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THE FUNCTION OF CYCLONES IN THE GENERAL CIRCULATION

by HAROLD JEFFREYS.

The customary attitude of meteorologists to cyclones seems to be to regard them as disturbances superposed in some way upon a general circulation of the atmosphere, the latter being tacitly supposed capable of independent existence. I wish here to bring forward an alternative view, namely that cyclones are an essential part of the general circulation, which could not exist without them.

Let the north polar distance of any point be θ, and the east longitude φ. The velocities of the air to the south and the east respectively are u, v. The perpendicular distance of a point from the axis is ϖ, and the mean radius of the earth a. The density of the air is ρ, and the pressure p, the earth's speed of rotation Ω, and the height above sea level z. We can prove three propositions, one relating to the variation of the geostrophic wind with height, one to the existence of steady motion in the absence of friction, and one to the conservation of angular momentum for the air north of any given parallel of latitude.

1. For the first purpose we may neglect effects due to the curvature of the earth and, taking an origin of Cartesian coordinates at a given point on the surface, denote the southward and eastward distances by x, y, and the component of angular velocity about the vertical by ω. Then

$$\omega = \Omega \cos \theta. \tag{1}$$

The geostrophic wind is then given by

$$u = -\frac{1}{2\,\omega\,\rho}\frac{dp}{dy}\,;\; v = \frac{1}{2\,\omega\,\rho}\frac{dp}{dx} \tag{2}$$

For a given gas

$$p = R'\rho\, T \tag{3}$$

where R' is a constant and T the absolute temperature. If the composition is not uniform, T is the virtual temperature as defined in a previous paper. Then

$$2\,\omega\frac{dv}{dz}=\frac{d}{dz}\left(\frac{R'T}{p}\frac{dp}{dx}\right)=-\frac{R'T}{p^2}\frac{dp}{dz}\frac{dp}{dx}+\frac{R'}{p}\frac{dT}{dz}\frac{dp}{dx}+\frac{R'T}{p}\frac{d^2p}{dz\,dx}\quad(4)$$

But

$$\frac{dp}{dz}=-g\rho=-\frac{gp}{R'T}\quad(5)$$

Hence

$$2\,\omega\frac{dv}{dz}=\frac{g}{p}\frac{dp}{dx}+\frac{R'}{p}\frac{dT}{dz}\frac{dp}{dx}-\frac{gT}{p}\frac{d}{dx}\left(\frac{p}{T}\right)=\frac{R'}{p}\frac{dT}{dz}\frac{dp}{dx}+\frac{g}{T}\frac{dT}{dx}\quad(6)$$

But we may eliminate R' by using (5); then

$$2\,\omega\frac{dv}{dz}=\frac{g}{T}\left(\frac{dT}{dx}\frac{dp}{dz}-\frac{dT}{dz}\frac{dp}{dx}\right)\Big/\frac{dp}{dz}\quad(7)$$

Similarly

$$2\,\omega\frac{du}{dz}=-\frac{g}{T}\left(\frac{dT}{dy}\frac{dp}{dz}-\frac{dT}{dz}\frac{dp}{dy}\right)\Big/\frac{dp}{dz}\quad(8)$$

The expressions (7) and (8) vanish if T is a function of p; hence if the surfaces of equal pressure and density coincide the geostrophic wind does not vary with height.

This theorem was proved in a different way by W. H. DINES (1).

For our purpose, since the temperature normally increases to the south, the first term in (7) corresponds to an increase with height of the component of the wind to the east (westerly in usual language). To estimate the amount of the effect, we notice that $\Omega=7.3\times10^{-5}$/sec. so that in middle latitudes ω is about 5×10^{-5}/sec. T varies by about 30° from the Arctic to the equator, or about 1 part in 10. Hence the contribution to dv/dz from the first term in (7) is about 10^{-3}/sec, or 1 metre per sec. per kilometre. The sign of dp/dx, on the other hand, is variable, so that the contribution from the second term is sometimes positive and sometimes negative. Its ratio to the first term may be estimated as follows. We have near the surface $dp/dz=-1.3$ dyne/cm³; $dT/dz=-6^{\circ}\times10^{-5}$/cm.; and if a horizontal variation of tem-

