QUARTERLY JOURNAL
OF THE
ROYAL METEOROLOGICAL SOCIETY

Vol. 77 OCTOBER 1951 No. 334

REVIEWS OF MODERN METEOROLOGY—3

551.513 : 551.515 : 551.509.3

Dynamics of flow patterns in extra-tropical regions

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1. Scope

Dynamical meteorology is concerned with the study of the motion of the air, the physical processes (radiation, condensation *etc.*) which are the ultimate cause of this motion, being taken for granted. This study may be pursued in an empirical and descriptive manner and to this extent is included within the scope of synoptic meteorology. Alternatively, or preferably at the same time, it may be pursued theoretically, the aim being to show the causal connection between observed phenomena and to predict new, possibly not yet noticed, relationships. In this article we shall be concerned primarily with the theoretical aspect, advances in synoptic meteorology being described only sufficiently to give a suitable background.

The whole subject of dynamical meteorology as defined above is too vast to be dealt with in one article. We are aware that *scale* plays a very important part in determining the nature of the motion. The motions on a large scale which we infer from synoptic charts are different from the smaller-scale motions inside a cumulus cloud and approximations justifiable in the one case are not so in the other. Because dynamical meteorology is so complicated we are forced to concentrate on one scale of motion at a time. Moreover we may study these motions from two points of view. In the first place we may be interested in the detailed development of one particular situation, as for example the deepening and movement of a depression or the growth and decay of a convection cell. In the second place we may be interested in average or statistical behaviour as, for example, when we study the average effect of disturbances in transferring heat and momentum between latitudes, or when we study the Reynolds stresses associated with turbulent motion. In the present article we shall be concerned only with the first point of view and shall confine our attention to the detailed study of relatively large-scale motions.

Despite this great restriction in scope the subject remains a large one. Neither of the writers feels competent to give a complete résumé of recent work, and the choice of subjects and references to original work is as much determined by their personal interests as by the need for selection. This lack of completeness may not be entirely disadvantageous. Every meteorologist has to be, to some extent, a dynamical meteorologist but not all can afford the time to sift what is new and significant from a spate of papers, filled in some cases with complicated mathematical analysis. More important than the detail is the general trend of recent developments.

The aim of this article is to elucidate some of the basic concepts and lines of thought in modern research. At last, we are beginning to get *all* the data needed for understanding how weather develops. New data have stimulated theoreticians as well as synopticians because in a complicated subject it is a great help to know the kind of behaviour that has to be explained. Moreover the new facts are enabling us to weed out misconceived hypotheses, so that in spite of apparent divergences there is beginning to develop a generally acceptable and relevant body of theory.

2. Recent advances in descriptive aspects of dynamical meteorology

The most notable advances in our knowledge of the movements of the atmosphere have come from the network of radiosonde and radio-wind observations established during and after the Second World War. This network is sufficiently close and extensive to permit the twice-daily construction of isobaric charts for several different levels in the troposphere and lower stratosphere extending over most extra-tropical parts of the northern hemisphere. In some areas more frequent observations are available. These new observations have served to confirm many of the ideas of the structure of depressions established, on the basis of limited numbers of aircraft ascents and 'aerological-day' observations, by J. Bjerknes, E. Palmén, J. van Mieghem and others. However the new observations have also brought into prominence several aspects of atmospheric flow which were not previously emphasized.

It is found that only part of the thermal contrast between air masses lies at the fronts, the baroclinity, *i.e.* the thermal change in isobaric surfaces, extends into the air masses themselves (Palmén 1948, Palmén and Newton 1948). It is also noted that the direction of the vertical wind shear is usually almost constant through a vertical column. Consequently, when for theoretical purposes it is necessary to adopt a simplified model of the atmosphere, it is possible to consider a baroclinic atmosphere with similar temperature gradients at all levels. This simplification seems to be in direct conflict with the frontal model, but both are useful and the atmosphere is intermediate in structure.

The technique most used in the study of the dynamics of the atmosphere has been that of constant-pressure analysis with particular emphasis on the contour chart of the 500 mb surface. As soon as charts were drawn for a large part of the northern hemisphere several features became apparent (Staff, University of Chicago 1947). It was found that the upper westerly winds were concentrated into a broad sinuous band which extended round the hemisphere in middle latitudes, and was shown on the 500 mb chart by closely packed contours in a wavy band with relatively slack gradients in low latitudes and towards the poles. The troughs in this band were found to be irregularly spaced at intervals usually of from 40 to 100 degrees of longitude (but sometimes more or less) with intervening ridges. These disturbances of the zonal flow have come to be known as *long waves*. It is not possible here to describe or illustrate in detail the behaviour of the long waves, but it is desirable to refer to some of their observed characteristics.

The long waves are slow-moving features of the synoptic chart compared with individual depressions and anticyclones which often appear on the 500 mb charts as relatively small distortions superimposed on the pattern of the long waves. Nevertheless, there is no clear-cut difference of scale – the size of a large depression or anticyclone in the surface-pressure field is similar to that of one of the shorter 'long waves' in the 500 mb flow, and the atmosphere appears to be subject to disturbances of any wavelength from a few miles to 3,000 miles or more.

Although more persistent than many individual depressions the long waves rarely move regularly or maintain their form over a period of more than 2 or 3 days. They may increase or decrease in amplitude and new troughs and ridges may be added to the wave train. Unlike frontal wave depressions which usually go through a systematic process of occlusion, the long waves do not systematically increase in amplitude. However, on some occasions their amplitude does become very large and results in the breaking off from the wave train of anticyclonic eddies to the north and cyclonic eddies to the south. When a cyclonic eddy is formed to the south, the main current of westerlies reforms further north, but when an anticyclonic eddy forms out of a distorted ridge in the flow pattern only a weak branch of the westerlies passes to the south. The formation of such an anticyclonic circulation at 500 mb is usually associated with a warm anticyclone on the surface chart; the process is known as *blocking*, because of the interruption of the normal westerly flow. Once a *block* of this nature is established it may last for a week or more before the westerlies are resumed (Palmén and Nagler 1949).

Following the suggestion of the Chicago school the mainly westerly current which extends round the hemisphere in middle latitudes has come to be known as the *jet stream*. Rossby has likened it to a meandering river of fast flowing air between relatively stagnant masses to north and south. Cross-sections of the atmosphere (Palmén and Newton 1948) drawn north to south through this current have drawn attention both to the high velocities attained in the upper troposphere and also to the relatively narrow range of height and latitude into which they are concentrated.

