does not occur, because it is baroclinically unstable. Baroclinic instability is favoured by a large Coriolis parameter, large vertical wind shear with its accompanying large horizontal temperature gradient, and low vertical stability. In the Hadley circulation these conditions are met in middle and high latitudes. Possibly they are not met in the tropics, where the Coriolis parameter and the poleward temperature gradient are smaller, but the separate latitudes do not act independently of one another, and the Hadley circulation as an entity is unstable. A circulation containing longitude-dependent eddies therefore occurs in its stead. Quantitative considerations are required to determine whether any particular Hadley circulation is baroclinically unstable. That the Hadley circulation belonging to the Earth's atmosphere should meet the conditions for instability must be considered accidental. It is conceivable that the atmosphere of a more slowly rotating planet could possess a stable Hadley circulation.

The eddies must lose kinetic energy through frictional dissipation. Under the assumption that the warmer portions of the eddies will radiate more heat to space than the colder portions, the eddies must

lose available potential energy by heating. They must therefore gain either available potential energy or kinetic energy from the zonally averaged flow. In the former case they must bring about a cross-latitude transport of sensible heat, and in the latter case a cross-latitude transport of angular momentum. In either event the zonally averaged flow must then differ from the Hadley circulation which would prevail if the eddies were absent.

The principal remaining properties of the circulation, including the occurrence of hydrostatic and geostrophic equilibrium, and the distribution of the surface easterly and westerly winds, depend upon the scale and structure of the eddies. We consider first the qualitative properties of quasi-hydrostatic equilibrium, quasi-horizontal motion, quasi-geostrophic equilibrium, and quasi-non-divergent motion. Small scales of motion are of course not geostrophic, and the very smallest scales are not even hydrostatic. We are interested in explaining why most of the energy of the atmosphere is in the hydrostatic and geostrophic modes.

It is sometimes stated that the flow must be quasi-horizontal and quasi-hydrostatic because the effective vertical depth of the atmosphere is extremely small compared to its horizontal extent. This argument does not appear sound. There is nothing physically impossible about a thin layer of fluid in which the principal motions or perhaps the only ones are vertically propagating sound waves, which are decidedly not hydrostatic. Likewise, even under quasi-hydrostatic conditions, the motions in such a layer could be confined to gravity waves, which are non-geostrophic. The relative unimportance of such motions in the atmosphere is due to the absence of the processes which would be needed to produce and maintain them.

The adjustment of an initially unbalanced flow toward geostrophic equilibrium has been studied by Rossby (1938b) and in greater detail by Obukhov (1949). However, the adjustment considered by these authors is local, occurring at the expense of geostrophic equilibrium elsewhere. The circulation as a whole becomes neither more nor less geostrophic during the process.

The problem may be clarified through a consideration of scale theory, in the manner suggested by Charney (1948). Assuming that the motions have horizontal velocities, horizontal and vertical dimensions, and time scales typical of the principal motions in the atmosphere, and applying order-of-magnitude considerations to the various terms in the governing equations, Charney finds that the motions must be nearly hydrostatic, horizontal, geostrophic, and non-divergent. The problem of explaining these properties thereby becomes equivalent to the problem of explaining the typical observed scales of atmospheric motions. It is therefore only slightly less involved than the whole problem of the general circulation, which requires an explanation of the shapes of the systems of motion as well as their dimensions and amplitudes.

It is thus obvious that hydrostatic and geostrophic equilibrium cannot be explained, any more than can the whole circulation, without considering the thermal forcing. The principal component of the forcing, the Equator-to-Pole gradient of heating, is of large scale and infinite period, and the motion which it directly forces, namely the Hadley circulation, is likewise of large scale and infinite period. We have already noted that the Hadley circulation is nearly hydrostatic and geostrophic. It is sometimes considered non-geostrophic because the transports of energy and momentum are accomplished entirely by the small non-geostrophic meridional motions. Nevertheless the zonal flow, which contains most of the kinetic energy, is approximately geostrophic.

In the real atmosphere the seasonal variations of heating provide another large-scale long-period component of the forcing. The diurnal variations provide a further component which has a large scale but a short period. The latter component forces the well-known thermal atmospheric tides which are

decidedly non-geostrophic, but which contain only a minor amount of the total energy, and which in general do not appear to interact too strongly with the remaining motions.