(1) Nature, 99, 1917, 24. Cf, also, SHAW, J. Scott. Meteor. Soc. 16, 1913, 171; Manual of Meteorology, 4, 196.

perature of 30° is associated with one of 20 mb. ($= 2 \times 10^4$ dynes/cm²) in pressure,

$$\frac{dT}{dz}\frac{dp}{dx}\Big/\frac{dT}{dx}\frac{dp}{dz} = \frac{6\times 10^{-5}\times 2\times 10^4}{30 \times 1.3} = \frac{1}{30} \qquad (9)$$

roughly. It appears therefore that with ordinary limits of variation the second term is a small fraction of the first. Thus the variation of the westerly wind with height depends on the variation of temperature with latitude, and is substantially independent of anything else.

The geostrophic wind is a good approximation to the actual wind except in disturbances of small horizontal extent, such as tropical cyclones and land and sea breezes, and in the lowest kilometre of the atmosphere, where the wind is appreciably influenced by the friction of the ground. Subject to these restrictions the above rules may be applied to the actual wind.

2. With any given steady distribution of temperature the number of possible steady motions of the atmosphere is infinite in the absence of friction. For the geostrophic wind is given by

$$2\,\Omega\, u \cos\theta = -\frac{1}{\rho}\frac{dp}{a\sin\theta\, d\varphi}\,;\; 2\,\Omega\, v\cos\theta = \frac{1}{\rho}\frac{dp}{a d\theta} \qquad (10)$$

The rate of loss of mass due to horizontal flow in an element of volume specified by $d\theta\, d\varphi\, dz$ is

$$d\theta\, d\varphi\, dz\left\{\frac{d}{d\theta}(\rho u\, a\, \sin\theta) + \frac{d}{d\varphi}(\rho va)\right\} = \frac{1}{2\,\Omega}\, d\theta\, d\varphi\, dz\left\{-\frac{d}{d\theta}\left(\sec\theta\frac{dp}{d\varphi}\right)\right.$$

$$\left. + \frac{d}{d\varphi}\left(\sec\theta\frac{dp}{d\theta}\right)\right\} = -\frac{1}{2\,\Omega}\sec\theta\tan\theta\, d\theta\, d\varphi\, dz\,\frac{dp}{d\varphi} \qquad (11)$$

Now if the pressure at height o is p_s, we have from (3) and (5)

$$\frac{p}{p_s} = \exp\left(-\int_0^z \frac{g\,dz}{R'T}\right) \qquad (12)$$

and the rate of increase of mass per unit area is dp_s/gdt. Hence, integrating (11) with regard to z from o to infinity, we get

$$\frac{dp_s}{g\,dt} = -\frac{1}{a^2\sin\theta}\left[\frac{d}{d\theta}(\rho u\, a\sin\theta) + \frac{d}{d\varphi}(\rho va)\right] = \frac{1}{2\Omega a^2\cos^2\theta\, d}\frac{dP}{d\varphi} \quad (13)$$

where

$$P = \int_0^\infty p dz \tag{14}$$

It follows that if P is independent of the longitude, p_s does not vary with time; and therefore with T independent of the time it follows from (12) that the pressure at every height is independent of the time. It is not necessary that p should be independent of φ; for though there would otherwise be inflow at some levels and outflow at others, this would be accommodated by vertical motion within the column, leaving the pressure at ground level unaltered. Thus P may be any function of latitude only. With any such function p_s may be found from (12) and (14), and therefore a steady *régime* exists. Thus with any steady distribution of temperature there are an infinite number of possible steady motions of the atmosphere. This is the meteorological analogue of a theorem relating to free steady motions in the ocean given by RAYLEIGH and extended by LAMB (1).

It appears further that if P increases to the east there will be accumulation of mass, and the pressure at all levels will increase with the time; thus anomalies in P will move to the west (2). If the pressure anomalies remain of the same order of magnitude up to height h, the rate of travel will be

$$\frac{gh \sin \theta}{2 \Omega a \cos^2 \theta} \tag{15}$$

With $h = 8$ km. and $\theta = 60^o$, this is about 3×10^4 cm/sec. This is much faster than the ordinary rate of travel of a cyclone, besides being in the wrong direction in temperate latitudes. Ordinary cyclones must therefore apparently be phenomena involving such temperature variation that the variation of P is small compared with that of hp_s, or else the travel arising from the linear terms must be balanced by a second order effect; it was found previously that the second order terms gave a rate of travel in the right direction but embarrassingly large (3)

(1) Hydrodynamics, art. 333.

