Comments on "On the Richardson Number Dependence of Nonlinear Critical-Layer Flow over Localized Topography"

DALE R. DURRAN

Department of Atmospheric Sciences, University of Washington, Seattle, Washington

15 October 1991 and 26 May 1992

In the late 1970s, Clark and Peltier (1977, also Peltier and Clark 1979) performed a series of significant numerical experiments establishing wave breaking as an amplification mechanism for downslope windstorms. They suggested that the wave-breaking region, characterized by strong mixing and a local reversal of the cross-mountain flow, acts as a boundary that reflects upward-propagating waves back toward the mountain. Subsequent investigations have unequivocally confirmed Clark and Peltier's basic findings about the importance of wave breaking and the tendency of the breaking waves to act as an internal boundary to upward energy propagation. Many of these studies, however, have disagreed with the precise explanation of the amplification process proposed by Clark and Peltier. Clark and Peltier noted that wave overturning first occurred at an elevation of 3/4 vertical wavelengths ($3\lambda_z/4$) above the surface and suggested that further amplification was produced by resonance within the cavity between the ground and a "wave-induced critical layer" in the wave breaking region (see Peltier and Clark 1983, hereafter PC83, for a mathematical statement of this theory). In order for linear resonance to occur beneath a rigid-lid reflector, the reflector must be positioned an odd quarter-wavelength above the surface, so one might expect the amplification process to be sensitive to the height of the wave-induced critical layer. Clark and Peltier (1984, hereafter CP84) investigated this sensitivity and claimed to

> show by explicit computation of the evolution of the mountain wave field in mean flows with critical layers that only if the height of the reflecting critical layer above the topography is $3\lambda_z/4$ or some integral number of vertical wavelengths in excess of this, will the direct and reflected waves interfere constructively to support a large amplitude resonant response.

Smith (1985, hereafter S85) subsequently proposed a stratified hydraulic model of the wave amplification process that predicts that a large amplitude response can be obtained for all critical-layer heights (z_c) between $\lambda_z/4$ and $3\lambda_z/4$, provided the mountain height (h) exceeds some threshold value that increases with increasing z_c. An alternative statement of Smith's result is that for any normalized mountain height ($\hat{h} \equiv N_0 h/U_0$) between $\hat{h} = 1$ and $\hat{h} = 0$, there is some critical-layer height z_c^* for which steady-state high-drag solutions exist. If the critical level lies above z_c^*, the flow remains in a weak low-drag state. If the critical level lies below z_c^* (and above $\lambda_z/4$), the flow transitions to a high-drag state and, in accordance with the standard hydraulic model, the upstream conditions adjust to establish a steady high-drag flow in the vicinity of the mountain. According to S85, z_c^* increases from $\lambda_z/4$ to $3\lambda_z/4$ as $\hat{h}$ increases from 0 to 1; Scinocca and Peltier (1991, hereafter SP) refer to this parametric dependence as a "resonance shift." The theories proposed in PC83 and S85 predict clear differences in the behavior of flow beneath a mean-state critical layer and are easily tested by numerical experiment. Such experiments were conducted by Durran and Klemp (1987, hereafter DK) and Bacmeister and Pierrehumbert (1988, hereafter BP), who examined the dependence of numerically simulated flow beneath a mean-state critical level on h and z_c and found that their numerical results were consistent with the predictions of Smith's theory and at odds with those of Clark and Peltier.[1]

Scinocca and Peltier recently reconsidered the problem of flow beneath a mean-state critical layer. They simulated the airflow over a mountain in an atmosphere with constant Brunt–Väisälä frequency (N_0) and a cross-mountain wind speed defined as

$$U(z) = U_0 \tanh\left[\frac{z_c - z}{b}\right]. \qquad (1)$$

Corresponding author address: Dr. Dale R. Durran, Department of Atmospheric Sciences, AK-40, University of Washington, Seattle, WA 98195.

[1] Note that DK found good agreement between their numerical simulations and Smith's theory over a wider range of the $\{\hat{h}, z_c\}$ parameter space than that considered in SP. In particular, DK found agreement for $\hat{h}$ as small as 0.1 and z_c as low as $0.4\lambda_z$.

