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The maintenance of the mean zonal surface currents

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1. The meaning of transfer

The observed persistence of mean zonal surface currents (one belt of westerlies and two belts of easterlies in each hemisphere) implies a correlation, as a function of latitude, between westerly and southerly components of velocity in accordance with Jeffreys' formula. On each vertical surface through a line of latitude there must be a stress, part of which may be due to meridional (toroidal) circulation, part to eddy stresses of Reynolds type associated with quasi-horizontal motion in weather systems. The problem examined is which, if either, of these ought, for hydrodynamical and thermodynamical reasons, to produce correlations of the observed magnitude and distribution.

If we understand by ' transfer ' an increase on one side of a surface and an equal decrease on the other side we may speak of stresses corresponding to angular momentum transfer. This does not imply that individual particles conserve their angular momentum – usually this is transferred through pressure forces to adjacent particles – and it will be convenient to distinguish this kind of transfer as ' *virtual*.' For although the transfer by pressure forces has no effect whatever on the *instantaneous* transfer (the forces are ' internal ' and do not appear in the formula for the stress) it does prevent our applying simple mixing ideas except as tentative (and sometimes erroneous) hypotheses. In the case of a conservative quantity such as mass (or, approximately, with certain restrictions, entropy or absolute humidity) we may reasonably suppose that any irregular turbulent motion will ' mix up ' this quantity at the same time as it mixes the particles carrying it, so that this transfer will be ' down the gradient ' of the mean distribution. Even in this case for a rigorous deduction it would be necessary to assume that the distribution of the conservative quantity involved does not influence the motion – and this is often not true. When the quantity involved is not conservative (except *in toto*) the assumption that turbulent motion produces ' mixing ' and virtual transfer ' down the gradient ' is more dubious. For example the process, involving only horizontal motion, which is *described* in terms of virtual transfer of angular momentum may equally well be described in terms of virtual transfer of absolute vorticity. (The concept of virtual transfer is applicable to any quantity conserved *in toto*.) If we apply the ' mixing ' hypothesis in the two cases we obtain mutually contradictory results. It is interesting to note that while both results differ from what is observed, the assumption of ' mixing ' of absolute vorticity (which is more nearly conservative) yields a distribution even more obviously at variance with actuality than that obtained by assuming ' mixing ' of angular momentum ! If we note further that the quantity whose virtual transfer we are concerned with is, far from being independent of the motion, actually one aspect of it, we are led to abandon mixing hypotheses and study the processes which cause the velocity correlations.

2. Meridional circulations

It is noteworthy that the theory of virtual transfer of angular momentum by steady meridional, Hadley-type, circulations must also be based on a theory of turbulent transfer. For in the absence of any internal stresses we must have, for this special type of motion, conservation of angular momentum. The circulation could continue only if there were enormous velocities at the earth's surface. Were these to be reduced by surface friction the motion in any ' cell ' extending from the equator to, say, 30° latitude or more could be maintained only if the thermal wind in some regions much exceeded that observed. In fact any moderate temperature gradient corresponds to a system of zonal winds which, from the point of view of perturbations of Hadley type, is in stable equilibrium. If however there exist ' eddies ' (possibly of cumulus convection size) which virtually transfer angular momentum in the *vertical* the equilibrium will be destroyed and a more or less steady meridional circulation *must* take place to maintain the balance according to the ' thermal wind ' equation. If the virtual transfer is down-gradient and the zonal wind increases with height the circulation must be direct (poleward motion at high levels). To maintain the motion it is necessary to maintain the temperature gradient against the tendency of the motion itself to even it out, but the difference in insolation which produces the thermal gradient will do this automatically. Hence the crucial point in a theory of meridional circulations is whether convective eddies ought, for hydrodynamical reasons, to cause virtual transfer of angular momentum, and, if so, how much. If we provisionally accept the transfer as a fact for which there is independent evidence – for example the increase in surface friction when the lapse-rate is unstable – then we must infer that Hadley-type circulations actually exist. It remains however to determine the role they play in virtual transfer of angular momentum between latitudes.

