

REPLY

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A careful reading of Clarke's comments shows that he does not contradict my conclusions, (Karelsky, 1957).

Clarke states: "The parameter β can clearly only be zero at the poles where U and the problem are undefined. It is difficult to excuse confusion on this point".

This statement is indeed in strict agreement with the well known fact $\cos(\pm \frac{\pi}{2}) = 0$ provided $\beta = \frac{2\omega \cos \varphi}{r}$. Unfortunately this fact is independent of another fact quoted by me, i.e. that $\frac{d \sin \varphi}{dt} = 0$ in the whole XOY plane of a Cartesian co-ordinate system fixed by origin to a point 'O' of the rotating earth at a constant latitude with the XOY plane tangent to the earth at 'O'. $\beta = \frac{2\omega \cos \varphi}{r}$ can not be obtained by differentiation in respect of y of the hydrodynamical equations corresponding to this system and does not exist in this system. Clarke's statement is therefore irrelevant.

It may be suggested (although Clarke did not mention this) that Rossby did not use the Cartesian co-ordinate system above or did not use any Cartesian system. I found these suggestions not tenable mathematically.

The expression "empirical formula" has been used by me as a mathematical term, accordingly to definitions of "theoretical" (analytically derived) and "empirical" (not analytically derived) formulae given in textbooks of mathematics.

Clarke's results of comparison of Rossby's and Haurwitz' formulae are in good agreement with Haurwitz' conclusion quoted by me. Yoshitake (1956), using his more general solution of the problem in a spherical co-ordinate system, is less optimistic. He mentions, for example: "Generally speaking, the speed of propagation of long waves isn't determined by such a simple formula as Rossby's formula or Charney-Eliassen's formula. It is not possible even to say that the relation between the speed of

propagation and the angular wave number is monotonic."

It is interesting to note that the Rossby formula was intended for "long waves" (three to five waves around the globe) but it is clearly shown by Clarke's results that the formula gives values of the speed of waves for these wave lengths which are unsatisfactory when compared with those obtained by Haurwitz. This is also pointed out in Yoshitake's conclusions.

Clarke's comment and Yoshitake's solution are not relevant to the question of the correctness of Rossby's mathematical treatment of the problem.

D'Alambert (1717-1783) has said: "If it happens that a question which we wish to examine is too complicated to permit all elements to enter into the analytical relation which we wish to set up, we separate the more inconvenient elements, we substitute for them other elements less troublesome but also less real, and then we are surprised to arrive, notwithstanding our painful labour, at a result contradicted by nature."

Sir Napier Shaw writes in his Manual of Meteorology (Vol. I): "There is still room for reflection upon a saying attributed to Rafael Aben Ezra in Charles Kingsley's HYPATIA, "No wonder that his theory fits the universe, when he has first clipped the universe to fit his theory"."

In regard to the points raised by Radok, I submit the following comment.

Applying operator $\nabla \times$ to the vector equation of motion, assuming barotropy and nondivergence, introducing $\vec{Q} = \nabla \times \vec{V}$ (\vec{V} is the wind vector) and using the symbol of the total derivative $\frac{d}{dt}$ we obtain the vorticity equation

$$\frac{d\vec{Q}}{dt} - (\vec{Q} + 2\vec{\omega}) \cdot \nabla \vec{V} = 0 \quad (1)$$

(in the Eulerian system of independent variables).

Here $\vec{\omega}$ is the vector of the angular velocity of the earth's rotation constant in space and time.

This equation (1) is valid for any kind of co-ordinate system.

If in a fixed Cartesian system rotating with the earth

$$\begin{aligned}\vec{\Phi} &= i\xi + j\eta + k\xi \\ \vec{V} &= iu + jv + kw \\ \vec{\omega} &= i\omega_x + j\omega_y + k\omega_z\end{aligned}$$

where i j k are unit vectors of the system (by definition - constant in this system), we will have, assuming $w = 0$, the scalar equation for k component of (1).

$$\frac{d\xi}{dt} = 0 \quad (2)$$

i.e. the well known Helmholtz's result.

Now this result contradicts characteristics of the natural flow on the spherical rotating earth because the earth surface cannot be considered as a plane both geometrically and physically (in relation to the gravity potential, Coriolis acceleration and metric accelerations).

It means that any attempt to represent the true vorticity equation for a large scale motion in a Cartesian system (or on a plane) is not tenable, a curvilinear system must be used and components of all vectors involved as functions of co-ordinates of this curvilinear system should be introduced.

Radok's attempt to justify incorrectness of the mathematical treatment of the vorticity equation by introducing $dy = ad\phi$ is no more than an attempt to use the cylindrical system only for the part of the second member of (1) ($2\omega \cdot \nabla \vec{V}$) without transformation into this system of the vertical components of $\frac{d\vec{\Phi}}{dt}$, and of $\vec{\Phi} \cdot \nabla \vec{V}$.

My conclusion is therefore, that treatment of problems of dynamical meteorology in a Cartesian system with the XOY plane tangent to the earth's surface at a fixed point is permissible only when $2\omega \sin \phi$ can be considered as a constant, i.e. in the immediate vicinity of the origin (according to Dynamic Meteorology by Godske and others over an area with radius not exceeding 300-350 miles around the origin).

In this case any constant C independent of x, y, z can be added to ζ and we will have $\frac{d(\zeta + C)}{dt} = 0$; in particular if $f = 2\omega \sin \varphi$ is a constant, (2) can be written $\frac{d(\zeta + f)}{dt} = 0$ under condition $\frac{df}{dt} = 0$ in this system.

If the area is large, the change of $2\omega \sin \varphi$ must be taken into account and there are no other mathematical means for the proper investigation of the problem but the introduction of a spherical or (for a limited number of problems) cylindrical system (obviously for all members of equations).

As indicated in my reply to Mr. Clarke, my article does not seek to minimize the importance of Rossby's work. It simply considers some weakness in the mathematical treatment. Rossby's work is of importance because of the stimulus it provided for research in a new field of meteorology - the direct application of theory to the forecasting problem.

References

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(S. Karelsky)
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