Somewhat smaller-scale motions are directly forced in the real atmosphere as a result of the heating contrast between continents and oceans, or between smaller geographical features, but most of the smaller-scale motions, either in the real or the idealized atmosphere, result from the non-linear interactions of larger-scale motions. Among the smaller-scale motions are those resulting from instability.

Instability, regardless of how readily it may be investigated by linearized equations, is a non-linear process. It is a special case of the non-linear interaction of two superposed fields of motion to produce a third, when the amplitude of the first of the interacting fields is much larger than that of the second. Because of the disparity in amplitudes, the amplitude of the first field remains nearly constant, and may be treated as a constant in the equations governing the second and third fields.

Typical studies of the baroclinic instability of zonally symmetric motion similar to that occurring in the atmosphere indicate that the most rapidly growing disturbances have the proper space and time scales to be nearly hydrostatic and geostrophic, according to scale theory. If we postulate that fully developed eddies have the same scales as the most rapidly growing small-amplitude eddies superposed upon the same basic flow, we obtain a fairly acceptable explanation for hydrostatic and geostrophic equilibrium. Here a word of warning is needed. The studies indicating that hydrostatic and geostrophic modes of motion amplify most rapidly are based for the most part upon equations which contain the hydrostatic and geostrophic approximations, and which are therefore incapable of revealing the possible growth of non-hydrostatic or non-geostrophic modes. At the present time it can only be assumed that further non-linear processes, whether interactions of two modes of eddy motion which are each demanded by the instability of the zonally averaged flow, or instabilities of a zonal flow plus a single mode with respect to still further modes, will lead, only to new motions also having the proper space and time scales, or else to motions of such small amplitude that they are not important components of the total circulation.

The remaining properties of the circulation depend upon the shapes of the eddies as well as their sizes. Thus a poleward transport of angular momentum is generally produced by a pattern of troughs and ridges which are displaced eastward with increasing latitude, while a poleward transport of sensible heat is produced by troughs and ridges which are displaced westward with increasing elevation. We regard the problem of explaining the pattern of the transport of angular momentum by the eddies as the most important problem in general-circulation theory among those for which we now lack a fairly adequate qualitative explanation. The convergence of the angular-momentum transport exerts a controlling influence upon the latitudes chosen by the surface easterlies and westerlies. It also tends to disrupt the geostrophic equilibrium aloft, and thereby leads to the formation of direct meridional cells in low latitudes and indirect cells in middle latitudes, which tend to restore the equilibrium. Thus the cells in the tropics are much stronger than they would be in the Hadley circulation. These direct cells transport large amounts of water equatorward, which then produce the heavy precipitation in the tropics. They also transport sensible heat plus potential energy away from the Equator.

The earliest attempts to explain the eddy transport of angular momentum were based upon an analogy with classical turbulence theory. The eddies were assumed to transport angular momentum toward latitudes of lower angular velocity, so that the stronger westerlies would effectively drag the weaker westerlies ahead. However, there is no physical basis for applying classical turbulence theory to eddies of cyclone scale; indeed the assumption that all eddies transport angular momentum in accordance with diffusion concepts is equivalent to the assumption that all zonally symmetric flows other than solid rotation are barotropically unstable. Moreover, the theory would yield incorrect results, since throughout

most of the tropics and subtropics angular momentum is transported toward latitudes of stronger westerlies.

Studies of baroclinic stability of flow on the beta-plane, such as the one by Pedlosky (1964), yield an angular-momentum transport of the proper sign at low latitudes, but they also indicate a counter-gradient transport on the poleward side of the westerly-wind maximum, where it is not actually observed. Possibly the latter result is only a shortcoming of the beta-plane, since the numerical experiments performed on the beta-plane, such as that of Phillips (1956), yield similar results. However, studies based upon perturbation theory cannot apply to finite-amplitude disturbances, unless further hypotheses are introduced.

The postulate that finite-amplitude eddies superposed upon a zonally averaged flow should have the same shape as the most rapidly amplifying incipient disturbances superposed upon the same flow would lead to simpler and more regularly shaped eddies than those actually found. Classical turbulence theory, on the other hand, would lead to more irregular eddies than those observed. In either event the eddies would be assumed to possess some equilibrium form determined by the zonally averaged flow.

