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Comments on "The Evolution and Stability of Finite-Amplitude Mountain Waves. Part II: Surface Wave Drag and Severe Downslope Windstorms"

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1. Introduction

In their recent paper Peltier and Clark (1979, henceforth referred to as PC) showed results of numerical simulations of waves over two-dimensional mountain ranges. In contrast to the hydrostatic treatments presented by Klemp and Lilly (1975, 1978, hereafter referred to as KL5, KL8, respectively), their anelastic model accounts for the shortwavelength modes which are always present to some degree in real mountain waves and often dominate. The simulation of the extreme amplitude wave associated with the downslope windstorm in Colorado on 11 January 1972 shows high fidelity to the observational data and may be close to the optimal simulation that can be expected. In addition, their results point out a possible sensitivity of the atmosphere to resonant wave modes which may be stronger than could be anticipated from linear theory. Because of the complexity of the model results, however, their interpretation is of comparable difficulty with that of observational data.

We here offer some alternative interpretations to those presented by Peltier and Clark, making particular reference to aspects of our previous related results which we believe were either overlooked or misunderstood. First, we defend the use of linear theory and/or isentropic coordinates within their applicable limits, which we believe are wider than indicated by PC. Linear theory is defended largely on the basis of its success in predicting the occurrence of strong winds over a significant data sample, including 11 January, indicating that it contains the most important physical effects. Our defense of the use of isentropic coordinate models rests largely on estimates of the relative importance of shearing and convective instability in generating turbulence, since the latter cannot be developed by these models. We also point out that nonlinear amplification, seemingly similar in its net effect to that produced by PC's "self-induced critical layers," was obtained from an isentropic coordinate model, discussed at some length in KL8, and that this model as well as linear theory predicted similar surface wind amplitudes to those obtained by PC for 11 January. Finally, we argue that PC's proposed amplification mechanism is unconvincing, but suggest a possible test of its validity.

2. Validity of linear theory and isentropic coordinates

As an aid to interpretation of their model results PC carried out some linear steady-state wave theory calculations, using both idealized and real sounding data. Based on such calculations using the unmodified Grand Junction sounding for 11 January they decided that linear theory was inadequate as a guide to the observed wave response, yielding a calculated momentum flux an order of magnitude too low. Their nonlinear nonhydrostatic simulations, on the other hand, provided excellent agreement with the observed flow. A careful examination of our results would show them in general agreement with PC's, although our hydrostatic isentropic coordinate model did not produce quite as good a simulation of the 11 January case as did theirs. We do not, however, agree with their conclusions regarding the usefulness of linear theory or isentropic coordinate simulations, for the following reasons.

Typically the most serious problem with linear theory in application to flow over large mountains,

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as pointed out by Vergeiner (1971), is the dilemma over what mean state wind speed profile to use at levels below and near that of the mountain top. The low velocity and low potential temperature flow often present at the upstream foot of a large mountain range is usually blocked or diverted around the mountain, an effect reproducible by properly designed simulation models but not by linear theory. On 11 January 1972 the potential temperature in Boulder and Denver was 8°C warmer than that at Grand Junction, 300 km further to the west, and was about the same as the 700 mb level potential temperature upstream of the mountains. These blocking effects are evidently present in PC's results, since the lowest isentrope on Fig. 29 disappears and the upstream velocity profile of Fig. 31 is reduced from that of the initial profile at all levels. From comparison of a number of cases (KL5, Fig. 16) we found that an empirical procedure which would allow linear predictions to agree fairly well with those of nonlinear simulations and with the stronger atmospheric events was to simply eliminate from consideration the flow below the lower half of the mountain range and to correspondingly decrease the mountain height by a factor of 2. This technique also worked well on the 11 January storm, producing a much stronger response than was obtained from the linear solution using the full mountain height and the full upstream sounding. The reason was and typically is that the linear amplification process becomes effective only if the distance from the base of the sounding to the tropopause is about one-half vertical wavelength, and we assume that this phase shift only applies to the portion of the atmosphere actually flowing over the mountain. In other words, the top of a low-level blocked layer can be effectively considered the mountain surface in estimating linear response.

We do not deny that nonlinearity alone can increase wave amplitude, an effect especially dramatic for asymmetric mountains, as demonstrated by Lilly and Klemp (1979). However, the high Colorado mountains and their shape are present every day and strong flow over them occurs very frequently in the winter, but we believe we have shown (in KL5) that extreme downslope windstorms are mainly confined to times when the linear theory (with the observed soundings modified as indicated above) predicts strong response. Apparently, nonlinearity with dissipation can also *reduce* amplitudes, since observed surface winds and momentum fluxes are often smaller than those predicted from linear theory. Thus a properly formulated nonlinear time-dependent model should generally give more accurate results, and both the physical formulation and the results for the 11 January case suggest that the PC model has merit. On the other hand, it is not clear whether this model exhibits as good a selectivity in its response to varying sounding environments as does a linear model. We are particularly puzzled by the rather anomalous prediction of "hurricane force" surface wind speeds behind a mountain range somewhat lower than the Colorado Rockies, using a "standard midlatitude winter" sounding. Such intense downslope winds are not part of the average midwinter conditions in Boulder, occurring probably less than 1% of the time.