SP explored the sensitivity of the flow to b, or more precisely, its nondimensional relative

$$\hat{R}_m = b^2(N_0/U_0)^2, \quad (2)$$

which they describe as "the third and final governing parameter of the critical layer problem." As one of their principal conclusions, SP state:

> It is clear, however, that when one considers *all* of the governing parameters that define the problem, there is no evidence that would support the occurrence of any such "resonance shift." In fact, the evidence strongly suggests that the resonance characteristics of the critical-layer problem demonstrate little variation over the entire range of topographic forcing $0.5 \leqslant \hat{h} \leqslant 85$. The height of the critical level for which resonance obtains is always very close to $\hat{z}_c = 3\pi/2$ [or equivalently $z_c = 3\lambda_z/4$ since $\hat{z}_c \equiv N_0 z_c/U_0$].

I disagree with SP's idea that special dynamics (i.e., "resonance") govern the flow when $\hat{z}_c = 3\pi/2$. SP's own Figs. 2, 5, and 6, which are plots of cross-mountain drag obtained for flows with various critical-layer heights, define two basic responses: high drag and low drag. Instead of focusing on the difference between the case when $\hat{z}_c = 3\pi/2$ (which SP refer to as the "maximal high-drag state") and all the other simulations, I would urge the reader to observe the similarity between the various high-drag solutions and to consider the substantial difference between the family of high-drag solutions and the family of low-drag results. The approximately bimodal drag distribution is not accounted for by simple resonance models, but it is nicely explained by simple hydraulic theory, which predicts enhanced drag for all simulations with critical-layer heights in the range $\lambda_z/4 < z_c < z_c^*$. As an example of the shortcomings of simple resonance models consider the following discussion from CP84:

> ... when such a [low-Richardson number] critical level is located at a height $z = 3\lambda_z/4$ (or $n\lambda_z$ in excess of this) above the level of forcing, the direct and reflected waves interfere constructively and an intense resonant growth of the low-level wave field ensues.

The preceding statement appears to conflict with the results shown in SP's Figs. 2, 5, and 6, in that the wave drag is greatly enhanced for both $z_c = \lambda_z/2$ and $z_c = 3\lambda_z/4$, yet the reflected waves that develop in these two cases are 180° out of phase when they encounter the topography. How can constructive interference produce similar amplification in both of these cases? Why is the drag in the $z_c = \lambda_z/2$ case not similar to the very low drag that develops when $z_c = \lambda_z$? Once again, these questions are neatly answered by hydraulic theory via the analytic model of Smith.

My second concern is with the importance SP attach to $\hat{R}_m$ as an aid to assessing the "validity of Smith's theory." Neither of the analytic theories previously presented in PC83 and in S85, nor the more recent numerical work of Laprise and Peltier (1989), takes explicit account of variations in the upstream wind speed and stability. Since they are all independent of $\hat{R}_m$, it is difficult to see how an exploration of the $\hat{R}_m$ parameter space can be used to test these theories. To be specific, SP conducted several simulations with $\hat{R}_m = 2.25$ and placed considerable emphasis on new results from the portion of the parameter space $2.25 \leqslant \hat{R}_m \leqslant 9$, whereas the analytic theories in PC83 [see Eqs. (18) and (19)] and S85 assume the mean cross-mountain wind speed below the critical layer is constant with height. This hypothetical constant-wind profile is plotted in Fig. 1, together with three hyperbolic tangent curves, for which $\hat{z}_c = 1.5\pi$ and $\hat{R}_m = 1$, 2.25, or 9. Also plotted is the piecewise linear profile used for the $\hat{z}_c = 1.5\pi$ case in DK. Note that when $\hat{R}_m = 2.25$, and when $\hat{R}_m = 9$, the cross-mountain wind speed deviates significantly from U_0 throughout a very deep layer. Both the $\hat{R}_m = 1$ hyperbolic tangent profile, which was used in the numerical simulations of BP, and the DK profile are in better agreement with the simple mean state assumed by the analytic models. Thus, if one wishes to test the fundamental *validity* of the theoretical models in PC83 or S85, one would do well to choose $\hat{R}_m \approx 1$ (which, to reiterate, were the cases for which DK and BP demonstrated the superiority of S85).

