NOTES AND CORRESPONDENCE

Comments on "A Theory of Cyclogenesis Forced by Diabatic Heating. Part I: A Quasi-geostrophic Approach"

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Lin (1989b, hereafter L89) presents a theoretical analysis of diabatically generated cyclogenesis with specific application to the formation of East Coast cyclones. While there is much to learn about these phenomena, I believe that L89 fails to make a meaningful contribution to our understanding. This failure stems from a variety of shortcomings in both the model formulation and application. Specific deficiencies are now delineated.

The first deficiency is the assumption that the flow conserves potential vorticity. As seen from the potential vorticity equation (1) of L89, vertical variations in the heating q will alter the potential vorticity. L89 ignores this q_z generation term, thereby assuming unrealistically that the heating is independent of height. L89 rationalizes this neglect by saying that the depth of the boundary layer heating is small compared to the deformation depth H_o. A posteriori, this relation does not agree with the parameters chosen in L89. For example, the choice $a_x = 75$ km implies a deformation depth of $fa_x/N = 0.75$ km [Lin (1989a, p. 925) makes a similar choice.] that is less than the boundary layer height of 1–1.5 km.

A priori, the neglect of q_z is unreasonable since the strength of this term varies inversely with the depth scale of the heating. L89 incorrectly states that Bannon and Mak (1984) used a similar approach in a model of diabatic frontogenesis. In fact, while we did present *mathematical* solutions for the Green's function for both surface and interior heating separately, we only presented *physical* solutions for a boundary layer heating of finite depth [see our section 4a and (4.7)] that included the q_z generation term.

Indeed Lin has recognized the importance of this term in an earlier study (Lin 1989a) for steady barotropic flow over a heat source. In the absence of friction, the solution for the nondimensional pressure in transform space is [(20) of Lin 1989a, with the Ekman number $E = 0$]

$$\hat{\pi} = \frac{\hat{h}\gamma}{ik(1-\gamma^2K^2)}[\gamma K e^{-Kz} - e^{-z/\gamma}] \qquad (1)$$

for

$$q = h(x, y)e^{-z/\gamma} \qquad (2)$$

where γ is the ratio of the heating depth to the vertical scale, H_0, of the flow and $K^2 = k^2 + l^2$. The second term in the square brackets results from the q_z generation term, and it is this term that actually dominates for small boundary layer thicknesses. In that case γ tends to zero,

$$\hat{\pi} \rightarrow \frac{\hat{h}\gamma}{ik}[-e^{-z/\gamma}], \qquad (3)$$

and the heating generates a surface low. Note that if one ignores this term as in L89, the lowest order solution predicts a surface high. If the depth of the heating is large, then $\gamma K > 1$, and the first term in (1) (due to surface heating) dominates. A surface low is also generated in this case since the denominator in (1) changes sign. The foregoing analysis clearly demonstrates that (i) the neglect of the interior generation of potential vorticity by the q_z term is inconsistent with the assumption of shallow boundary layer heating, and (ii) the q_z term can generate a surface pressure anomaly of comparable strength to the surface heating.

In his reply to this point, Lin (1990) presents an analysis for a tophat heating profile that is uniform below the height $z = H$. His result (6a) indicates that the q_z term, e^{-KH} in the square brackets, approximately balances the surface heating term, -1, for shallow heating, $KH \ll 1$. This tendency [see also (1) above] of the interior forcing to compensate for the surface forcing implies that L89 has overestimated the heating effect.

The second deficiency is the use of the heating parameterization (12) of L89 due to Stern and Malkus (1953). This parameterization is mathematically consistent with the surface heat equation (2) of L89 only for steady, barotropic flow. This leads to a physical inconsistency since a surface potential temperature anomaly implies a thermal wind shear by the model

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assumption of quasi-geostrophy. Its use in the transient baroclinic problem of L89 is equally questionable since the thermal wind (5) of the basic state is inconsistent with the imposed heating. For example, the surface thermal structure of the heating in Fig. 1 of L89 implies a shear in the meridional wind, not the zonal wind as L89 depicts.

Use of this parameterization also leads L89 to claim an analogy between flow over a mountain and a region of steady state diabatic heating. This analogy, while mathematically correct, appears physically inappropriate. From (13) an isolated negative orographic feature is equivalent to an isolated warm anomaly, but (12) implies a *dipole* heating distribution with warming on the upstream side of the thermal anomaly and cooling downstream. Thus a fluid parcel initially gains heat from the lower boundary and later returns the same amount of heat with no net heating. This process is at odds with our general ideas of air–sea interaction.

I believe that the analogy illustrated by Smith (1979, Fig. 15) compares the anticyclone generated by a mean flow over a mountain with the anticyclone of a cold-core thermal anomaly *in the absence of a mean flow and in the absence of any heating;* structurally the two are identical. The mountain anticyclone forms by vortex stretching as fluid columns ascending the mountain are squashed. A cold-dome anticyclone undergoes an analogous squashing: an initially static thermal anomaly expands horizontally and shrinks vertically as it achieves geostrophic balance.

In light of (12) one must also question the analysis of the sensible heat flux of Fig. 10b. While the results appear representative of other studies, an inspection of the relationship between the winds and the isotherms of Fig. 10a suggests that a traditional bulk aerodynamic approach was used to determine the fluxes rather than (12).

A third point is an incorrect interpretation (p. 3019 ff) of the flow development. It is misleading to describe the solution (19) in terms of baroclinic waves. Technically the wave solutions are equivalent barotropic and they do not tap the available potential energy of the basic state as suggested by L89. It is straightforward to demonstrate that the associated v and θ fields are 90° out of phase and hence

$$\int_0^{2\pi/k} (v\theta)dx = 0. \qquad (4)$$

Thus there can be no *net* energy conversion from the mean flow. It is therefore incorrect to conclude that the westward phase tilt of the trough of Fig. 1d implies such a conversion. Note that the ridge axis in that figure tilts eastward with height, thereby compensating for any local conversion by the trough. The lee trough develops as the ensemble of stable Eady edge waves (Bannon 1989) disperses downstream.

Lastly the applicability of the quasi-geostrophic model must be questioned. The surface Rossby number for the case of Fig. 1 is $\mathrm{Ro} = U(z = 0)/fa_x = 1.33$; for that of Fig. 11: $\mathrm{Ro}_x = U(z = 0)/fa_x = 1.16$ and $\mathrm{Ro}_y = V(z = 0)/fa_y = 0.16$. Thus the solutions of L89 are a posteriori inconsistent with the model formulation.

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