(2) This was noticed by L. F. RICHARDSON, *Weather Prediction by a Numerical Process* 1922, 9. I obtained it about the same time (Q. J. R. Met. Soc. 48, 1922, 36), but there seems to be a numérical error in the estimated rate of travel.

(3) Phil. Mag. 37, 1919, 1-8.

3. The main result of the last section is that we cannot hope to determine the general circulation uniquely from frictionless theory alone, since with any assigned distribution of temperature the number of possible general circulations is infinite. The actual general circulation must therefore apparently either have been given in the beginning and persisted ever since, or else be essentially controlled by friction. Now it is easily seen that friction must have a very important influence on the general circulation. With an easterly or westerly wind the magnitude of the frictional stress is $k\rho v_s^2$, where k is a numerical constant equal to about 0.002. With a velocity of 400 cm/sec this is about 0.4 dyne/cm^2. Now the momentum of a column of air 1 cm^2 in cross section extending the whole height of the atmosphere and moving with velocity 400 cm/sec is 4×10^5 gm.cm./sec. It would therefore be annihilated by friction in 10^6 seconds, or about 12 days. Allowance for the variation of wind with height makes no difference; for this variation is determined by temperature distribution alone, and therefore any loss of momentum due to friction is redistributed through the column so as to leave the vertical variation of wind unaltered. The time needed to abolish the *surface* wind remains as estimated. This effect is so drastic that we may say at once that only such motions are possible as are permitted by friction. It further becomes clear that either there must be no surface wind anywhere, or the surface winds must be eastward in some places and westward in others; for in the latter case the friction will tend to accelerate rotation of the atmosphere in some places and retard it in others, and with a suitable distribution the two effects may balance. But we must go into greater detail; for not only must the angular momentum of the whole atmosphere be steady, but also that between any two given parallels must be steady. Now if the winds between any given pair of parallels are prevailingly in one direction, the friction will be in the opposite direction, and the circulation will be rapidly brought to rest unless new angular momentum is brought in. Pressure within the belt cannot do this, and ordinary viscosity is ineffective. The only possible source of the new angular momentum is interchange of air with the regions to the north and south, air that has lost its angular momentum travelling out of the zone and being replaced by other air with a new supply.

The theory of turbulence in the atmosphere implies a drift across the isobars in the lowest kilometre, due to friction, and with a symmetrical distribution of pressure this would imply a transfer of angular momentum in the right sense. But it proves to be quantitatively inadequate. Let us consider the southward transfer across a given parallel. The rate of transfer of mass is

$$Q = \int_0^{2\pi} \int_0^{\infty} \rho\, (\varpi + z \sin \theta)\, u d\varphi\, dz = 0 \qquad (16)$$

since the total mass must not vary secularly. The angular momentum per unit mass is $(\varpi + z \sin \theta)\, 2\, (\Omega + \dot{\varphi})$. Hence the rate of transfer of angular momentum to the south is

$$M = \int_0^{2\pi} \int_0^{\infty} \rho\, (\varpi + z \sin \theta)^3\, (\Omega + \dot{\varphi})\, u d\varphi\, dz \qquad (17)$$

Now ϖ and Ω are independent of φ and z; so if we multiply (16) by $\varpi^2\, \Omega$ and subtract from (17) we find

$$M =$$

$$\int_0^{2\pi} \int_0^{\infty} \rho u \left[\left\{ (\varpi + z \sin \theta)^2 - \varpi^2 \right\} \Omega + (\varpi + z \sin \theta)^2 \dot{\varphi} \right] (\varpi + z \sin \theta)\, d\varphi dz.$$

$$\int_0^{2\pi} \int_0^{\infty} \rho u\, \varpi^2\, (2\, \Omega\, z \sin \theta + v)\, d\varphi dz \qquad (18)$$

nearly. Now in any case ρu is negligible when z is more than about 20 km. If then we take $z = 2 \times 10^6$ cm., $\Omega = 7.3 \times 10^{-5}$/sec., $\Omega z = 150$ cm/sec. But the velocity of prevailing winds is of order 400 cm/sec or more; hence with this hypothesis, which is unduly favourable to the first term in the integrand, the contribution from v is three times that from Ωz (1). Hence we have approximately

$$M = a^2 \sin^2 \theta \int_0^{2\pi} \int_0^{\infty} \rho\, u v\, d\varphi\, dz. \qquad (19)$$