Sharp maxima and high velocities are probably not typical of the westerlies on all occasions or around the whole hemisphere. The strongest and most clearly defined maxima of wind seem to occur in rather straight stretches within the westerlies. There is some confusion as to whether the name jet stream should be applied to these well defined maxima or to the whole belt of meandering westerlies. The sharp wind maxima of the upper troposphere are closely associated with the polar front. They are situated in the warm air mass. Palmén (1948) has stated that the axis of the jet normally lies above the point where the front reaches the 600-500 mb level, but there is some evidence that the axis is often above a somewhat higher section of the front. Two explanations of the jet stream have been proposed. Rossby (Staff, University of Chicago 1947) suggested that it could be explained as a result of the mixing by horizontal eddies of the air nearer to the pole under conditions which tended to render uniform the absolute vorticity of the air. Such conditions would result in a rapid increase of the zonal wind southward culminating in the jet stream south of which Rossby suggested that mixing transported vorticity from one hemisphere to the other and resulted in a rapid decrease of wind southward. Rossby's explanation is directed primarily to explaining the concentrated westerlies as a feature of the general circulation of the atmosphere. On the other hand J. Namias and P. F. Clapp (1949) were concerned with the local wind maxima when they interpreted the jet stream as a result of the juxtaposition of airmasses of very different temperatures in a fronto-genetical field. Frontogenesis is an observed feature of the entrance to most jet streams but the frontogenetic pressure field at low levels may itself be a dynamical consequence of the presence of the jet (Sutcliffe and Forsdyke 1950). Any complete explanation of the jet stream will probably have to treat the high- and low-level wind fields as one three-dimensional dynamical system.

The wealth of available upper-air soundings has also provided the material for the direct study of the fields of divergence and convergence of the horizontal motion and the estimation of the field of large-scale vertical motion. The problem has been approached by different investigators by several methods – the most direct approach, that of evaluating horizontal divergence from wind observations alone has been adopted by H. G. Houghton and J. M. Austin (1946) ; R. C. Graham (1947) has evaluated vertical velocities thermodynamically, using potential temperature or wet-bulb potential temperature to identify the air parcel ; and R. G. Fleagle (1947) has used an essentially similar technique. J. S. Sawyer (1949) has studied the changes of vorticity of a moving air stream (values were approximated on the geostrophic assumption) and evaluated horizontal divergence therefrom. Further insight into atmospheric dynamics has been obtained from comparisons between observed and geostrophic winds (Godson 1950, Bannon 1949).

In all of this work it has been necessary to extract the utmost from the observations. Observational errors and local fluctuations in wind and temperature introduce errors which are comparable with the quantities which are sought. It is therefore highly satisfactory that a consistent picture can be built up from these various modes of attack. Firstly, the average magnitude of the geostrophic departure (root mean square vector-difference) at the 700 mb level seems to be about 7 to 10 kt. (It is probably greater at higher levels). Secondly with regard to vertical movements, the investigators using methods requiring averages over 12 hr and those studying subsidence (S. Petterssen, *et al* 1947) deduce typical values of about 5 cm/sec. On the other hand investigations which involve less smoothing suggest maximum velocities of 10-20 cm/sec in active cyclonic areas and these are confirmed by estimates from rates of rainfall (Bannon 1948). It therefore seems likely that areas of vertical motion exceeding 10 cm/sec exist on a scale of 200-300 mi, much greater than individual convection clouds, but nevertheless affecting rather limited areas on the synoptic scale. The pattern of convergence, divergence and vertical velocity in association with a pressure trough is given by R. G. Fleagle (1947) in cross-sectional form. Ahead of a trough convergence takes place in the lower troposphere and divergence in the upper troposphere and lower stratosphere ; in the rear of the trough convergence and divergence are interchanged. Adequate confirmation of the broad features of this picture can be found in other work, but much remains to be done in filling in the details in relation to other more complicated synoptic systems. The material is available for this, but the work involved is lengthy and laborious ; moreover it is doubtful if the detail can be improved by present techniques because of the limits imposed by the distance between observations, their errors and by local variations of wind and temperature.

3. Some fundamental problems of atmospheric dynamics

For many years past the equations of motion of a frictionless fluid on the surface of a rotating sphere have formed the starting point for most theoretical studies of atmospheric motions. There are five fundamental equations which govern the motion, three equations for the components of the motion along the three axes, the equation of continuity, and a thermodynamic relation between pressure and density. Using isobaric co-ordinates these equations can be expressed in the form (see Sutcliffe (1947) for Eqs. (1), (2) & (4))

$$\frac{du}{dt} = -\left(\frac{\partial h}{\partial x}\right)_p + lv \qquad (1)$$

$$\frac{dv}{dt} = -\left(\frac{\partial h}{\partial y}\right)_p - lu \qquad (2)$$

$$\frac{\partial h}{\partial p} = -\frac{1}{\rho} \qquad (3)$$

$$\left(\frac{\partial u}{\partial x}\right)_p + \left(\frac{\partial v}{\partial y}\right)_p + \frac{\partial}{\partial p}\left(\frac{dp}{dt}\right) = 0 \qquad (4)$$

$$\frac{1}{\rho}\frac{d\rho}{dt} = \frac{1}{p}\left(1 - \frac{R\gamma'}{g}\right)\frac{dp}{dt} \qquad (5)$$

where u, v are components of the velocity along the horizontal axes of x and y. Pressure p is taken as the vertical co-ordinate and the geopotential h is regarded as a function of x, y and p. ρ is the density, g the acceleration due to gravity and γ' the adiabatic lapse rate for dry or saturated air as appropriate. In deriving the equations in the form given above several terms are neglected which can be shown to be insignificant in large-scale atmospheric motions. The motion is nominally referred to rectangular axes with the x, y plane tangential to the earth's surface ; however, the Eqs. (1) to (3) are then only strictly true at the origin. Nevertheless, it is usual to interpret the equations as though referring to a curvilinear set of co-ordinates which conform to the figure of the earth and of which the p-axis is vertical. The Coriolis parameter $l = 2\omega \sin \phi$, then becomes a function of y and this may be important in motion on the scale of the long waves. Further approximations are introduced into Eqs. (1) to (4) when we interpret them with respect to a curved co-ordinate system. McVittie (1949) has shown how the equations of motion can be placed on a fundamentally more sound basis by the strict use of curvilinear co-ordinates. However, there is little reason to believe that the apparent inconsistencies of the rectangular treatment introduce any errors which are large enough to be significant when compared with approximations inevitably introduced in later stages of the work.

More important is the omission from Eqs. (1) to (3) of terms due to friction, because fundamentally the atmospheric motion is the result of a balance between the generation of kinetic energy from potential energy and its dissipation in friction. However, most studies of atmospheric dynamics have been based on the assumption that friction has a relatively small effect on large-scale motions over a period of 24 hr, and this is justified by consideration of the rate of dissipation of kinetic energy.