We may immediately infer that any meridional circulation, whether direct or reverse (which would be the case only if the thermal gradient were temporarily or permanently reversed at any latitude), virtually transfers angular momentum from lower to higher latitudes and never in the opposite direction. This is directly related to the fact that the thermally-driven ' engine ' has to do work against friction. Hence meridional circulations could not possibly account for the existence of polar easterlies. Could they account for the main transfer which is directed polewards across latitude 30° ? This question can be answered only if we know the magnitude of the (presumably) ' convective ' stresses. If we assume these are proportional both to the thermal wind and to the amount of convection we must infer that the circulation is almost entirely confined to the region between the equator and a latitude (say 35°) slightly poleward of the maximum thermal wind. This is because for a given stress the circulation (measured by $\partial \overline{V}_y/\partial z$) needed to restore the vertical gradient of zonal velocity is inversely proportional to the N-S gradient of angular momentum, i.e. roughly to the Coriolis parameter. We may also study the maximum internal stress (across, say, the 500-mb surface) at the centre of the cell in relation to the surface stresses to the north and south of this latitude which destroy the northward-transferred angular momentum. We find that for cells extending into high latitudes the internal stresses would have to be much greater than the surface ones. More precisely if δM is the difference in angular momentum (due to thermal wind) between top and bottom of the cell and ΔM is the difference in angular momentum between its northerly and southerly limits, then the ratio of maximum internal to maximum surface stress must be of the order of $\Delta M/\delta M$, which for a hypothetical cell extending from equator to 60° latitude would be something like 10 : 1. For a cell extending only to 30° latitude the ratio would be nearer 3 : 1. Now if the main transfer of angular momentum across latitude 30° were due to meridional circulations the cell would have to extend to near 60° and we should expect to find some other evidence of the enormous internal stresses with which it would have to be associated. The general inference from this discussion would seem to be that Hadley-type circulations must exist but are probably almost entirely confined to low latitudes. They will have the effect of pushing the trade-wind maximum nearer to the equator than it would otherwise have been, but they will not explain the main momentum transfer responsible for the trades (and westerlies) as a whole, nor can they possibly explain the polar easterlies.

3. Gross-turbulence

It remains to consider the virtual transfer of angular momentum by 'gross-turbulence' associated with weather systems. We have seen that 'mixing' ideas do not even give a unique answer let alone the correct one. The fact is that this gross-turbulence is, in the last analysis, thermally driven and the virtual transfer of angular momentum a secondary, almost accidental, characteristic. No completely satisfactory account of the process is yet possible and we shall have to make hypotheses. As a first approach let us suppose (in the manner of H. L. Kuo) that the disturbances are thermally (baroclinically) generated in the first instance but, since this process itself reduces the mean baroclinity, that subsequent motion is quasi-barotropic, and governed by conservation of absolute vorticity. Now in two-dimensional (frictionless) barotropic motion there is conservation of the total kinetic energy. It is also easy to show that there is conservation of total 'activity' where this quantity is defined as $\frac{1}{2}\rho\zeta^2$, ζ being the *relative* vorticity. Hence we may regard this quantity as virtually transferable. Baroclinic processes generate 'activity' which is then redistributed and finally destroyed by surface friction. Now it may be shown that :

$$\frac{\partial}{\partial t}\int\!\!\int\!\!\int\left[\tfrac{1}{2}\rho\zeta^2 + \frac{2\omega}{R^2}\,r\rho\,\overline{V}_x\right]dx\,dy\,dz = \left[\int\!\!\int V_y\,\tfrac{1}{2}\rho\zeta^2\,dx\,dz\right]_{y_1}^{y_2}$$

where the coordinates are the usual ones, V_y is the southerly component and V_x the westerly (zonal) component of the motion. The second integral is over the whole vertical plane through a line of latitude. ω is the angular velocity of the earth, R its radius and r the axial distance. The interest of this equation is that it illustrates a close relation between the total angular momentum (measured by $r\rho\,\overline{V}_x$) and the total 'activity' in a zone. If the term on the right hand side of the equation were zero – it is important to note that this does *not* measure the virtual transfer of 'activity,' in spite of appearances – then a decrease in 'activity' in any zone would imply an increase in angular momentum and vice-versa. Now continued genesis of 'activity' implies continued regeneration of baroclinity which occurs most powerfully in regions where the gradient of the radiation balance is a maximum, i.e. in middle latitudes. Hence we consider what would happen if 'activity' were generated in middle latitudes and subsequently diffused by irregular motion. If we could ignore the right-hand side of the equation the inference would be that $\overline{V}_x$ must continually increase in middle latitudes (where $\frac{1}{2}\rho\zeta^2$ is decreased by the diffusion) and increase elsewhere. Ultimately this process would be balanced by surface friction and we should have a belt of westerlies in middle latitudes surrounded by *two* belts of easterlies. The picture is a simple one and the result is qualitatively correct but it must be admitted that logically there is no improvement on the 'mixing' hypotheses rejected earlier. Its chief weakness is that we have to assume, quite arbitrarily, a zero (or, at any rate, small) correlation between V_y and ζ^2.