When SP consider larger values of $\hat{R}_m$ they test the *applicability* of Smith's theory to a family of flows with one particular type of vertical structure, that is, constant stability and a hyperbolic-tangent wind profile. This is a worthwhile endeavor, but it needs to be kept in per-

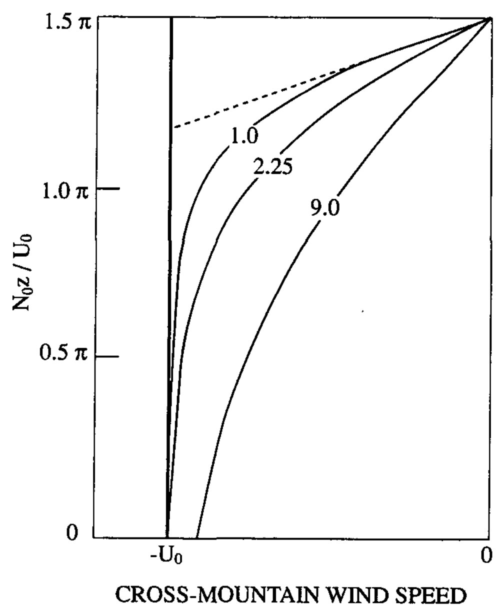

FIG. 1. Comparison of the theoretical constant wind speed profile (heavy solid curve) with hyperbolic-tangent profiles for which $\hat{z}_c = 1.5\pi$ and $\hat{R}_m = 1.0$, 2.25, and 9.0 (thin solid curves). Also shown is the piecewise linear profile from DK (dashed curve).

spective. Far from being the "the third and final governing parameter of the critical layer problem," $\hat{R}_m$ is only one of many possible parameters that might be defined to describe more complex atmospheric structures. Note in particular that $\hat{R}_m$ does not account for vertical variations in the Brunt–Väisälä frequency or for wind speed distributions that fail to follow an inverse tangent profile. One can, nevertheless, argue that if real world events tend to have wind profiles resembling the hyperbolic tangent profile with $\hat{R}_m = 2.25$ or 9, theoreticians should carefully study such cases. Does observational evidence compel us to attach special significance to the large $\hat{R}_m$ profiles? The situation in one real-world example is depicted in Fig. 2, which shows aircraft sounding data from the 15 April 1982 Bora gathered 125 km upstream of the mountain crest during the ALPEX experiment. The vertical profile of the cross-mountain wind component has been plotted as a function of height using the data appearing in Fig. 9a of Smith (1987). Three hyperbolic tangent wind profiles have been generated from (1) to fit this profile. In each case $U_0 = -6.5$ m s^{-1} and $z_c = 2.3$ km. The values of b were chosen to facilitate comparison between this high-wind bora and the simulations conducted by SP for the "optimum high-drag" configuration $\hat{z}_c = 1.5\pi$; the b's for each of the wind profiles in Fig. 1 correspond to $\hat{R}_m$ of 1, 2.25, and 9. As apparent in Fig. 2, the wind shear around the critical level in the 15 April 1982 bora was confined to a relatively thin layer. The curves $\hat{R}_m = 2.25$ and $\hat{R}_m = 9$ do not provide a particularly good fit to these data. If one wished to approximate the observed wind profile with a hyperbolic tangent, $\hat{R}_m = 1$ would be a better choice. (The piecewise linear profile of DK would also allow a reasonable fit.) Although reference to a single observation is not conclusive, the 15 April data do demonstrate that at least some important real world windstorms occur in flows with $\hat{R}_m \approx 1$ and are, therefore, well described by the hydraulic analog in general and Smith's mathematical model in particular.