The fundamental equations have solutions which represent oscillations on several different scales. Thus there are solutions corresponding to sound waves and gravity waves of a few hundred metres length, as well as those representing oscillations on the synoptic scale. The need to modify the equations to represent only the oscillations on the cyclonic scale has led Charney (1948) to study further the effect of scale on the equations of motion ; *i.e.* the facts that the oscillations which we wish to study have a horizontal extent of 1,000 km or more, a much smaller vertical depth of 10 km and a period of the order of a day. He points out that large-scale atmospheric motion is characterized by approximately geostrophic flow,

$$lu = -\left(\frac{\partial h}{\partial y}\right)_p \;,\; lv = \left(\frac{\partial h}{\partial x}\right)_p \text{ approximately} \quad . \quad . \quad (6)$$

Charney proposes to make use of this approximation to simplify Eqs. (1) to (4) and he claims thereby to eliminate the small-scale short-period solutions. However, it is first necessary to modify the equations by expressing them in terms of the vorticity and divergence of the horizontal flow. Thus following Sutcliffe (1947) and omitting some further terms which are believed to be small in large-scale atmospheric motion, we obtain

$$\frac{d}{dt}(\zeta + l) = -(\zeta + l)\,\mathrm{div}_p V \quad . \quad . \quad . \quad . \quad (7)$$

where $\zeta = (\partial v/\partial x)_p - (\partial u/\partial y)_p$, approximately the vertical component of vorticity. Eq. (7) is often referred to as the ' vorticity equation.'

It appears that if we substitute from the geostrophic-wind relation in the expression for the vorticity we obtain a result which is correct to a degree of approximation similar to that of the geostrophic assumption. On the other hand the value of the divergence obtained in this way would always be zero. This is because the divergence is of a smaller order than the vorticity. Nevertheless the divergence is of great importance in the synoptic problem because of its direct connection with the vertical velocity and change of pressure on the moving particle through the equation of continuity (4). Moreover in the vorticity equation, which relates changes in vorticity to the divergence, the vorticity may be approximated by the geostrophic assumption and the equation still gives valid approximations to the divergence. Charney proposes to eliminate the divergence between equations corresponding to (7) and (4) and to substitute for u, v and ζ from the geostrophic condition in the resulting equation. This gives a differential equation involving as variables only the geopotential, h, and the rate of change of pressure on the moving particle, dp/dt. The elimination of dp/dt and the density, ρ, between the resulting equation and Eqs. (4) and (5) is still necessary, but finally a differential equation can be obtained containing only one variable, the geopotential, h. The solution of such a complicated differential equation under appropriate boundary conditions is likely to be difficult, but it does seem that the approach to numerical solution of the equations of motion could follow these lines which recognize the essential characteristics of large-scale atmospheric motion. Up to the present time, however, numerical solution has only been attempted in the essentially simpler case in which the flow is restricted to be horizontal and density changes are ignored (the barotropic model).

The so-called ' tendency equation ' has formed the basis of many discussions of dynamical problems. This equation expresses the fact that the change in pressure at the ground is equal to the change in the weight of air above.

$$\left(\frac{\partial p}{\partial t}\right)_0 = g \int_0^\infty \frac{\partial \rho}{\partial t} dz$$

However, the equation is unsatisfactory for evaluating the important surface-pressure change because it is found that in most synoptic systems the divergence at one level approximately balances the convergence at another (Fleagle 1947). It is indeed a direct consequence of the approximate geostrophic balance and Eq. (7) that surface-pressure changes can only occur with changes of the vorticity of the flow at low levels and these require convergence and divergence much larger than is necessary to produce the change of mass needed to effect the pressure change. Consequently, changes of surface pressure usually occur with compensating convergence and divergence in the column above, and the change of mass in the column is due to a rather small residual divergence.

Attempts made to use the tendency equation as a basis for forecasting are thus unlikely to prove successful. Priestley (1947) also finds this conflict between opposing factors when he attempts to substitute the divergence at several levels into the tendency equation. His divergence has been obtained from the gradient-wind condition. The gradient-wind condition and the tendency equation also form the basis of the largely descriptive account of the dynamics of cyclonic systems given by Bjerknes and Holmboe (1943).

4. Long waves : barotropic theory

Meteorologists have for long been aware of the close relationship between dynamical meteorology and classical hydrodynamics, yet hitherto there have not been very many successful attempts to apply hydrodynamic methods and principles to dynamical meteorology. One of the difficulties has been that the interesting features often depend on the fact that the density is not uniform and then much of classical hydrodynamical theory is inapplicable. Another difficulty is that the motion is three-dimensional and very complicated. Yet it might be that *some* features of atmospheric motion could be studied in an approximate manner by ignoring the actual baroclinic nature of the atmosphere and also the vertical motion.

It was by making sweeping approximations of this sort that Rossby was led to his theory of long waves. The approximations were made tentatively and their justification is that there is in fact *some* resemblance between the theory and observed behaviour. The aim is not a completely satisfactory theory but rather to find a way of making a start towards a realistic theory. With this ' experimental ' attitude let us replace the actual flow by a flow pattern which is similarly distributed in the horizontal but which does not vary with height, *i.e.* by a ' barotropic model.' We ignore the vertical motion so that our model flow is strictly horizontal. Consider the case of a westerly current with a slight sinusoidal oscillation (*i.e.* a uniform current plus a sinusoidal ' small amplitude perturbation '). Now for horizontal barotropic motion it is easily proved from the equations of motion that the vertical component of absolute vorticity $(l + \zeta)$ is a conservative quantity, *i.e.* any particle takes its absolute vorticity with it (if we ignore friction). This follows from Eq. (7) by inserting the condition $\mathrm{div}_p V = 0$. Here l, the Coriolis parameter, is the vertical component of vorticity of air at rest on the earth and ζ is the vorticity of the motion relative to the earth. (Vorticity may be thought of as twice the angular rate of rotation of the air in the immediate neighbourhood of the particle). It follows that as a particle moves towards the pole its relative vorticity must decrease (since l increases) *i.e.* its motion must become less cyclonic or more anticyclonic. Similarly motion towards the equator implies more cyclonic motion. In the case we are considering the vorticity depends only on the curvature of the path (the motion is similar at all latitudes). It is clear then that our assumption of sinusoidal motion is consistent with the vorticity equation, for motion towards the equator leads to a more and more rapid cyclonic turning and eventually the air must begin to move polewards. Later the turning becomes anticyclonic and eventually motion towards the equator again occurs. For one particular wavelength there is also quantitative agreement. If L is the wavelength of the oscillation and U the speed of the westerly current we can satisfy the vorticity equation for a stationary pattern if

$$U = \frac{L^2}{4\pi^2}\frac{dl}{dy} \qquad (8)$$

If instead of supposing the pattern stationary we allow it to move with speed U_w (*i.e.* the wave velocity) then if U_0 is the mean speed of the westerly current

$$U_0 - U_w = \frac{L^2}{4\pi^2}\frac{dl}{dy} \qquad (9)$$

determines the velocity of a pattern with wavelength L. We note that the waves move more slowly than the mean current.