We feel that there are good reasons for believing that the properties of the eddies cannot be represented in terms of the current zonal flow. Let us assume that there does exist some equilibrium configuration which the eddies would ultimately attain if the zonal flow did not vary. The time required for eddies of cyclone scale to reach approximate equilibrium might be one or two days. But during this time the zonal flow will vary, largely as a result of the transport of angular momentum and energy by the eddies. The new zonal flow will demand a new equilibrium configuration, and in approaching this new configuration the eddies will further alter the zonal flow, etc., and no equilibrium will ever be reached. We may note that these considerations do not preclude the possibility that the effects of small-scale eddies can be fairly well represented in terms of the larger-scale flow, since small-scale eddies may attain an equilibrium configuration in the course of an hour or less, while the larger-scale flow should remain reasonably constant for considerably longer.

Despite these observations, the postulate that finite-amplitude eddies possess the same general shape as rapidly amplifying infinitesimal eddies leads to a number of correct conclusions. Sensible heat is transported toward colder latitudes, so that the eddies gain available potential energy from the zonal flow. Angular momentum is transported predominantly toward latitudes of higher angular velocity, so that the eddies give up kinetic energy to the zonal flow. We should therefore note that any explanation based upon the theory of baroclinic stability has departed considerably from the type of qualitative explanation which we presented earlier in this chapter. The mathematical work required to find the form of the most rapidly amplifying disturbance, when the zonal flow varies both horizontally and vertically, is extremely involved. The investigator who has solved the problem may still gain little physical insight as to why the eddies prove to have one particular shape rather than another.

If the eddies are to transport angular momentum into the latitudes of maximum westerlies, as they apparently do on the beta-plane, the trough and ridge lines must assume somewhat the same shape as the westerly wind profile itself, with their maximum eastward positions coinciding with the maximum westerlies. The troughs and ridges therefore act somewhat in the manner of elastic bands, which are stretched out by the zonal flow, but are prevented by the elastic restoring force from being pulled too greatly out of shape. Yet we can offer no simple explanation as to why the troughs and ridges should behave in this manner. Their typical shape on the sphere presents an equally perplexing problem.

Simple qualitative arguments could perhaps be offered to explain some of the principal remaining features, such as the equatorward temperature gradient in the stratosphere and the reversed stratospheric

energy cycle. By this point, however, our explanation of the general circulation has become so incomplete that we shall not attempt to extend it any further.

In closing this monograph, we must express an intuitive conviction that a complete qualitative explanation of the principal features of the general circulation will eventually be found. It seems possible, for example, that there should be some system of equations and ordered inequalities, with as many unknowns as relations, which will yield the rigorous result that the poleward eddy-transport of angular momentum across middle latitudes is greater than zero. It seems further possible that this rigorous qualitative result might then be converted into a comprehensible qualitative argument. To our knowledge, however, the desired system of relations has yet to be found.

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LIST OF SYMBOLS

Symbols containing subscripts are not separately listed except where the subscript alters the meaning of the symbol. A subscript o denotes a value at the Earth's surface. Subscripts z and e attached to the symbols A, K, G, C, D denote zonal and eddy quantities. Subscripts λ, φ, z denote the components of a vector.

A	Available potential energy
a	Mean radius of Earth, 6.37×10^3 km, (or radius of container in laboratory experiment)
b	Inner radius of annulus in laboratory experiment
C	Rate of conversion of available potential energy into kinetic energy by reversible adiabatic processes
c	Specific heat of liquid water, 41.85×10^6 cm^2 sec^{-2} deg^{-1}
C_A	Rate of conversion of zonal available potential energy into eddy available potential energy
c_D	Surface drag coefficient
C_K	Rate of conversion of zonal kinetic energy into eddy kinetic energy
c_p	Specific heat of air at constant pressure, 9.96×10^6 cm^2 sec^{-2} deg^{-1}
c_v	Specific heat of air at constant volume, 7.09×10^6 cm^2 sec^{-2} deg^{-1}
D	Rate of dissipation of kinetic energy by friction
E	Rate of upward turbulent transfer of water vapour, per unit horizontal area
E_0	Rate of evaporation from Earth's surface, per unit area
$\mathbf{F}$	Frictional force per unit mass
f	Coriolis parameter
G	Rate of generation of available potential energy by heating
$\mathbf{g}$	Acceleration of gravity
g	Mean magnitude of $\mathbf{g}$, 981 cm sec^{-2}
H	Rate of production of internal energy by heating
h	Depth of fluid in laboratory experiment
I	Internal energy per unit mass
$\mathbf{i}$	Unit vector directed eastward
$\mathbf{j}$	Unit vector directed northward
K	Kinetic energy per unit mass
$\mathbf{k}$	Unit vector directed upward
k	Average number of arithmetic operations needed to compute a single time derivative in a numerical experiment
L	Latent heat of condensation
M	Absolute angular momentum per unit mass about Earth's axis
m	Number of dependent variables in a numerical experiment
N	Efficiency factor, $1 - p^{-\kappa} P^\kappa$
n	Total number of time steps in a numerical experiment
P	Average pressure on an isentropic surface