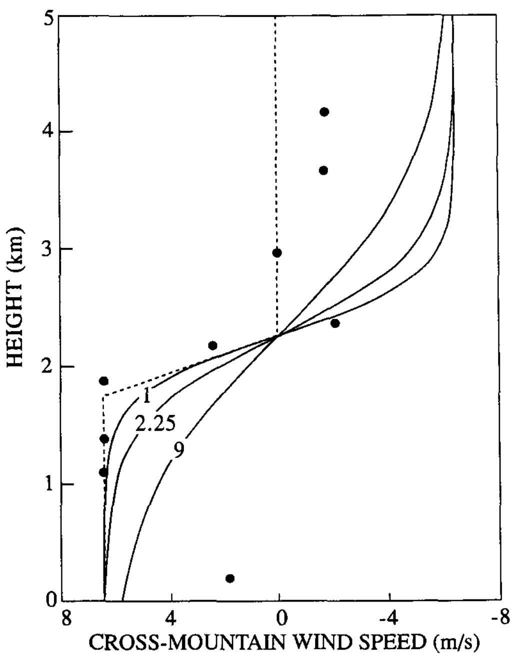

FIG. 2. Comparison of hyperbolic-tangent profiles for which $\hat{z}_c = 1.5\pi$ and $\hat{R}_m = 1.0$, 2.25, and 9.0, with ALPEX data for the 15 April 1982 bora. Also shown is the piecewise linear profile from DK (dashed curve).

Having attempted to put the significance of the parameter $\hat{R}_m$ in perspective, let us return to SP's assertion that "there is no evidence that would support the occurrence of any such 'resonance shift'." SP appear to reconcile the conflict between this statement and the results in DK and BP by dismissing those data obtained in the portion of the parameter space $\hat{R}_m \leqslant 2$ as insignificant in comparison to those obtained for large $\hat{R}_m$. As I have argued previously, however, the cases in which $\hat{R}_m \leqslant 2$ are both practically and theoretically important. Moreover, I would suggest that SP's claims about the disappearance of the resonant shift in flows with $\hat{R}_m > 2$ are overstated. SP's results concerning the resonance shift question are summarized in their Figs. 2, 5, and 6. SP do find Smith's "resonance shift" as $\hat{h}$ is reduced from 0.75 to 0.625 (compare their Figs. 2 and 5), but they do not find a shift as $\hat{h}$ is further decreased to 0.5 (Fig. 6). The lack of a shift at $\hat{h} = 0.5$ is supported by just one data point in Fig. 6b. Moreover, as shown in SP's Fig. 8, they find that decreasing $\hat{h}$ once again to 0.25 requires a further downward shift in $\hat{z}_c$ in order to produce a high-drag solution. SP suggest that their $\hat{h} = 0.25$ results are not relevant to the resonance shift investigation because the high-drag solution obtained for $\hat{z}_c = \pi$ is not completely steady. The concern about nonsteadiness should not, however, be allowed to eclipse the fundamental result that a much stronger response is obtained when $\hat{z}_c = \pi$ than when $\hat{z}_c$ has the supposedly optimal value of 1.5π. Thus, except for the single case in which $\hat{h} = 0.5$, $\hat{z}_c = 1.5\pi$, $\hat{R}_m = 2.25$, all SP's results are consistent with the idea of a resonance shift and the general behavior predicted by Smith's theory.

Scinocca and Peltier are aware of the importance of the single point $\hat{h} = 0.5$, $\hat{z}_c = 1.5\pi$, $\hat{R}_m = 2.25$ in Fig. 6b, and they attribute the high drag obtained in that simulation to their choice for $\hat{R}_m$. They investigate the sensitivity of the surface wave drag to variations in $\hat{R}_m$ in their Fig. 9, which shows a family of solutions for which $\hat{h} = 0.5$ and $\hat{z}_c = 1.5\pi$. SP find that high-drag solutions develop whenever $\hat{R}_m$ is sufficiently large. This is not surprising since if $\hat{R}_m$ is increased, $U(z)$ decreases everywhere below the critical level, thereby decreasing the local values of $N_0h/U(z)$ and increasing the opportunity for breaking waves to develop well below the mean-state critical level. In the case $\hat{R}_m = 9.0$, the decrease in wind speed with height implies a decrease in

