Zonal scale and temporal variability of the Asian monsoon anticyclone in an idealised numerical model by Rupp & Haynes is a very clear investigation into the upper tropospheric Asian monsoon anticyclone. The spatial scale of this prominent summertime atmospheric feature has been a puzzle and it is nice to see a clear numerical experiment that shows the possible means by which longitudinal localization is achieved that does not involve *ad hoc* frictional or damping terms. The authors begin by showing that the response of the atmosphere to local heating with a base state of rest is almost unconfined longitudinally. Whereas, a base state that supports midlatitude jets and baroclinic eddies immediately leads to a PV anomaly that confined in the zonal direction. Further, the time dependent solution with differing forcing strengths shows a shedding of vortices from this anticyclone which have important implications for local monsoons. In particular, the change from eastward to westward shedding events depending on base state (and thus the midlatitude flow) is interesting and certainly worthy of further investigation. In all, I am happy to recommend the publication of this article in *Weather and Climate Dynamics*, but I have one or two main questions and some other points (detailed below) that the authors should try to address.

Main Comments

1. I see the remarkable change in the response of the atmosphere when the base state is changed. Specifically, the zonal restriction of the response when a state with jets and midlatitude baroclinic waves is considered, as opposed to the unconfined nature of the response with a base state of rest. But why? What is the physical reason for this change in the response? Are the baroclinic eddies acting as a means of eddy-diffusivity for the PV? Does this take the place of an *ad hoc* damping?
2. What sets the scale of the PV anomaly? Indeed, in figures 4,7 and 9 we see a zonally confined PV response with large ΔT. In Figure 9 this is measured to be about 5,000 km. Is there an estimate where this length scale comes from? Is this a reflection of the scale of baroclinic eddies? Also, this scale is much smaller than the observed anticyclone that measures about 10,000 km in Figure 1. Any reasons for this mismatch?
3. The anticyclones obtained in these simulations all have a marked tilt, but the actual anticyclone in reanalysis (Figure 1) does not. Any thoughts on why this is the case?

Specific Comments

1. In these simulations, at early times, does the model produce wavetrains like those seen in typical equatorial heat source interacting with a jet type problems (for eg. those seen in Sardeshmukh & Hoskins 1988)?
2. In Figures 3, 7 & 9, it might be useful to include contours of the zonal mean flow.
3. Figures 1 and 10. Please either remove, or maintain, the aspect ratios of country outlines in these figures.
4. Line 315 and Figure 4. Does this "superposition" like result point to the linearity of the phenomenon?

5. Line 360 and Figure 7. Why does the PV anomaly become weaker with increasing ΔT?

6. Line 405 and Figure 10f. Don't we also see a piece moving westward on Aug 1?

7. Figure 10. As an aside, this split of the anticyclone and its reformation is reminiscent of the break-up/reforming of the polar vortex! Any comments?

8. Line 440 and Figure 12. It's quite difficult to follow the strengthening and shedding of vortices in this figure. Won't a line plot at 20N of the be easier to follow? Further, what is going on the southern hemisphere? There seems to be a similar vortex formation and translation in the subtropics.