Some reservation is apparently taken to the last conclusion in a note at the end of the paper, in which it is pointed out that there exists a stable stationary phase lag between the temperature and pressure fields provided the wave number exceeds a certain critical value.

The conclusions quoted above from Thompson's paper are based on a theory in which the velocity field is independent of the north-south direction and, in addition, that the amplitudes of the disturbances are small. Due to these assumptions one would expect to find an agreement between Thompson's conclusions and the complete solution of the equations for the 2-level model, which can be solved in this particular case as demonstrated by OGURA (1957).

From Ogura's solutions it is first of all seen that there is no "growth" and no "occlusion" in the stable case. The amplitudes will have characteristic periods of fluctuation depending on the vertical windshear and the wave-number as seen from Table 1 in Ogura's paper. The phase-angle will vary monotonically in the stable case as seen from his figure 2.

In the unstable case Ogura has only illustrated one case, in which the disturbance at the upper level initially lags a quarter of a wave-length behind the disturbance at the lower level. It is found that the amplitude increases with time at both levels (fig. 3), and that the phase-angle approaches a limiting phase-angle depending on the vertical windshear and the wave-number (fig. 2). A closer inspection of the solution shows that the phase-angle *independent* of the initial state quite rapidly will approach the limiting phase-angle, which always is such that the disturbance at the upper level is lagging behind the disturbance at the lower level, or in Thompson's formulation: the temperature field is lagging behind the pressure field. With regard to the amplitudes of the disturbances it is found that they may decrease initially if the temperature field precedes the pressure fields, but that amplification sets in when the phase-angle has changed in such a way that the temperature field is lagging behind the pressure field.

The 2-level model has therefore no selective preference for disturbances in which the temperature pattern lags behind the pressure pattern in the sense that disturbances with the opposite phase lag will die out. It is rather a result of the development of the flow pattern in the unstable case that the disturbances will have the temperature field lagging behind the pressure field. Another indication that the 2-level model has no selective preference is the fact that the instability criterion for this model only depends on the wave-number, the vertical windshear, the static stability and the Coriolis parameter. It has thus no reference to the initial arrangement of the temperature field relative to the pressure field.

Concluding the comparison between Ogura's and Thompson's conclusions it is evident that the development of the flow in predictions with the 2-level model, at least in the case where the flow is independent of the north-south direction, does not approach a state of quasi-barotropy, and that Thompson's equations for "growth" and "occlusion" at best can give only the initial changes of the amplitude and phase-angle of the disturbance. Thompson's conclusions are further in disagreement with recent calculations of the conversion between potential and kinetic energy based on the 2-level model as performed by SALTZMAN (1959) and the present writer (1959), in which it is shown that the conversion at any time is from potential to kinetic energy. This result can only be explained if the temperature field systematically lags behind the pressure field.

Very truly yours,

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Reply

Dear Sir:

Several points of criticism implied (but charitably unstated) in Dr. Wiin-Nielsen's letter I find to be very well taken. I have been uneasy about these points for some time and welcome this opportunity to set them straight.

The conclusions quoted by Dr. Wiin-Nielsen, taken literally and as they stand, are correct only to the extent that they correctly relate the rate of growth to the current phase-lag; the error lay in misinterpreting my own equations and, in particular, in supposing that the stationary states of amplitude and phase-lag are *asymptotic* states, in the sense that the system inevitably tends toward those states. In fact, I could not have resolved the latter question, simply because my formulation of the problem was then incomplete.

A mathematically complete description of the behavior of the linear two-level model in question (sinusoidal perturbations of small amplitude, superposed on a horizontally uniform westerly motion, with velocity independent of latitude) is contained in the following equations:

$$\frac{dR}{dt}=\frac{\sin\alpha\lambda}{\alpha}\left[\alpha^2(U_2-U_1)(R^2-1)+\right.$$
$$\left.+\beta(R^2+1)\right] \tag{1}$$

$$\frac{d\lambda}{dt}=\frac{U_2-U_1}{2}-\frac{\cos\alpha\lambda}{4R}\left[(U_2-U_1)(R^2+1)+\right.$$
$$\left.+\frac{\beta}{\alpha^2}(R^2-1)\right] \tag{2}$$

in which R is the ratio of the amplitudes at the upper and lower levels, respectively; λ is the lag between the pressure perturbations at the two levels (positive for westward tilt); α is about the wave-number for minimum stability; U_2 and U_1 are the equilibrium values of the zonal wind at the lower and upper levels, respectively; and β is the Rossby parameter. These equations state how the changes of amplitude and phase-lag are related to the amplitude and phase-lag themselves.

To illustrate the nature of my earlier error, let us consider states in the neighborhood of the state ($R = 1$, $\lambda = 0$). According to Eqs. (1) and (2), the latter state is a stationary one, but *it is not an asymptotic state*, simply because Eqs. (1) and (2) for small departures (R' and λ') from that stationary state reduce to:

$$\frac{d^2R'}{dt^2}=-\frac{\beta^2}{\alpha^2}R'$$

$$\frac{d^2\lambda'}{dt^2}=-\frac{\beta^2}{\alpha^2}\lambda'$$

Thus, if the system lies near the state ($R=1$, $\lambda=0$), it will oscillate around that state with frequency β/α, passing back and forth between positive and negative phase-lags. Dr. Wiin-Nielsen is quite correct in stating that the system will not invariably tend toward a quasi-barotropic state; *it may, however, oscillate around a state of zero phase-lag, without regard to the vertical wind shear.* Otherwise, a more complete investigation of Eqs. (1) and (2) leads to results that are in accord with Ogura's conclusions for a case of *large* phase-lag.

Some months before receiving Dr. Wiin-Nielsen's letter, my uneasiness about these questions led me to undertake a study of the behavior of a linear baroclinic model with *continuous* vertical structure. In summary, I have derived a simultaneous system of two equations, analogous to Eqs. (1) and (2), in which the only unknowns are the vertical tilt and amplitude of the pressure perturbation (both considered as functions of time and elevation). Analysis of those equations indicates that a baroclinic model with *continuous* vertical structure *does* possess an asymptotic state—i.e., a state that is stationary in a special sense and which is stable in that the system always tends toward that state. Briefly described, this state is one in which the pressure perturbation has a definite vertical tilt toward the west, and in which amplification continues at all levels throughout the troposphere. These results will be reported in a forth-coming issue of Tellus.

In short, far from emphasizing the virtues of the 2-level model, I should stress its shortcomings. Although it may predict growth-rates that are *initially* correct, its mechanism of "occlusion" does not correctly simulate the atmosphere's. For this reason alone, we must now study baroclinic models in which there is more freedom of interaction between the motions at different elevations.

Sincerely yours,

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