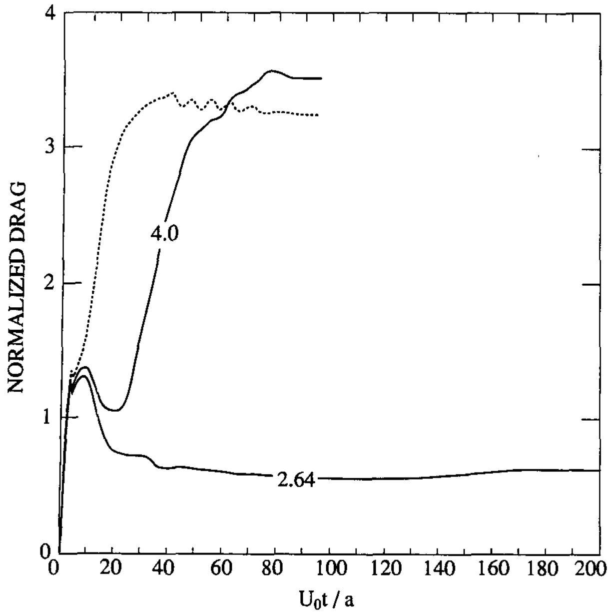

FIG. 3. Solid curves: surface wave-drag histories for simulations with $\hat{h} = 0.5$, $\hat{z}_c = 1.5\pi$ and $\hat{R}_m$ either 2.64 or 4.0. Dashed curve: surface wave-drag history for $\hat{h} = 0.5$, $\hat{z}_c = 7\pi/6$, and $\hat{R}_m = 2.64$. The wave drag has been normalized by drag over an identical mountain in the linearized problem with constant U_0 and N_0, $\pi\rho_s N_0 U_0 h^2/4$. Time is nondimensionalized as $U_0 t/a$; 192 is equivalent to the 1200-min point of SP's Fig. 9.

the local vertical wavelength (computed in the WKB sense) such that the first wave-steepening level occurs at $0.71z_c$. The waves should break at this steepening level, since $N_0 h/U(z)$ exceeds 0.85—the critical value for wave breaking in a uniform atmosphere—for $z > 0.56z_c$. Thus, when $R_m = 9.0$ (and $\hat{h} \geqslant 0.5$, $z_c = 1.5\pi$) the presence of the mean-state critical level has little direct influence on the development of the high-drag flow. A similar high-drag response would occur if the critical layer were eliminated by replacing the upper portion of the hyperbolic-tangent wind profile with a region of uniform velocity in which $U(\hat{z}) = U(\pi)$ for all $\hat{z} \geqslant \pi$. Under these circumstances, it is not surprising that Smith's theory works best if the dividing streamline is assumed to originate from the upstream elevation of the wave-steepening level, that is, from $0.71z_c$.

In addition, the dependence of the flow on $\hat{R}_m$ appears to be somewhat model dependent. Inspired by SP, I attempted to reproduce some of the simulations in their Fig. 9 and found that the onset of high-drag flow begins at larger values of $\hat{R}_m$ than those suggested in SP. Figure 3 shows drag histories for two simulations, with $\hat{h} = 0.5$, $\hat{z}_c = 1.5\pi$, and $\hat{R}_m = 2.64$ or 4.0, which were conducted with the nonhydrostatic numerical model described in Miller and Durran (1991). The numerical resolution and total domain size were identical to those in SP. Thus, the solid curves shown in Fig. 3 are an attempt to reproduce the curves labeled "2.64" and "4" in SP's Fig. 9. As evident from Fig. 3, high drag is obtained only when $\hat{R}_m = 4.0$. There is no tendency for the $\hat{R}_m = 2.64$ simulation to drift into a high-drag state at later times. The numerical model used in these experiments has been shown to consistently reproduce the basic character of solutions obtained with the Clark model (Clark 1977) used by SP. Thus, the difference in the behavior of the $\hat{R}_m = 2.64$ simulation is likely to be due to rather minor numerical effects, that is, boundary conditions, the parameterization of subgrid-scale mixing, or details of the finite differencing.[2] It is, therefore, probably not important to obtain agreement between the various models on the exact value of $\hat{R}_m$ at which the simulations begin to exhibit a high-drag response. It is more important to observe the evidence that a resonant shift is operating as predicted in S85 even when $\hat{R}_m$ is as large as 2.64. The existence of such a shift is indicated by the dashed line in Fig. 3, which shows the drag history from a simulation in which $\hat{R}_m$ and $\hat{h}$ were held at 2.64 and 0.5, respectively, but $\hat{z}_c$ was reduced from 1.5π to $7\pi/6$. Smith's model predicts that if $\hat{h} = 0.5$, the maximum $\hat{z}_c$ that should yield a high-drag response is roughly $7\pi/6$. As illustrated in Fig. 3, when $\hat{z}_c = 1.5\pi$ (solid line labeled 2.64) the disturbance is weak, but when $\hat{z}_c = 7\pi/6$ (dashed line) a high-drag flow is obtained. Thus, in contrast to SP, I find that Smith's theory may be applied using the height of the critical level to approximate the upstream height of the dividing streamline for $\hat{R}_m$ as large as 2.64. Indeed, the ability of Smith's theory to accommodate the significant variations in the cross-mountain wind profile found in the $\hat{R}_m = 2.64$ case suggests that the theory is rather robust.