If we are to apply this barotropic theory to the actual baroclinic atmosphere we must make some hypothesis regarding the appropriate value of U_0 which in practice is a function of height. Since baroclinic long waves extend throughout the troposphere and even some distance into the stratosphere it seems reasonable to interpret U_0 as the *average* zonal flow in this layer, which in practice corresponds roughly to the actual current at about 500 mb. If we observe the motion of apparently well-marked waves at this level we get the impression that they do in fact move with a speed less than that of the mean flow. A more precise, quantitative check is difficult for several reasons. In the first place waves are easily observed only if they have not too small an amplitude so that a theory of small perturbations cannot be quite accurate; also, a practical matter is the fact that simple quasi-sinusoidal waves moving without development are not too common. Moreover troughs sufficiently long (in the N. to S. direction) to justify the application of a formula for infinitely long ones are not observed. In principle the formula can be modified, and troughs which vary in amplitude in the N. to S. direction or which are inclined to the meridian have an effective wavelength different from (normally shorter than) infinite troughs with the same E. to W. wavelength. However in practice this effective wavelength is difficult to estimate. Apart from this the wavelength is usually a function of latitude and it is not obvious whether we should expect the formula to apply at all latitudes or only to mean conditions (averaged over latitude, perhaps weighted in proportion to the amplitude of the wave). There is also great difficulty in estimating U_0. Not only is this sensitive to the exact height which we suppose to be appropriate but this also is a function of latitude and the answer depends greatly on the width of the zone over which we take the mean. It is not surprising that attempts to check Rossby's formula have not led to universal agreement.

It is quite certain that we cannot explain *all* the features of atmospheric motion in terms of a barotropic model. (For example a barotropic atmosphere could not generate kinetic energy and would eventually, owing to the action of friction, come to rest everywhere. Moreover we know that absolute vorticity in the 500 mb surface is not accurately, sometimes not even approximately, conserved). Nevertheless the qualitative success in regard to the behaviour of long waves has encouraged further investigation of the properties of the barotropic model.

One application of barotropic theory is to the waves associated with geographical features such as mountain ranges and plateaux. The effect of an obstruction in an air stream is complex but it is easily shown that an important feature of the resulting flow pattern consists of *stationary* waves. These waves could exist (*i.e.* they satisfy the equations of motion and boundary conditions) if the obstruction were *not* present. Precisely for this reason when the obstacle *is* present these ' components ' must be enormously amplified (as compared with other components) if the boundary conditions are still to be satisfied. In general an obstacle will set off in this way all the possible stationary waves of whatever kind. For example a mountain range will set off ordinary gravitational waves (*lee waves*) with a wavelength of the order of 10 mi. (We shall refer to this problem later). It is evident that the barotropic model is inappropriate for *this* kind of investigation but on the other hand it is evident from Eq. (2) that a different kind of wave (constant-vorticity wave) would also be stationary if the wavelength were suitably adjusted, *i.e.* if Eq. (8) applied with $U = U_0$ the mean flow speed. As a corollary we should expect to see evidence on the upper-air climatic charts that the principal mountain ranges obstructing the flow of the westerlies (the Rocky mountains with parallel ranges and plateaux and the mountain masses and plateaux of E. Asia) are associated with ' long lee waves.' To what extent the observed patterns are to be explained from this point of view is not completely clear, quite apart from the hypothesis inherent in the use of a barotropic model.

The barotropic theory of both travelling and stationary waves may be extended by including another dimension, *i.e.* by abandoning the assumption that the flow pattern is the same at all latitudes. We are then led to more complicated mathematical problems but the solutions are essentially of the same kind as those discussed above.

Another idea which stems from Eq. (9) is that of the *dispersion of wave energy*. If, for simplicity, we retain the assumption that the motion is similar at all latitudes and also that the disturbances may be regarded as small in amplitude, then we may by Fourier analysis split up a complex oscillation into components of various wavelengths. In general there will be components of all wavelengths. Suppose that in the analysis we find a certain *range* of wavelengths dominant. Then our disturbance is to be regarded not as a single wave but as a *wave-packet* (or series of wave-packets). There is an active region (the wave-packet) where the components re-inforce one another and the oscillatory energy is large. Elsewhere the components interfere and the oscillatory energy is small. Now one very interesting feature is the motion of the wave-packet, the region of maximum oscillation, and this in general does not travel at the same speed as any of the component waves. The wave-packet moves with what is called the group velocity U_g and if U_w is the velocity of a component (*i.e.* phase velocity) the group velocity is given by the equation,

$$U_g = U_w - L\frac{dU_w}{dL} \qquad (10)$$

Applying Eq. (2) we obtain :

$$U_g = U_0 + \frac{L^2}{4\pi^2}\frac{dl}{dy} \qquad (11)$$

Thus the velocity of the wave-packet is *greater* than the mean current velocity. Some investigators have inferred that there is evidence of such behaviour in the atmosphere, but as in the case of the individual waves there are difficulties in drawing very definite conclusions. The actual wave-packets are spread over such a wide band of wavelengths that it is difficult both to locate the centre of the packet and to estimate the mean value of L which is the appropriate value to substitute in Eq. (11).

Barotropic theory has been applied in other important investigations. For example Munk has used it to explain many important features of the principal oceanic currents. Of more direct interest to meteorologists is its application to weather forecasting by actual computation, but we shall for the moment postpone discussion of this subject. One further idea may however be mentioned here. We are aware, through the efforts of synopticians, that whatever may be the nature and cause of long waves these do not always maintain their wave character. From time to time the warm tongues and cold troughs become first elongated and later ' cut-off ', leading to quasi-independent cold upper cyclonic vortices and warm upper anticyclones. Wave ideas are no longer appropriate and it is interesting to see what barotropic theory would predict about the motion of vortices.