p	Pressure
P_0	Rate of precipitation, per unit area
P_{00} . . .	Standard pressure, 1000 mb
Q	Rate of heating per unit mass
q	Specific humidity
q_s	Specific humidity of saturated air at given pressure and temperature
R	Gas constant for air, $c_p - c_v$, 2.87×10^6 cm^2 sec^{-2} deg^{-1}
$\mathbf{r}$	Position, with respect to Earth's centre
r	Magnitude of $\mathbf{r}$, distance from Earth's centre
R_o	Rossby number
R_{OT} . . .	Thermal Rossby number
R_w	Gas constant for water vapour, 4.62×10^6 cm^2 sec^{-2} deg^{-1}
S	Area of Earth's surface
s	Specific entropy
T	Absolute temperature
t	Time
T_a	Taylor number
T_v	Virtual temperature
$\mathbf{U}$	Horizontal wind velocity, horizontal projection of $\mathbf{V}$
u	Eastward component of $\mathbf{V}$
$\mathbf{U}_d$	Divergent irrotational part of $\mathbf{U}$
$\mathbf{U}_g$	Geostrophic wind velocity
u_g	Eastward component of geostrophic wind
$\mathbf{U}_r$	Rotational non-divergent part of $\mathbf{U}$
$\mathbf{V}$	Wind velocity
v	Northward component of $\mathbf{V}$
w	Upward component of $\mathbf{V}$
X	Arbitrary dependent variable
x	Eastward distance on beta-plane
y	Northward distance on beta-plane
z	Elevation, measured upward
α	Specific volume
β	Derivative of Coriolis parameter with respect to northward distance, df/dy
Γ	Vertical lapse rate of temperature, $-\partial T/\partial z$
γ	Ratio of specific heats of air, c_p/c_v, 1.405
Γ_d	Dry-adiabatic lapse rate, g/c_p, 9.8°/km
δ	Horizontal divergence, $\nabla \cdot \mathbf{U}$
ε	Coefficient of thermal expansion, $d (1)na/dT$
ζ	Vorticity, $\nabla \cdot \mathbf{U} \times \mathbf{k}$
η	Efficiency of atmospheric energy cycle
θ	Potential temperature
κ	R/c_p, 0.288
λ	Longitude, measured eastward
μ	Coefficient of turbulent viscosity
v	Kinematic viscosity
ρ	Density
σ	Static stability factor, $-(T/\theta)\partial\theta/\partial p$

τ . . . . . Frictional stress per unit horizontal area

Φ . . . . Potential energy per unit mass, geopotential

φ . . . . Latitude, measured northward

χ . . . . Velocity potential for divergent velocity

Ψ . . . . Stream function for meridional circulation

ψ . . . . Stream function for rotational velocity

$\boldsymbol{\Omega}$ . . . . Angular velocity of Earth (or angular velocity of container in laboratory experiment)

Ω . . . . Magnitude of $\boldsymbol{\Omega}$, 7.292×10^{-5} sec^{-1}

ω . . . . Individual pressure change, dp/dt

$\{(\ )\}$ . . . Integral over entire mass of atmosphere

$\widetilde{(\ )}$ . . . . Horizontal average

$(\ )''$ . . . Departure from horizontal average

$[(\ )]$ . . . Longitudinal average

$(\ )^{\star}$ . . . Departure from longitudinal average

$\overline{(\ )}$ . . . . Time average

$(\ )'$ . . . . Departure from time average