This evidently has an intimate relation to REYNOLDS's eddy shear stress $u'v'$. The present proof is simplified from an earlier one (2); REYNOLDS's discussion cannot of course be applied directly because we have to consider the amount of the complication introduced by the earth's rotation.

(1) This might not hold for a planet with a deep atmosphere, such as, probably, Saturn.

(2) Q. J. R. Met. Soc. 52, 1926, 96.

Now consider the rate of change of angular momentum due to friction. We use suffix s to indicate surface values; the frictional stress is $-\,k\,\rho_s\,(u_s^2+v_s^2)^{\frac{1}{2}}\,v_s$, and the moment of the friction about the polar axis is

$$-\,k\int_0^{\theta}\int_0^{2\pi}\rho_s\,(u_s^2+v_s^2)^{\frac{1}{2}}\,v_s\,a^3\sin^2\theta\,d\theta\,d\varphi \tag{20}$$

The condition for the constancy of angular momentum north of colatitude θ is therefore

$$\sin^2\theta\int_0^{2\pi}\int_0^{\infty}\rho\,u\,v\,d\varphi\,dz=-ka\int_0^{2\pi}\int_0^{\theta}\rho_s\,(v_s^2+u_s^2)^{\frac{1}{2}}\,v_s\sin^2\theta\,d\varphi\,d\theta. \tag{21}$$

This should hold as an approximation for all values of θ.

4. Now consider the possibility that the pressure is independent of φ. Then the geostrophic wind is eastward or westward, and the integration with regard to φ merely gives a factor 2π on each side. The southward component u is zero except in the lowest kilometre, where it has the opposite sign from v in the northern hemisphere, reaching about $\frac{1}{4}$ v on the ground. Hence (dropping the factor 2π) we see that the left side is about $-\frac{1}{8}\rho_s\,v_s^2\sin^2\theta$ (1 km). The right side is of order $-\frac{1}{3}\,ka\,\rho_s\,v_s^2\sin^3\theta$. A balance can therefore be obtained if $\sin\theta$ is about $\frac{3}{8}\,\frac{1\text{ km}}{ka}=\frac{1}{32}$. Thus a balance of the angular momentum equation due to inflow through friction cannot hold for more than about 2° from the pole. Further, even if this held in such a restricted region, we should still need to apply the principle to the northern hemisphere as a whole. We may suppose similarity of thermal conditions between the northern and southern hemispheres, so that there is no correlation between u and v on the equator, and the left side vanishes there. Hence the contributions to the right of (21) from the ranges $0<\theta<2^\circ$ and $2^\circ<\theta<90^\circ$ must be equal and opposite. In view of the factor $\sin^2\theta$ in the integrand this would imply such a concentration of velocity near the poles as seems entirely unplausible, if not impossible. It seems therefore that we are entitled to say that when friction is taken into account it is impossible to reconcile

a steady general circulation with the equation of angular momentum.

5. It appears therefore that the general circulation must either involve no surface winds and therefore no friction, or else be unsymmetrical. We can see that the latter condition makes it possible to satisfy (21). Consider a moderate latitude, say 45°, and suppose that north of it the order of magnitude of the velocities remains the same. Then the right of (21) is of order $-ka \times 2\pi \times 0{\cdot}14 \rho_S v^2_S$. The left is $\pi \int_0^\infty \rho\, uvdz$, a mean with regard to longitude being understood. Hence

$$\int_0^\infty \rho\, uvdz = -\ .0006\ a\ \rho_S\ v_S^2 = -\ \rho_S\, v_S^2\ (4\ \text{km}).$$