The idea has emerged that cyclonic vortices are subject to an attractive force towards the poles while anticyclones are attracted towards the equator. Without going into details it is easy to see why barotropic constant-vorticity theory could lead to such a result. For a cyclone moving polewards under this law would decrease its *relative* vorticity and therefore its kinetic energy. Since (we are ignoring friction) total energy is conserved there must be a growth in the energy of translation of the vortex, *i.e.* it could be accelerated in the direction of motion. The argument is valid only as long as we can neglect the motion of the air *outside* the vortex, which is not always the case even initially, and is never true indefinitely ; sooner or later the vortex comes to a stop. The protagonists of this view are not always clear on this point. If they had paid more attention to the boundary conditions they would probably have discovered that the final result strongly resembles the wave equation with wave crests and troughs replaced by vortices. There is thus nothing fundamentally new in this idea. Nevertheless it presents barotropic theory in an interesting light. When we observe the growth of a long wave until cutting-off takes place the cyclonic vortices are formed and maintained on the equatorial side while the upper anticyclones appear on the poleward side. This process is exactly opposite to what has been described above. It follows that the barotropic forces on vortices cannot be the only significant ones since in this case they are evidently overpowered. Of course this kind of result should not have been unexpected. It is important to bear it in mind however because it is a reminder to over-enthusiasts of the barotropic model that we cannot hope to explain *all* the principal features of large-scale motion by such means. Rossby has attempted to get over difficulties such as those indicated by postulating a *cold* anticyclone originating in high latitudes. The argument is weakened because he attempts to take into account changes in potential energy – in other words he effectively abandons the barotropic model. Logically the next step is to take full account of the baroclinic nature of the motion.

5. Development theories based on the relative divergence

Since the early studies of the free atmosphere by W. H. Dines (1914 and 1925) and others, it has been recognized that there must be divergence in the upper atmosphere above a developing depression which (slightly) more than compensates the convergence at the lower levels. The location of the convergence and divergence are reversed in the anticyclone. Sutcliffe (1939 and 1947) has therefore directed attention to the *relative divergence*, which he defines as the difference between the divergence at two levels. It is convenient to consider a simple case of cyclonic development in which divergence takes place in the upper troposphere and convergence near the ground. If we then consider two levels, one in the upper troposphere and one near the ground, the relative divergence is positive ; in anticyclonic development the relative divergence is negative. It is also easily seen that the relative divergence is directly related to the large-scale vertical velocity. It is therefore taken as a convenient measure of cyclonic development.

Cyclonic and anticyclonic development defined in this way are not to be regarded as necessarily leading to the formation of a new pressure centre. Indeed, there are few changes of the surface-pressure field that take place without accompanying fields of convergence or divergence (Sutcliffe 1938).

Sutcliffe (1939) first studied the dynamics of relative divergence by considering the difference between the equations of motion at the two levels. His later treatment (Sutcliffe 1947) is based on the vorticity equation and leads to results which can readily be applied in synoptic practice. Sutcliffe considers the difference between the divergence at two selected pressure levels as given by the vorticity equation in a form such as Eq. (7). Like Charney he makes use of the quasi-geostrophic nature of the flow, and assumes that a valid approximation to the divergence is obtained when the geostrophic wind is substituted for the true wind when inserting the vorticity on the left hand side of (7). Proceeding in this way Sutcliffe obtains the following expression for the relative divergence

$$(\mathrm{div}_p \mathbf{V} - \mathrm{div}_p \mathbf{V}_0) = \frac{1}{l}\left\{ \mathbf{V}_0 . \nabla_p(\zeta_0 + l) - \mathbf{V} . \nabla_p(\zeta + l) \right\} - \frac{1}{l^2}\frac{\partial}{\partial t}\nabla_p{}^2 h' \quad . \quad (12)$$

where h' is the thickness of the atmospheric layer between the two levels.

Sutcliffe also makes use of the fact that the motion at the two levels is not independent but, since it is quasi-geostrophic, it is related through the thermal wind equation with the thickness pattern *i.e.* with the field of h'. The changes in h' arise partly from horizontal advection, partly from vertical motion, and partly from non-adiabatic heating and cooling. The importance of these processes for time-intervals not too large is probably in the order indicated. In the simple case in which the changes in thickness are due only to advection, Eq. (12) can be transformed by further use of the geostrophic wind conditions into the simple and convenient form

$$\text{Relative divergence} \equiv \mathrm{div}_p \mathbf{V} - \mathrm{div}_p \mathbf{V}_0 = -\frac{V'}{l}\frac{\partial}{\partial s}(l + 2\zeta_0 + \zeta') \quad . \quad . \quad (13)$$

where V' is the thermal wind, ζ' is its vorticity, and $\partial/\partial s$ denotes differentiation in the direction of the thermal wind.

Leaving aside the relatively small effect due to the Coriolis parameter, $-\dfrac{V'}{l}\dfrac{\partial l}{\partial s}$, the expression for the relative divergence separates into two terms which can be interpreted easily in terms of a synoptic chart which carries both contours of the 1,000 mb surface (or surface isobars) and thickness lines for a substantial layer of the troposphere. These terms $-\dfrac{2V'}{l}\dfrac{\partial \zeta_0}{\partial s}$ and $-\dfrac{V'}{l}\dfrac{\partial \zeta'}{\partial s}$ may be conveniently referred to as the *thermal steering term* and the *thermal development* term respectively.

Consider first the thermal steering term, $-\dfrac{2V'}{l}\dfrac{\partial \zeta_0}{\partial s}$, associated with a maximum of cyclonic vorticity, a depression, over which a thermal wind exists. There will be positive relative divergence (from Eq. (13)) and therefore cyclonic development on the side of the depression towards which the thermal wind is directed ; similarly anticyclonic development is expected on the opposite side. It follows that the depression should move in the direction of the thermal wind as is frequently observed. Anticyclones too will tend to be steered in the direction of the thermal wind if as has been assumed the thermal steering term alone is operative.

The remaining term, $-V'\dfrac{\partial \zeta'}{\partial s}$, the thermal development term, depends only on the configuration of the thickness lines. It makes an important contribution to cyclonic development in certain recognizable situations within the thermal field, notably downstream from the thermal trough and to the left of a difluent region of the thickness pattern.

The general application of these theoretical ideas to the synoptic chart are described by Sutcliffe and Forsdyke (1950). The theoretical ideas suggest convenient rules for the motion and development of individual depressions and anticyclones. A start has been made on the statistical testing of such rules for the motion of anticyclones and the development of secondary depressions on warm fronts and at the point of occlusion (Sawyer 1949b and 1950).

Eq. (13) ignores the effect of dynamical heating and cooling on the thickness pattern. Apart from other approximations the result can therefore only be strictly true if the atmosphere is in neutral static equilibrium. Sumner (1950) has considered the effect of the existence of a stable lapse rate and has shown that it tends to damp down the development that would otherwise be expected. This damping depends on the size of the system ; it is overwhelming for small systems of diameter less than 400 km but is small for large systems of the order of 2,000 km. Consequently, damping by vertical stability may be an essential factor in determining the size of cyclones and anticyclones.

