LETTERS TO THE EDITOR

Magnitude of horizontal divergence

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19 June 1946

The recent papers by Fleagle and by Namias and Clapp in the JOURNAL OF METEOROLOGY, March 1946, pp. 9–13 and 14–22, show appreciable differences in the order of magnitude of horizontal divergence obtained. While it is possible that these differences are due solely to the data considered, this does not appear probable; and it is in any case pertinent to note that on page 10 Fleagle disregards two separate terms, each of the order of magnitude 10^{-6} sec^{-1}, which are precisely of the order of magnitude of the isopleths of normal horizontal divergence drawn by Namias and Clapp in hemisphere maps on pp. 20 and 21.

A point of mathematical nicety is Fleagle's apparent disregard on page 10 of the convention that Cartesian coordinates are formed by the intersection at right angles of *straight* lines. If other orthogonal systems are used, e.g. intersecting curved lines of latitude and longitude, the equation for divergence takes a different appearance and the term $-(v/r)\tan\phi$ appears naturally rather than as an "additional term" which some might erroneously conclude to be a *correction*.

Fleagle's investigation rests heavily on two assumptions, namely, that the air flow is both adiabatic and isosteric, these assumptions being made in fact when using his equations (1) and (6) simultaneously. Since any two thermodynamic coordinates completely specify the state of dry air (the working substance with which Fleagle is concerned), it is necessary to conclude that the temperature and the pressure of individual air parcels are also constant throughout whatever motion the air undergoes. The individual parcels remain always on their individual points on a thermodynamic diagram, so that no provision is made in the theory for any "weather" (relative humidity remains constant unless diffusion is considered). This situation is so unreal in nature that one may question the applicability of the investigation as it stands.

Namias and Clapp also compute divergence using several very restrictive assumptions (their page 16). It is undoubtedly true that so far as observational data tell us their assumptions hold good locally for some period of time, but it seems pertinent to remark that, "No wind shear across the current throughout the path of the parcel," does not hold good over an entire map at any one time, e.g., there is shear across any diameter of the usual closed pressure system.

In spite of the restrictions set by their assumptions, it is believed by the present writer that Namias and Clapp have obtained the correct orders of magnitude of horizontal divergence on their maps. A check is provided by the numerical results obtained by me in an entirely different way.[1]

It is also interesting to note that the qualitative picture of divergence-convergence in troughs and ridges obtained by Namias and Clapp (page 19, paragraph 1) fit the quantitative results obtained in the reference cited.[1]

[1] N. R. Beers, "Divergence of the horizontal wind," *Trans. Am. Geophys. Union*, **26**, 225–236, 1945.

Reply

By ROBERT G. FLEAGLE
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22 July 1946

I am grateful to Professor Beers for providing me with the opportunity to discuss further the magnitude of divergence computed by different methods. The order of magnitude shown in my paper in the March issue of the JOURNAL OF METEOROLOGY, 10^{-5} sec^{-1}, also has been obtained by my associates at New York University and by members of the Department of Meteorology at Massachusetts Institute of Technology when computing divergence either from observed winds or with the aid of the adiabatic assumption. The fact that individual computations using both methods usually give the same sign and the same order of magnitude is evidence that 10^{-5} sec^{-1} is the correct order of magnitude for the actual divergence.

However, the divergence of the gradient wind is usually of the order of 10^{-6} sec^{-1} when computed from the horizontal equations of motion, indicating that the neglected terms, friction and acceleration, are sufficiently large to account for most of the divergence at a specific point.

Regardless of the method used in computation, the magnitudes of individual computations should not be expected to be identical with the magnitude of mean values taken over an extended area or time period. This is clear if it is recognized that the sign of divergence is by no means constant in time or space;

therefore mean values represent the algebraic average of a large number of individual values, some positive and some negative.

Professor Beers fails to distinguish between neglecting a term as small in comparison with larger terms and neglecting it in comparison with other terms of the same order of magnitude. Consider equation (3) from my paper:

$$\frac{1}{\rho}\frac{d\rho}{dt} + \frac{\partial w}{\partial z} = -\operatorname{div}\mathbf{V}.$$

The approximate magnitude of the three terms is 10^{-6}, 10^{-5}, and 10^{-5} sec^{-1}, respectively. Therefore, $(1/\rho)d\rho/dt$ may be neglected without doing violence to the approximate magnitudes of the other two terms. But in the equation

$$\frac{1}{\rho}\frac{d\rho}{dt} - \frac{w}{\rho}\frac{\partial\rho}{\partial z} = \frac{1}{\rho}\frac{\partial\rho}{\partial t} + \frac{\mathbf{V}\cdot\nabla\rho}{\rho},$$

which is equivalent to equation (1) from my paper for the conditions specified there, the approximate magnitudes of each of the four terms is 10^{-6} sec^{-1} or smaller. Obviously, $(1/\rho)d\rho/dt$ could not be neglected here without leading to serious error. It was therefore proper to neglect $(1/\rho)d\rho/dt$ in equation (3), but it would not have been proper to neglect its equivalent in equation (1).