It appears therefore that (21) can be satisfied if u is equal and opposite to v over a height of rather more than 4 km. (since we must allow for the reduction of density with height). To maintain an eastward circulation in high latitudes the winds at the southern boundary must be mostly north-east and south west. This agrees with observation; *mutatis mutandis*, it holds also in the southern hemisphere. But the correlation between u and v can hardly be complete, and if we allow for this it seems that the currents must persist through about the whole height of the troposphere. The same must therefore apply to the associated pressure gradients. This agrees with the work of W. H. Dines, who shows that the pressure on the ground is (coefficient = + 0.68 to 0.88) positively correlated with that at heights up to 9 km (2). On the other hand the constitution of the cyclone as fundamentally a combination of south-west and north-east winds seems to agree better with the model of Bjerknes than with the symmetrical model of earlier writers. I think, however, that the apparent difference is one of emphasis and method of approach rather than one of fact. On the other hand the suggestions that the cyclones represent either an instability of the general circulation, or oscillations about a steady general circulation, appear to be incorrect. These sugges-

(1) It might appear that (21) could be satisfied without departure from symmetry if there were S W winds on the ground, with NE winds above them. But if this held all round a parallel of latitude we should h ve the component of velocity to the south always diminishing with height, and therefore, by (8), the temperature always decreasing to the east. Hence the temperature could not be continuous.

(2) Collected papers, 247.

tions agree in assuming that a steady symmetrical general circulation is possible; whereas it has been shown here that friction renders such a circulation impossible *ab initio*. Cyclones are then only the irregularities inevitable in any circulation when skin friction over the earth's surface is taken into account. Their fundamental function is to transport momentum, just as in the smaller eddies usually considered in turbulence; and DEFANT's notion of a horizontal Austausch including the cyclones exhibits their true nature.

6. The foregoing inferences proceed from the assumption that there is a systematic circulation involving surface winds. The result is therefore a necessary condition for the existence of such a circulation. The main problem of the circulation, however, may be stated as follows : given the supply of heat, the mean of which with regard to time is a function of latitude only, why should there be any surface winds at all? If the atmosphere was originally isothermal and at rest, and was heated up in such a way that there was no horizontal outflow, there would be no change of the mass above any point, and none of surface pressure; and therefore when the atmosphere was left to itself there would be no geostrophic wind on the ground and therefore, apparently, no surface wind. At other levels there would of course be a geostrophic wind, since the temperature is a function of latitude. But if there is no surface wind the right side of (21) vanishes, and the left side will vanish if $u = 0$, and the equation is satisfied. Our problem is, then, why is this not the correct solution? The question is serious because it appears at first sight as if such a solution would be stable. Suppose we have a symmetrical circulation; friction makes the air drift towards the regions of low pressure and therefore tends to fill them up, so that a state with no surface inequalities of pressure apparently tends to be restored. We may however proced by comparing this motion with one that is certainly stable : that where the whole atmosphere rotates like a rigid body with the earth and the temperature is a function of height only (more strictly, of the geopotential). Here there are no winds at all at any height. The motion to be considered differs from this in two respects. The temperature depends on the latitude; and the velocity depends on the height. The former condition leads to disturbances of the thermal state by

radiation and heat conduction, but there seems to be no reason to suppose that these would be unsymmetrical. The latter on the other hand implies a general shear in the atmosphere, of an amount far greater than is required to initiate turbulence according to REYNOLDS's results. Thus momentum would tend to be transferred downwards, producing surface winds, until these reached such strength that the friction balanced the downward transfer to the surface layer. But then the friction on the ground would imply a secular change of angular momentum in the column as a whole, unless this was restored by interchange of air between different latitudes. It seems therefore that the state with no surface winds would lead to a state involving cyclones. The nature of the resulting movement is not easy to see in this way, but can be seen from other considerations. The initial state considered is not one of rotation like a rigid body, and therefore, by a well-known theorem, involves dissipation of energy though viscosity. Now it contains only one source of energy, namely the variation of density over the level surfaces. On account of the higher temperature near the equator, combined with the assumed uniformity of pressure at the surface, the pressure at any height other than zero is greatest at the equator, and the mean height of the air is greatest there. Hence there is a store of potential energy corresponding to this variation of the mean height. In establishing any other motion this potential energy must be drawn upon (1). Hence the pressure near the equator must be diminished and that in higher latitudes increased. Thus the final state will involve a belt of low pressure around the equator, with a system of easterly tradewinds. But the winds elsewhere must be westerly, so that there must be westerly circulations in middle latitudes, corresponding to the usual prevailing winds there. It seems probable, however, that these would not extend to the poles. To maintain such circulations near the poles NE and SW winds would have to extend in spirals all the way to the poles, a state of affairs probably difficult to realize dynamically. It seems more plausible that a southeast wind should turn round before it reaches