As has been indicated, Eqs. (12) and (13) are valuable in synoptic meteorology and forecasting because they enable the sign of the development and an estimate of its intensity to be made from synoptic charts which can be drawn as routine. The application to forecasting is immediate. Up to the present the possibilities of the numerical application of Eqs. (12) and (13) have not been fully explored, but it is not unreasonable to hope that if numerical values of the relative divergence could be computed on a field basis it would be of great value to the forecaster. Moreover it is possible that in combination with a modification of Eq. (7), applicable to a level in mid-troposphere, the equivalent barotropic level, Eq. (12) may well form the basis for future work on numerical methods of predicting the surface-pressure field.

Sutcliffe's method is the simplest for examining the properties of atmospheric motion due to its baroclinic nature. It has the advantage, in common with barotropic theory, that it is not necessary to ' linearize ' the equations and we are not restricted to disturbances of small amplitude. However, we always have to pay for simplicity and Sutcliffe's method has the disadvantage that hypotheses are made about the nature of the motion which cannot be verified any more than can the hypotheses made in applying the barotropic model. This is still the case when the method is modified in an attempt to take into account the vertical motion. Sutcliffe's method has this in common with Rossby's that both attempt to reproduce three-dimensional motion with a two-dimensional model. The vertical structure of the disturbances is not properly taken into account so that, quite apart from other approximations, it is not surprising that the results are inexact and are approximately true only in limited sets of circumstances. However useful two-dimensional models may be it would be an advantage if we could check some of the results against accurate three-dimensional solutions. This brings us to a consideration of the kind of three-dimensional problem which has as yet been most amenable to solution. The subject is conveniently approached via the stability problem.

6. The stability of atmospheric motion

The question at issue is whether or not steady baroclinic flow represents a stable type of atmospheric motion, *i.e.* whether or not a small disturbance of the flow would result in its complete transformation. There is a general method which may be applied to attempt to solve this type of problem. We assume a small change in the flow pattern (*i.e.* a small perturbation) and on substitution in the equations of motion obtain a set of linear differential equations which determine (with the boundary conditions) how the perturbation will change with time. Usually we cannot solve the equations in complete generality but we can study certain particular solutions. Often it is possible to find solutions corresponding to disturbances whose structure does not vary with time. There are normally two possibilities. The disturbance may remain of constant amplitude (though moving), corresponding to *stable wave* solutions. (If frictional terms are included the amplitude decreases slowly with time). Alternatively the solutions may correspond to disturbances which either increase or decrease exponentially with time, corresponding to *unstable waves*. The solutions which decrease are of little interest physically. Those which increase will do so until the disturbance is no longer small, and the flow pattern is transformed. It follows that the original flow pattern is unstable if *any* unstable disturbance can be found ; it is stable only if *all* disturbances are stable. Since the behaviour of a disturbance depends on its structure we ought to examine all possible types. Moreover, apart from our interest in the stability problem for its own sake, there is the additional interest that we are led to compute three-dimensional solutions of the equations of motion. Some of these may be typical, in certain circumstances, and they provide a useful check on the assumptions made in two-dimensional model theories.

There are two types of disturbance which lead to fairly simple equations. The first type involves circulations in a vertical plane. It may conveniently be studied by the so-called *particle* method.

7. Dynamical instability – particle method

In the full treatment of atmospheric oscillations the variation of pressure must be taken into account and the equation of continuity must be satisfied. However, in the last decade an extensive literature has developed regarding a less realistic type of oscillation in which the pressure field is supposed to remain constant, and in which the motion of one individual particle of the fluid is regarded as independent of all others without consideration of continuity requirements. The results are applicable to oscillations restricted to a vertical plane. The published work on these lines was recently reviewed by one of us (Sawyer 1949a) and references to the original papers will be found there. The criterion for instability of a straight steady current on a rotating earth was first given in full by Solberg (1936) and may be presented in the form

$$\left(\frac{\partial u}{\partial y}\right)_\theta > l \qquad . \quad . \quad . \quad . \quad . \qquad (14)$$

where u is the velocity component along the stream and the y axis is perpendicular to the stream. Thus, for instability the horizontal wind shear across the current when taken along an isentropic surface (θ = constant) must be anticyclonic and exceed the Coriolis parameter l.

If a current is unstable when the pressure is held constant it is likely also to be unstable if this restriction is removed. The criterion (14) is a sufficient condition for instability. It is a necessary condition only if we restrict consideration to instability of this limited type.

If we look for regions of the atmosphere where the condition (14) is satisfied, we find they are very restricted in vertical and horizontal extent. Condition Eq. (14) requires a very strong anticyclonic wind shear, and conclusive evidence for it can only be obtained from a very close network of wind soundings. However, there is evidence that such wind shears are approached on the right hand side of jet streams, and also in frontal regions if the shear is measured along surfaces of constant wet-bulb potential temperature as is appropriate in regions of saturation. The criterion may therefore impose a limit on the shear on the south side of a jet stream, and may also have some importance in the motion in frontal cloud masses, but there is no evidence that it has any direct importance in determining the development of depressions and anticyclones, for which we should expect the criterion to be satisfied over wide areas.

Recently attempts have been made to establish a criterion similar to (14) and applicable to systems of curved flow. However, unless an initial steady flow can be prescribed (and very few simple steady flows are possible) it is difficult to decide what constitutes instability. Recent work by Petterssen (1950) has indicated that the presence of anticyclonic curvature and anticyclonic shear renders a wind system more sensitive to disturbances from such causes as pressure changes. Perkins (1950) has also given a general treatment of particle motion in an arbitrary pressure field.