Since in the assumed model absolute vorticity is accurately conserved it is interesting to see what would result from a 'mixing' hypothesis. Absolute vorticity increases towards the poles. Hence simple 'mixing' would increase vorticity in low latitudes and decrease it in high latitudes – precisely the opposite of what is observed regarding the main transfer across about latitude 45°. But the 'mixing' hypothesis suffers from an even more serious defect. Suppose the transfer to take place across latitude l_2 between two zones, the poleward zone being bounded by latitudes l_2 and l_3 and the equatorward zone by latitudes l_1 and l_2 and for simplicity suppose there is initially no motion relative to the earth. Then after 'mixing' the vorticity in the poleward zone is anticyclonic and that in the equatorward zone cyclonic. But no transfer has taken place across l_3 so the vorticity in the polar cap is unaltered. It follows immediately that the zonal motion along l_3 is unchanged. Similarly it is unchanged along l_1. Then the change in vorticity in the zones between l_1 and l_3 requires the development of an easterly wind along l_2. Hence the mean angular momentum of the combined zone between l_1 and l_3 must decrease. Since there is no change in the motion outside the zones this result is inconsistent with the conservation of total angular momentum. Thus not only does the ' mixing ' hypothesis give the wrong answer – it is not even consistent with the equations of motion ! It is true we could get round the difficulty by assuming a velocity discontinuity to develop at l_1 and l_3 but this is just the opposite of what ' mixing ' is supposed to do. While the result might conceivably be correct it could not be deduced by consistent application of this ' mixing ' hypothesis. This simple example illustrates how tentative are ' mixing ' hypothesis even when applied to conservative quantities. We cannot entirely be surprised at this result for the process we imagine, with the production of organized zonal flow out of ' random ' weather systems, has the character of ' unmixing ' rather than of ' mixing.' Is there any inconsistency, for example with the second law of thermodynamics, in the supposition that such a ' sorting out ' process can occur ? Clearly not, for we have not considered the whole system. The genesis of weather systems takes place with loss of ' organization ' – in the long run their continued production involves consumption of ' negative entropy.' Hence we have only to assume that, as a result of the hydrodynamical constraints, part (actually a small part) of the ' organization ' supplied by radiation goes into the regeneration of zonal motions.

4. Hydrodynamical approach

Instead of making the ' activity-diffusion ' hypothesis (which is illuminating but not compulsive) we may attempt a deductive approach based on the laws of hydrodynamics. Kuo has made such an attempt based on the second-order changes which must occur in fields of motion initially supposedly ' unbiased.' It would take too long to discuss here the merits and defects of his approach. It must suffice to remark that his very valuable contribution (in spite of, apparently, some errors in detail) throws considerable light on the nature of the process. However we cannot yet say that a purely deductive treatment of our problem exists. Apart from other gaps in the argument we have that between the 'baroclinic' and 'barotropic' stages, the virtual transfer of angular momentum being associated only with the latter. One way of remedying this defect is to study the growth of disturbances and examine whether or not this growth is associated with genesis of zonal motion. Simple disturbances of the type studied by Charney and Eady do not give transfer of angular momentum. For all of these, representing breakdown of a region in which the thermal wind is zonal, the trough-lines are along meridians and there is no (geostrophic) correlation between V_x and V_y. However no solutions have yet been published which take into account *both* the variation of the Coriolis parameter with latitude *and* variation of Richardson number such that there is a zone in which this number is a minimum. As a substitute the writer has worked out (unpublished) the disturbances of maximum growth rate of such a system using a ' two-layer ' model. The result is that the ridge- and trough-lines are now bow-shaped, the ends of the bow sweeping backwards towards the west just as are so many ' long-wave ' troughs on 500 mb synoptic charts. The correlations of V_x and V_y are now such as to generate westerlies in the region where the Richardson number is a minimum with two belts of easterlies on either side. The agreement with synoptic experience is encouraging and there is little doubt that when eventually the fully three-dimensional solutions are computed they will show similiar characteristics. We must infer that at least part (perhaps most) of the virtual transfer of angular momentum takes place during the active growth stage of large-scale disturbances and the genesis of zonal motions may be a more direct process than Kuo's analysis suggests. However it must not be supposed that this explanation is *quite* different from that of Kuo. The main difference is that we have here studied as one process what Kuo studies in two parts which are merely logically, not physically, distinct.

The main conclusion would seem to be that although no completely satisfactory deductive treatment exists there are good reasons for supposing that gross-turbulence *ought* to ' transfer ' angular momentum between latitudes according to the pattern actually observed, with westerlies developing near the climatological minimum Richardson number and easterlies on either side. A more detailed analysis will probably indicate that the equatorward easterlies must be stronger than the polar easterlies. Taken together with our discussion of the nature and location of the meridional circulations and the observed pattern of $V_x V_y$ correlations the conclusion would seem inescapable that the virtual transfer of angular momentum between latitudes is due almost entirely to ' gross-turbulence ' associated with weather systems except in low latitudes where meridional circulations are probably dominant.