(1) Dr L. F. RICHARDSON, in a letter to me, puts this point in another way. " Why are there surface winds? We see the answer most clearly by studying the conditions under which an occasional surface calm is moved by the wind in an upper layer. This commonly happens about 3 hours after sunrise." Clearly there is downward transport of momentum even when there is no surface wind initially.

the pole. If it does so, the shift would most frequently occur through the air passing around the north of a cyclone, its direction changing from SW through SE to NE, and the velocity would diminish through friction in the process. Since the transmission of angular momentum depends on the product ρuv, and the average value of ρu is zero, we shall expect the SE winds to make a somewhat larger contribution to the angular momentum near the north pole than the NE ones do. Thus the mean of ρuv would be positive and weak anticyclonic circulations would be expected near the poles. Such an effect would be complicated by the effect of unsymmetrical distribution of land near the pole, notably by Greenland, and would probably be difficult to disentangle from the observational material; on the other hand it may possibly be identified with the phenomenon of " polar air ".

7. The foregoing development is mainly qualitative or relating to orders of magnitude rather than to accurate values. This appears to be inevitable at present. The ordinary phenomena of turbulence are still only somewhat vaguely understood, and here we have the additional complications of rotation and a spherical boundary. It does however indicate some considerations that seem fundamentally important in the theory of atmospheric motions, and that will have to appear in some form or other in any more precise theory.

DISCUSSION

Dr. F. Y. W. Whipple : one of the first problems to be studied in dynamical meteorology was the cause of the Trade Winds. It is certain that the solution given by Hadley and still reproduced in texbooks of geography is inadequate. — Hadley's scheme would logically entail high pressure over the poles, low pressure over the equator and winds with easterly components in all latitudes. The real problem of the Trade Winds is to explain the belts of high pressure north and south of the tropics.

I believe that Dr. Jeffreys's analysis supports the suggestion which I put forward some years ago that the westerly surface winds of the middle latitudes are produced through turbulence by the westerly upper winds and that the westerly surface winds produce by geostrophic force the high pressure belts. The upper west winds are themselves explained by the distribution of temperature and the trade winds are explained by the fall of pressure towards the equator.

The difficulty in the chain of cause and effect is to see how the drag of the upper winds is exerted to produce the westerly surface winds. The mechanism which Dr. JEFFREYS has explained must serve the purpose but I must confess I am not yet able to picture just how this happens. The recognition that the departures from the average flow are as important as the average flow is, however, a notable advance in the subject.

Sir G. T. WALKER says that after Dr. WHIPPLE'S challenge he must express his view that if the earth's atmosphere has assumed a steady state there must be an east wind round the equator, for the air may be regarded as having in a state of steady motion all the angular momentum about the earth's axis that it will acquire; and hence the resultant of the couples about the earth axis of the friction between the air and the earth will be zero. Now near the equator the wind will be easterly, for there is no place from which a particle could arrive with a more rapid angular velocity than that of the earth at the equator. Then since the total couple is zero the friction at a distance from the equator must be in the negative direction : hence the winds there must be westerly. These results hold whether the equator is hotter or colder than the other parts of the earth.

In his reply Dr. H. JEFFREYS agrees with Dr. WHIPPLE'S remarks. He thinks that Sir G. T. WALKER, overlooks the possibility that surface air at the equator may be air that has descended from some height; then the reduced distance from the axis gives an increased absolute velocity and therefore a wind from the west. This would not arise in the actual case of high temperatures near the equator; but if the temperature distribution was reversed Dr. JEFFREYS thinks that it would, and that the final result would be a reversal of the directions of the prevailing winds everywhere.