8. Baroclinic disturbances

As we have seen steady baroclinic flow is stable from the point of view of circulations in a vertical plane unless the absolute vorticity in the isentropic surfaces is negative, so that from *this* point of view and for motion on a large-scale stability is the rule rather than the exception. We cannot however infer that the flow is stable because we have not yet examined other types of disturbance. There is in fact a second type of disturbance which involves alternating regions of northerly and southerly motion, much more like the initial stages of the large-scale disturbances seen on synoptic charts. These disturbances are usually referred to as *baroclinic waves*. The stability criterion for such disturbances was discovered by Charney (1947). He found that the disturbances with a wavelength smaller than a certain critical value can be unstable, but that disturbances with wavelengths greater than the critical value are stable. On substituting average values for the parameters his criterion indicates stability for disturbances *about* the size of, or larger than, the average long wave. Judging from subsequent papers Charney appears to have drawn the conclusion that large-scale features are essentially stable phenomena and therefore suitable objects for forecasting by direct computation ; we shall refer to this question again later. He appears to be less interested in the unstable disturbances. Eady (1949) has adopted a different point of view. According to him the developing unstable waves may represent the initial stages of the mechanism by which solar radiation is transformed (via potential energy) into kinetic energy of the atmosphere. His object therefore has been to analyse their structure and growth rate for comparison with observation. He found that in most circumstances unstable waves could (and generally would) occur, but that in given circumstances one particular disturbance should grow faster than any other and dominate the final pattern. All these unstable disturbances are shorter than the critical wavelength found by Charney. The first analysis in fact assumed that the disturbances were so small that the latitude variation of the Coriolis factor (dl/dy) could be neglected. A second approximation including this factor then showed that the disturbances besides growing should be retarded in the same manner as Rossby's barotropic waves but to a somewhat smaller extent. It is interesting to note that this modification in the wave-retardation factor and also the existence of instability (mathematically expressible as an ' imaginary ' wave velocity) both arise from terms analogous to Sutcliffe's development term. Since Sutcliffe (1951) has subsequently shown that his model implies instability in the circumstances in which Eady's analysis applies it is possible to use Eady's three-dimensional model as a check in a particular case on the accuracy with which the two-dimensional models of Sutcliffe and Rossby respectively reflect the actual three-dimensional motion. It appears that while Rossby's model exaggerates the wave retardation (and thus shows quite incorrectly the growth rate) Sutcliffe's model exaggerates the instability (partly as a result of ignoring vertical motion). Thus both Sutcliffe and Rossby get *one* feature of the motion very approximately correct. This strongly suggests that a two-dimensional model combining the features studied by Rossby and Sutcliffe might be of very considerable value, at any rate pending the development of a three-dimensional theory capable of dealing with more general types of motion. One difficulty in three-dimensional theory, even to the extent to which it has as yet been applied, is that it is still necessary to make approximations in order to obtain tractable equations. It is worth noting that Charney and Eady and more recent contributors (Berson (1949), Fjortoft (1950)) make different approximations and approach the problem from rather different angles yet the results are all in broad agreement. This not only increases confidence in the broad correctness of their conclusions but suggests that the essential features are of a sufficiently simple nature to make further generalizations possible.

One may reasonably enquire what practical use may be made of the theoretical results outlined above. So far as Rossby's and Sutcliffe's models are concerned the answer is obvious. Both these models can be and have been applied to actual synoptic problems and, if the results are only of limited value, this is due as much to computational difficulties (or, rather, difficulties in computing the answer fast enough to be of practical forecasting value) and lack of sufficiently accurate or sufficiently detailed data, as to limitations inherent in the crudeness of the model. Before discussing how the computational difficulties may be overcome we may pause to consider the significance of the three-dimensional solutions. From one point of view they represent refinements of the two dimensional solutions and if they could be generalized should give more accurate solutions of similar problems. On the other hand the existence of instability as, apparently, a normal feature of atmospheric motion has led Eady to the conclusion that questions of long-term forecasting can *in principle* only be answered statistically, *i.e.* in terms of the average or most probable behaviour of disturbances. His attempt (Eady 1950) to apply this principle to account for climatic features (such as the general circulation) is outside the scope of this article. Nevertheless it is relevant to point out that, if, in order to solve the equations relating to quasi-stable large-scale motion, we need to include stresses and heat-transfer terms associated with slightly smaller-scale 'turbulent' motions – which cannot be forecast in detail because of the unstable nature of the motion – then we may find it necessary to investigate the laws relating the *transfer properties* of the smaller-scale motion *to the large-scale motion itself*. It is for this reason that Eady emphasizes the importance of investigating the development and transfer properties of disturbances from a theoretical as well as an empirical point of view. It cannot be said that this line of approach has as yet got very far, but neither has the analogous problem in relation to the much smaller-scale turbulence familiar near the earth's surface (though probably existing at all levels) and in aerodynamics. The whole subject of turbulent transfer stands wide open as a challenge to the imagination of the theorist and to the ingenuity of the experimenter and synoptician.

9. Computing the weather forecast

Reverting to more straightforward problems let us consider how a two-dimensional model may be used most effectively. If we abandon the arbitrary simplifications which lead to simple explicit solutions such as Rossby barotropic waves we are faced with the solution of one or more partial differential equations.

If we are using a barotropic model we find that the solution of one equation is sufficient to determine how the field of motion (or, rather, our model of it) will change with time. By repeating this integration of the equations of motion at suitable small intervals of time it is possible to forecast the behaviour of our model, which we hope will bear some resemblance to the actual behaviour of the atmosphere. But before discussing the details of this process it may be of interest to note some of the difficulties involved in computing the weather by *any* method (quite apart from those relating to turbulent transfer, as mentioned above). It will be recalled that just as Babbage invented the automatic digital computing machine long before it was possible to construct one, so L. F. Richardson devised a method of computing the weather forecast before the means of carrying out the computations in a reasonable time existed. The actual construction of an ultra-rapid electronic computer stimulated new interest in Richardson's ideas but it has been found desirable to modify his methods. It is possible to write down a system of simultaneous partial differential equations whose solution (assuming that all the necessary data are available) should determine subsequent motion. We cannot hope to solve these equations explicitly but we may hope to solve them by numerical methods (*e.g.* successive approximation, relaxation etc.). If we suppose that the terms relating to radiation and surface friction can be estimated, we are faced with a problem which, though complicated, is *formally* less formidable than might have been imagined. Nevertheless there are several practical difficulties. In the first place the vertical velocity is not directly measurable nor can it be inferred from the pressure field (only by the *accurate* wind field) so that initial values must usually be estimated. In the second place the arithmetic involved in solving complicated three-dimensional problems is a formidable problem even for electronic machines. These difficulties do not rule out the possibility of computing the weather using quite accurate three-dimensional models but they strongly suggest that we should develop the technique gradually using quite crude models at first to obtain the necessary practical experience. Charney has suggested that we should investigate a hierarchy of models and he has commenced with what is undoubtedly the simplest, the barotropic model. The goodness or otherwise of his solutions is really a matter of secondary importance.

It will suffice to consider only the latest and most ambitious of Charney's contributions (Charney, *et al.*, 1950) to the computation of the behaviour of the barotropic model. The relevant equation is the vorticity equation (7) which may be re-written :

$$-\nabla^2\left(\frac{\partial\psi}{\partial t}\right)=\frac{dl}{dy}\frac{\partial\psi}{\partial x}-\frac{\partial\psi}{\partial y}\frac{\partial}{\partial x}(\nabla^2\psi)+\frac{\partial\psi}{\partial x}\frac{\partial}{\partial y}(\nabla^2\psi)$$