Atlantic coast cyclogenesis

By HARVEY W. HALBERT
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6 September 1946

Having had more than a casual acquaintance with the two types of Atlantic coast cyclogenesis described by Professor J. E. Miller in the June copy of the JOURNAL [**3**, 31–44], I read his paper with a good deal of interest. Four questions connected with it have come to my mind.

1. In what percentage of the total number of cases (during the ten year period), in which a primary low was located as indicated in figure 13 and a ridge line located as in table 3, did type B cyclogenesis occur? This information would be of assistance to the forecaster. Similarly in what percentage of the total number of cases in which a quasi-stationary front lay off the Atlantic coast and an anticyclone covered the central part of the continent, did type A cyclogenesis occur?

2. Is it possible that the warm front in the case of type B cyclogenesis is often quite shallow in its southern portion and that the new low pressure centre develops where the slope of the front becomes appreciably steeper?

3. Does it seem probable that in some of the cases studied the cyclogenesis could be at least partially attributed to the occurrence of a "triple point" as suggested by Scherhag, based on his divergence theorem? Scherhag argues that strong upper divergence will occur where three air masses meet with the consequent development of a new wave or the rapid deepening of an existing low. When a cold front moves off the Atlantic coast, it frequently appears to catch up with the previous front that is quasi-stationary somewhere between the coast and Bermuda. Frequently when this appears to be happening, however, lack of data makes it difficult to definitely confirm it.

4. Is it possible that the lack of correlation between the formation of new cyclones and the zonal index is due to the complex nature of the zonal index? Referring to the yearly mean sea-level pressure chart of the northern hemisphere (see Shaw's *Manual of meteorology*, Vol. II, p. 216), we see that the zonal index is contributed to by the Aleutian low and Pacific high combination, the Icelandic low and Atlantic high combination, and the inverse combination of the Eurasian high and Indian Ocean low. I would like to see consideration given to a system of "indices" based on the configuration of the mean map, and expressed in terms of pressure profiles.

It will be noted that not only in the yearly mean map but also to a close approximation in each of the monthly mean maps, the Pacific system extends from about 90°W to 150°E, the Atlantic system from 90°W to 30°E, and the Eurasian system from 30°E to 150°E. Differential changes in the curves of mean pressure versus latitude for five-day means for these three 120° sectors should be of considerable value in anticipating modifications of surface pressure patterns.

A further study of the mean sea-level pressure maps already referred to will show that five-day mean profiles of pressure versus longitude, for each of two latitude ranges 0–40°N and 40–80°N, should also be useful in anticipating changes in the sea-level charts.

Changes in the first set of profiles will give an indication of the north-south displacement of the semi-permanent highs and lows, and the second set will give an indication of their east-west displacement.

Reply

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4 October 1946

I concur with Mr. Halbert's suggestion that the forecaster could make good use of an analysis of cases where the surface map exhibited type-A or type-B features but no new cyclone formed in the east coast region. For example, the probability of type-B cyclogenesis could be expressed as an empirical function of the positions of the primary cyclone and the wedge line. (The probabilities would obviously be somewhat less than 100 per cent.) Such an analysis would have required detailed study of 1,914 daily maps of the 70-month period, on which new cyclones did not appear, as well as the 208 maps on which they did appear, and the analysis was not carried out because our time was limited.

In answer to Mr. Halbert's second question, I would say it is probable that the warm front in the case of type-B cyclogenesis is often shallow in its southern portion. In the relatively few cases where I have had access to sufficient upper air data, the cold air wedge has appeared quite shallow, but I have not observed whether the new low pressure center develops at a point where the slope of the front becomes appreciably steeper.

On the basis of recent studies of atmospheric divergence, carried out at New York University, I doubt that Scherhag's divergence theorem would contribute much to an explanation of east coast cyclogenesis. Divergence in one layer is usually accompanied in other layers by convergence of nearly equal magnitude, and the relatively small net effect in any column gives the surface pressure change. Thus the occurrence of divergence in any layer is not necessarily accompanied by a surface pressure fall. For example, the strong frictional divergence which takes place in the lowest layer of an anticyclone is associated with rising pressure as often as with falling pressure.

The zonal index represents only one of many atmospheric properties that jointly affect cyclogenesis. Hence, it is not surprising that no simple correlation between the index and formation of new cyclones was found.