Reply

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In Lin (1989b; hereafter L89), we have shown that a low can be produced by a low-level diabatic heating in a quasi-geostrophic baroclinic flow with a level of wind reversal. When applied to East Coast cyclogenesis, a cyclone develops near the western boundary of the Gulf Stream. The low-level sensible heating and the existence of a wind reversal level play key roles in the coastal cyclogenesis. The purpose in L89 is to help understand the mechanism responsible for the initial formation of coastal cyclones.

In the quasi-geostrophic theory, we have used a value of 75 km for the half-width of the diabatic heating. Bannon (1990; hereafter B90) comments that the deformation depth \( f\alpha_s/N = 0.75 \text{ km} \) is less than the boundary layer height of 1–1.5 km. The deformation depth estimated in L89 is \( fL/N = 10 \text{ km} \) with \( L = 1000 \text{ km} \). Obviously, the difference comes from the horizontal scales chosen. From my point of view, it is more appropriate to use \( 2a_s \) instead of the half-width \( a_s \) for the horizontal scale of the forcing because \( a_s \) represents less than a quarter of the whole horizontal scale for a bell-shaped function. Furthermore, it may be more appropriate to use the horizontal scale of the disturbance—i.e., the cyclone in this case—instead of the forcing scale. The horizontal scale of the disturbance is about 500 to 1000 km \( (L) \) after it is well developed. The horizontal scales of the disturbance and the forcing may be roughly the same in some cases, such as a steady-state barotropic flow over a mountain, but not necessarily the same as in a baroclinic flow over a heat source.

We agree that the \( q_z \) term in Eq. (1) of L89 should be included in the calculation. It is neglected in L89 for mathematical simplicity. However, this assumption does not propose serious problems in the case of L89. In his derivation of Eq. (3), Bannon has assumed \( \gamma K < 1 \). The dimensional form of this condition may be written as \( NH_1K'/f < 1 \) (see Lin 1989a for the meaning of notations). As a rough estimate, \( NH_1K'/f \) is about 6.18 with \( N = 0.01 \text{ s}^{-1} \), \( f = 10^{-4} \text{ s}^{-1} \), \( H_1 = 1 \text{ km} \), and \( K' \approx 2\pi/a_s \approx 2\pi/100 \text{ km} = 6.28 \times 10^{-5} \text{ m}^{-1} \) for the case in L89. This will lead to the opposite signs of both terms in the brackets of Eq. (1) of B90. In other words, the first term will generate a surface low, instead of a high as claimed in B90. To elucidate this point more clearly, one may consider the following problem,

\[
\frac{\partial}{\partial x} (\pi_{xz} + \nabla h^2 \pi) = q_z
\]

(1)

\[
\pi_{xz} = q \text{ at } z = 0
\]

(2)

where

\[
q(x, y, z) = h(x, y), \quad 0 < z < H
\]

\[
= 0, \quad \text{elsewhere.}
\]

(3)

The mathematical problem of the above system is similar to that of Eq. (12) in Lin (1989a) except the heating is uniformly distributed in the layer below \( H \). The solution may be written as

\[
\hat{\pi} = \frac{\hat{h}}{ikK} [e^{-Kz} - e^{-Kz}] , \quad 0 < z < H
\]

(4a)

\[
\hat{\pi} = \frac{\hat{h}}{ikK} [\cosh KH - 1] e^{-Kz} , \quad H < z.
\]

(4b)

It can be shown that the neglect of the \( q_z \) term leads to

\[
\hat{\pi} = \frac{\hat{h}}{ikK} e^{-Kz} , \quad 0 < z.
\]

(5)

The surface pressures given by Eqs. (4) and (5) are

\[
\hat{p} = \frac{\hat{h}}{ikK} [e^{-Kz} - 1] \text{ with } q_z,
\]

(6a)

and

\[
\hat{p} = \frac{\hat{h}}{ikK} \text{ without } q_z.
\]

(6b)

Equation (6b) indicates that the heating will produce a surface low with the \( q_z \) term neglected. The inclusion of the \( q_z \) term will weaken the low, but still generate a surface low, since \( e^{-Kz} < 1 \).

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The purpose of using the heating parameterization (12) of L89 is to simplify the mathematical problem. We are aware of the fact that neglecting the local derivative may be unrealistic. Thus a diabatic heating \( q \) is directly specified in sections 4 and 5 in L89, unlike that treated in Lin (1989a). It remains to be analyzed if the surface potential temperature anomalies imply a thermal wind shear because the forcing is on the mesoscale. Otherwise, we agree that the basic state should be in thermal wind balance with the imposed heating.

Regarding the analogy between flow over a mountain and a region of steady state diabatic heating, B90 states that the process is at odds with our general ideas of air–sea interaction. I agree with B90; however, as stated above, this is because we have assumed that the diabatic heating is dominated by the horizontal temperature advection. Bannon also comments that the anticyclone of a cold-core thermal anomaly in Smith (1979 Fig. 15) is in the absence of a mean flow and in the absence of any heating. Under this situation, the air must be pushed away symmetrically by the pressure gradient force from the top of the cold dome to its surroundings and then turns anticyclonically around the cold dome due to geostrophic adjustment. Thus the formation mechanism of the high is different from that of flow over a mountain because the later is caused by the vortex shrinking. Notice that the flow over a mountain is not necessarily in geostrophic balance. The anticyclone generated by a mean flow over a cold dome as described in Eq. (12) of L89 is caused by vortex shrinking, which should be more analogous to the mountain wave case compared with the cold dome with no mean wind and no diabatic heating/cooling. In the case of L89, the cold dome may act to block the mean flow as the cold-air damming phenomena often observed to the east of the Appalachians in winter.

In his third point of comments, Bannon states that the solution (19) of L89 cannot be described as baroclinic waves. I agree with this point if one defines the baroclinic wave as a growing wave in a baroclinic flow, which eventually leads to instability. Under this definition, it is well known (Eady 1949) that there must exist a net northward heat flux. With a rigid upper lid included, the solution (19) (not shown in L89) will become unstable as also shown in a corresponding lee cyclogenesis problem (Smith 1986, Fig. 7). Similar to the Eady wave, there will exist a net northward heat flux in the unstable case. I agree that the local energy conversion by the trough in Fig. 1d of L89 may be compensated by the eastward tilt of the ridge axis. However, the restoring force for the generated waves in L89 is associated with the horizontal temperature gradient—i.e., the baroclinicity, similar to flow over a mountain ridge (Smith 1984). As long as the restoring force is associated with the horizontal temperature gradient, the generated waves may be categorized as baroclinic waves no matter whether they are stable or not.

The last comment of B90 is related to the adoption of the horizontal scales as discussed earlier in this reply. It is more appropriate to use \( 2a_x \) for the horizontal scale of the forcing in L89. I agree that the Rossby number associated with the flow may be slightly too large for the quasi-geostrophic theory to be valid. To improve the theory, we have developed a semi-geostrophic model in part II (Lin 1990) to simulate the same phenomenon. The results indicate that the ageostrophic advection does play an important role in strengthening the low. However, the basic features of the low are similar to that found in L89.

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REFERENCES