where ψ is the stream function, the velocity components being $-\partial\psi/\partial y$ and $\partial\psi/\partial x$ and $\nabla^2\psi\equiv\partial^2\psi/\partial x^2+\partial^2\psi/\partial y^2$, the vorticity. At $t=0$ the terms on the right-hand side are known so that $\partial\psi/\partial t$ may be determined by solving a Poisson equation with the appropriate boundary conditions. Strictly speaking the boundary conditions can be applied only if we know ψ over the whole of the earth's surface but a close enough approximation is possible if we consider only a limited region. Poisson's equation is not a difficult one to solve numerically and methods differ only in the speed with which solutions are obtained in given cases. The method used by Charney is suitable only for rapid automatic computation using a relatively fine network of observations. (In this as in most numerical methods the differentials in the formula are replaced by small finite differences). Having solved the equation for $\partial\psi/\partial t$ as a function of x and y we can extrapolate linearly over a small interval of time δt, *i.e.* we replace $\psi_0\,(t = 0)$ on the right-hand side by $\psi_0 + \frac{\partial\psi}{\partial t}\,\delta t$. The process of integrating the equation is repeated to give $\partial\psi/\partial t$ at $t = \delta t$, then $\psi\,(t = 2\delta t)$ is computed and so on. In order that errors due to the linear approximation in extrapolating shall not mount up disastrously δt must not be taken too large. (In practice Charney found $\delta t = 3$ hr possible). As is only to be expected, the results of computations of behaviour 24 hr ahead leave much to be desired when compared with the behaviour of the atmosphere. Nevertheless the *feasibility* of computing with a two-dimensional model has been established ; it remains to be seen whether or not more realistic models can be made whose behaviour is sufficiently close to that of the atmosphere to justify computations on a routine basis. In this connection it is of interest to note that the speed of Charney's computations was sufficient barely to keep up with the weather. But it is almost certain that more rapid and efficient methods can be developed.

10. Experiments and dynamical meteorology

Up to now we have confined our attention to theoretical and synoptic work since most recent work (as most of the work in the past) has been along these lines. Although advances in meteorology as a whole have been due as much to experiment as to observation, the kind of dynamical meteorology which deals with motion on a large scale seems an unsuitable field for experiment. We are unable to introduce sufficient new energy into the atmosphere to cause any appreciable modification of the large-scale motion, so that the only possibility for experimental work is the study of the behaviour of models. Unfortunately it is rather difficult to construct anything like a realistic working model of the atmosphere. There is of course some difficulty in scaling down but the real stumbling block is the force of gravity. It is not difficult to reproduce fluid motion on a spherical surface, nor is it difficult to reproduce rotating baroclinic motion, but it is difficult to reproduce the two together, especially with the correct boundary conditions. What we need are body forces of some sort depending on density (not necessarily true gravity) and acting towards a point, the centre of our model sphere. But true gravity acting on our model is so strong that this is practically impossible to realize. If therefore model experiments are to be of value it must be because we realize and allow for the differences between our model and the atmosphere. We may be able to infer *indirectly* some useful facts about baroclinic motion on a sphere *outside* the range in which it is possible to infer facts from synoptic observations. Many meteorologists will be familiar with such experiments as those of Exner in which baroclinic fluid motion was reproduced in a rotating dish by heating at the circumference and cooling with a block of ice at the centre. The result was the genesis of disturbances much like atmospheric cyclones and anticyclones. Experimental work of this sort has tended to be spasmodic, possibly because theory has been insufficiently developed to draw the appropriate conclusions regarding atmospheric motion and to suggest what experiments should be attempted next. A healthy sign is that the most recent work, that of Fultz (1949), appears to have been suggested by theoretical work on the barotropic model. However naïve or unsatisfactory we may consider the theoretical background and however inaccurately the model may reproduce atmospheric motion, there is no doubt that the results are interesting and they may be more relevant to dynamical meteorology than appears at first sight. Fultz rotated a fluid between two glass hemispheres (concave upwards) at the same time heating the fluid from below (*i.e.* at the 'pole'), generating a kind of convective turbulent motion. He observed that an easterly zonal current (*i.e.* one rotating more slowly than the hemispheres) was generated at the top (above about 30°S.) and a westerly zonal current at the bottom. It would be rather strange if this reproduction of atmospheric zonal motion were purely coincidental, though we cannot make *direct* inferences because gravity acts in a different manner in Fultz's model from what it does in the atmosphere. Nevertheless these experimental results may be a pointer in regard to the theory of the atmospheric zonal currents since several meteorologists (Rossby, Starr, Eady *etc.*) have given reasons for abandoning the classical theory of meridional circulations in favour of quasi-horizontal large-scale turbulent transfer of angular momentum or vorticity. Their alternative theories would give *similar* results whether the fluid were heated at the equator (as in the atmosphere) or at the pole (as in Fultz's experiments).

11. Topographical effects

In this highly selective account of recent work no mention has been made of work relating to relatively small-scale phenomena. There is however one small-scale feature which may appropriately be mentioned here. This is the modification of flow caused by topography. We have already noted the close parallelism between the large-scale quasi-horizontal lee waves set off by barriers of continental size and the short vertical gravity lee-waves set off by ranges of hills. Lyra (1943) and Queney (1948, 1950) have made important contributions to this subject but the most realistic account is perhaps that of Scorer (1949). As has been indicated the lee waves are stationary solutions of the equations of motion which satisfy the boundary conditions in the absence of the obstruction and these component disturbances are there greatly magnified (theoretically an infinitesimal range of wavelengths is infinitely magnified) when the obstruction exists. The frequency of the wave depends partly on the static stability (the vertical gradient of potential temperature) and partly on the rate of wind shear. The wave velocity is that of the main current at some height depending on the distribution of wind and static stability. It may be noted that the existence of this phenomenon is not dependent on the rather peculiar wind distributions assumed (for mathematical convenience) by Scorer. Lee waves are of considerable interest to glider pilots. For dynamical meteorologists they have the additional interest of being associated with a considerable *form drag* which must be added to the ordinary skin friction of the earth's surface. It may be noted that *any* kind of permanent stationary interference in the flow pattern may be associated with lee waves. The change of friction at a coast line, for example, is capable of generating them.

12. The future of dynamical meteorology

The present time is a difficult one at which to look forward to the future development of dynamical meteorology because activity on the subject is now much greater than ever before. The most hopeful facts for the future of the subject are that, now for the first time, the dynamical meteorologist has adequate observations which permit him to know the three-dimensional structure of atmospheric systems and which permit him to test his computations. It is a healthy sign that research workers are endeavouring to study and predict the development of actual synoptic situations, albeit by idealising them by restrictive models. This is the work which, if successful, could bring the greatest aid to forecasting, but a very great deal of development and research will be necessary before we can hope to compute tomorrow's weather map. Meanwhile the less ambitious dynamical studies of special models of atmospheric flow will no doubt aid the empirical interpretation and extrapolation of the synoptic chart which is the basis of forecasting – perhaps, more important, they will help to advance meteorology as a science.

Acknowledgment

One of us (J. S. S.) acknowledges the permission of the Director, Meteorological Office, to contribute to this paper.

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