Tellus (1977), *29*, 287–288

Reply to J. R. Holton

By LLOYD J. SHAPIRO, *National Hurricane and Experimental Meteorology Laboratory, NOAA, Coral Gables, Florida 33124, U.S.A*

(Manuscript received December 13, 1976)

I agree with Holton that further studies incorporating synoptic-cumulus scale interactions as well as boundary layer friction are necessary. In particular, the last two terms in eq. 5 of Shapiro (1977) can not balance with an inviscid interior (Stevens et al. (1976)). The tendency of these terms to balance can be attributed to the inclusion of cumulus momentum transports and can not be specified *a priori* without omitting essential physics. Thus a barotropic model, such as Holton's (1975), which can not incorporate the vertical structure of the heating or the waves and which allows only a Rayleigh friction form of cumulus momentum transports (Holton's eq. 14), can not properly diagnose the dynamics of the waves above the boundary layer.

Shapiro (1977) explicitly considered the more basic question of how well Holton's barotropic model could represent the interaction between an *inviscid* thermally forced wave and a frictional boundary layer. When a wave is thermally driven it makes sense to diagnose the response to a specified heating distribution (cf. Holton 1971). A model which does not faithfully represent the character of such forced solutions can not correctly model the time-dependent evolution of a wave. The analysis in Shapiro (1977) indicated that the interaction between the boundary layer and the interior was misrepresented in Holton (1975) due to the absence of any condition on the continuity of vertical velocity w at the interface z_B. The response (in pressure, for example) to a specified heating Q in any baroclinic model is dependent on the presence of the boundary layer, due to the coupling of $w(z_B)$ to the interior. In Holton's model the response in pressure to a specified heating is *independent* of the presence of the boundary layer. It would seem that any system which could not model such a significant feedback mechanism would be of quite limited validity. If w were allowed to couple with the thin interior layer in Holton's model, a strong convergence would be induced in regions of $w(z_B) < 0$ (boundary layer divergence, where $Q = 0$). The interaction between the boundary layer and interior in a baroclinic system, where $w(z_B)$ appears as an interface condition and not simply as a source term, is much more complex. It should be noted that the equations for the horizontal structure of the waves become formally equivalent to the shallow water equations with mean depth equal to the equivalent depth of the baroclinic system only in the absence of friction. The use of inviscid modes determined from such a system, as is done in Holton's model, is invalid in the presence of a frictional boundary layer if the boundary layer is not a small perturbation on the interior so that the inviscid modes, and their associated pressure fields, are not altered.

Holton's contention that his model is not equivalent to the "inconsistent" solutions in Shapiro (1977) is not supported. In fact Holton does use the pressure field determined externally from an inviscid model to drive a frictional boundary layer, as explicitly stated in Section 4 of Holton (1975). The inclusion of the feedback to the interior by the cumulus heating due to boundary layer convergence in Holton's model omits the vital coupling from $w(z_B)$ itself. As pointed out in Section 4 of Shapiro (1977), R^* is identically the ratio δ_s/δ between boundary layer and interior convergence which would be obtained for the mid-latitude modes with Holton's barotropic model.

There is no implication that Holton's magnitudes of pressure, horizontal velocity and vertical velocity are not consistent with observations. This in itself, however, is not a true test of the validity of Holton's model. Since the ratio between the thermal forcing Q and $w(z_B)$ is determined in Holton's model by an adjustable "empirical constant" γ (Holton's eq. 13), the magnitude (but not the phase) of the response is also adjustable. However, whatever the magnitude of Q in Holton's solutions,

$w(z_B)$, Φ and horizontal velocity are all a factor of $|R^*/\hat{R}|$ larger than that consistent with the dynamics of the forced inviscid wave above the boundary layer. In a baroclinic model Φ adjusts by a (complex) factor of $\hat{R}/R^*$ in the presence of the boundary layer, while in Holton's model Φ is fixed for a specified forcing Q. *Thus the barotropic model will misrepresent the relation between the heating and the response.* Since $R^*/\hat{R}$ is complex, the adjustment of the response is in both magnitude and phase relative to Q. Tables 1–3 in Shapiro (1977) indicate that the extent of the adjustment depends on the horizontal and vertical structure of the mode. It will also depend on the vertical distribution of heating (which cannot be specified in Holton's model), boundary layer friction and nonlinearities. Due to the presence of the factors mentioned above, as well as Holton's use of equatorial rather than mid-latitude beta-plane geometry, the exact error in Holton's model cannot be easily determined. To the extent that boundary layer friction is significant for Holton's wave modes, the error in his solutions is potentially large.

REFERENCES

Holton, J. 1971. A diagnostic model for equatorial wave disturbances: the role of vertical shear of the mean zonal wind. *J. Atmos. Sci. 28*, 55–64.

—— 1975. On the influence of boundary layer friction on mixed Rossby-gravity waves. *Tellus 27*, 107–115.

Shapiro, L. 1977. Frictional effects on thermally forced waves. *Tellus 29*, 264–271.

Stevens, D., Lindzen, R. & Shapiro, L. 1976. A new model of tropical waves incorporating momentum mixing by cumulus convection. Submitted to *Dynamics of Atmospheres and Oceans.*