We believe that PC also condemn too strongly the use of a hydrostatic model expressed in potential temperature coordinates, at least by comparison with their non-hydrostatic model. Overturning, of course, cannot occur in the θ coordinate framework, so that very large amplitude solutions of Long's equation cannot be simulated. In the presence of significant mean shear, however, we find that wave-induced shearing instability occurs frequently, and from observational evidence appears to be more important than overturning instability. Our use of a turbulent mixing formulation dependent on the Richardson number was thus based on both observations and physical reasoning. The formulation seems to be essentially irrelevant to (and unhelpful with) computational instability problems. The latter, when they exist, are apparently associated with resonant interactions, and are controlled by use of a small uniform viscosity.

Returning to the 11 January case, we are inclined to accept the PC results indicating the existence of turnover in the θ field, although the observational data density is insufficient for proof. As simulated by PC, however, the available potential energy of buoyancy, $g \int \delta(\ln\theta)dz$, does not appear to exceed 100–200 $m^2\ s^{-2}$, while the shear energy present in the wave trough near 5.5 km (PC, Figs. 31e and 31f) is over $10^3\ m^2\ s^{-2}$ in a region of very low Richardson number. The latter magnitude corresponds well with Lilly's (1978) observational analysis and also with the KL8 prediction.

The PC model also allows simulation of the trapped modes often observed in the lower troposphere. In the 11 January case there is no evidence that these modes appreciably influenced the dominant forced mode or the surface wind intensity, but their presence seems to considerably confuse the interpretation of the PC results. PC exhibit a calculation for comparison with that of 11 January using a sounding identical except for increased wind speed in the stratosphere. For both cases linear analysis indicates the existence of a "leaky mode" lee wave, but in the unmodified sounding case its resonant wavelength is too small to be excited significantly by the smooth mountain profile imposed. In the modified case this resonant solution dominates, and for unclear reasons prevents the apparent amplification observed in the unmodified case. If PC simulated actual mountain shapes in either two or three dimensions there would be greater excitation of the resonant mode in the unmodified case, which might prevent development of the large-amplitude hydrostatic mode.

For the 11 January case we note that the maximum low-level wind speeds predicted by the KL5 linear theory, the KL8 hydrostatic model, and PC were all nearly 60 m s^{-1}. Thus, in our view the need for nonlinear amplification mechanisms (particularly those related to overturning) is not clearly established for this data set, although nonlinear influences are obviously significant to the wave structure. We offer no objections to researchers adding realistic complexities to their models and testing the atmosphere's sensitivity to them, but believe there is also merit in exploiting the simplest models which contain the physical core of the problem and also make verifiable predictions.

3. Self-induced critical layers

In their Section 7 PC hypothesize a new mechanism of nonlinear wave amplification involving reflection from the region of zero or reversed flow associated with an overturning wave. The need for such a mechanism was apparently discerned from some of the results of their simulations, including the anomalous one mentioned above, and comparison of these results with the 11 January data. From the comparisons noted above we find little evidence for such self-induced amplification in the 11 January observational data. In KL8, however, we reported on the results of two simulations based on profiles with mean flow decreasing with height. One of these dealt with the strong mountain wave observed above the eastern slope of the Colorado Rockies on 17 February 1970. Upstream of the mountain range and near the tropopause a 2 km deep layer of neutral stability and essentially zero velocity was documented for both the simulation and the observed data. This layer appeared to absorb a significant portion of the wave energy propagating up to its level, thus acting like a critical layer, but one which was not present in an upstream sounding. The observed and computed vertical transport of momentum agreed well throughout the troposphere, yielding a value of ~6 dyn cm^{-2}. The linear transport of 5 dyn cm^{-2} (not presented in KL8 but calculated using the KL5 model) was also close to the observed value, which indicates that significant reflection from the apparent critical layer did not occur.

In the second simulation a true critical layer was imposed in the initial environment. The momentum transport beneath it was nearly double that computed from linear theory, suggesting that "substantial partial reflections of the wave may be occurring" (KL8). We were not confident of the validity of this result, however. Since linear theory predicts that the phase of the waves changes rapidly with height as the critical layer is approached, reflection apparently can either increase or decrease the net transport, depending on the detailed structure and interactions near this layer. We question whether the reflection effect is properly represented in any existing mountain wave model.

The PC hypothesis for formation and operation of a wave-induced critical layer is restated in the authors' accompanying reply but in our view does not contain a reasonable semblance of an amplification or instability mechanism. In describing nonlinear wave dynamics the authors seem to use linear theory concepts where they wish to, around critical layers, but discard them elsewhere, such as at other reflecting interfaces. Yet we are inclined to agree, based on the somewhat ambiguous evidence in their simulations, that some unexplained amplification mechanism comes into play in the presence of wave breakdown, whether caused by convective overturning or by shearing instability. Breakdown and dissipation produce irreversible changes in the mean flow, and at least in the shearing instability case these changes can evidently propagate upstream through blocking action.

Finally we would like to suggest an independent test of the validity of the PC hypothesis, involving simulations of flow over a highly asymmetric mountain (gradual upslope and steep downslope), as used in our study of nonlinear waves (Lilly and Klemp, 1979). For such a mountain profile the maximum flow steepening occurs one-half wavelength above the surface rather than the three-quarter wavelength appropriate for the symmetric bell-shaped mountain. If flow breakdown and subsequent overreflection occurs as suggested by PC then the "resonant cavity" should, we suppose, lead to nonlinear wave suppression, rather than amplification. Since our solutions of Long's equation exhibit strong nonlinear enhancement, a reversed effect on breakdown should be quite striking if present.

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