In summary, I applaud SP's efforts to investigate the development of downslope winds in atmospheres with nontrivial vertical structure, but I disagree with several of their conclusions. In particular, I suggest that 1) SP's attempt to identify $\hat{R}_m$ as the "third and final governing parameter of the critical layer problem" has overemphasized its importance, 2) their numerical experiments in the parameter space $2.25 \leqslant \hat{R}_m \leqslant 9$ are not in essential conflict with the hydraulic model for downslope windstorms, and 3) the assertion that resonance is "always" obtained for critical-level heights "very close to $\hat{z}_c = 3\pi/2$" is incorrect. As one might guess, I also question SP's assertion that "the implications are not encouraging with regard to the validity of Smith's theory."

[2] One potential source of spurious numerical amplification in the SP simulations, which is absent in the present simulations, is SP's practice of performing a forward time difference every tenth leapfrog time step. Given this frequent use of forward time differencing, I would hesitate to attach a great deal of physical significance to the amplification process that appears to occur only after long simulation times in some of SP's results (e.g., the case $\hat{R}_m = 2.25$ in SP's Fig. 9).

Acknowledgments. This research was supported by NSF Grant ATM-8914852. Computational facilities were furnished by National Center for Atmospheric Research, which is sponsored by the National Science Foundation.

REFERENCES

Bacmeister, J. T., and R. T. Pierrehumbert, 1988: On high-drag states of nonlinear stratified flow over an obstacle. *J. Atmos. Sci.*, **45,** 63–80.

Clark, T. L., 1977: A small-scale model using a terrain following coordinate system. *J. Comput. Phys.*, **24,** 186–215.

——, and W. R. Peltier, 1977: On the evolution and stability of finite-amplitude mountain waves. *J. Atmos. Sci.*, **34,** 1715–1730.

——, and ——, 1984: Critical level reflection and the resonant growth of nonlinear mountain waves. *J. Atmos. Sci.*, **41,** 3122–3134.

Durran, D. R., and J. B. Klemp, 1987: Another look at downslope winds. Part II: Nonlinear amplification beneath wave-overturning layers. *J. Atmos. Sci.*, **44,** 3402–3412.

Laprise, R., and W. R. Peltier, 1989: The linear stability of nonlinear mountain waves: implications for the understanding of downslope windstorms. *J. Atmos. Sci.*, **46,** 545–564.

Miller, P. P., and D. R. Durran, 1991: On the sensitivity of downslope windstorms to the asymmetry of the mountain profile. *J. Atmos. Sci.*, **48,** 1457–1473.

Peltier, W. R., and T. L. Clark, 1979: The evolution and stability of finite-amplitude mountain waves. Part II: Surface wave drag and severe downslope windstorms. *J. Atmos. Sci.*, **36,** 1498–1529.

——, and ——, 1983: Nonlinear mountain waves in two and three spatial dimensions. *Quart. J. Roy. Meteor. Soc.*, **109,** 527–548.

Scinocca, J. F., and W. R. Peltier, 1991: On the Richardson number dependence of nonlinear critical-layer flow over localized topography. *J. Atmos. Sci.*, **48,** 1560–1572.

Smith, R. B., 1985: On severe downslope winds. *J. Atmos. Sci.*, **42,** 2597–2603.

——, 1987: Aerial observations of the Yugoslavian bora. *J. Atmos. Sci.*, **44,** 269–297.