Response to Interactive comment from Z. Wang.

The comments are reproduced here in Arial font, and the responses to the comments are in Times New Roman font.

1. To reduce or eliminate the downdraft CAPE or D-CAPE, it is necessary to have a neutral atmosphere as illustrated by Tory et al. (2008) and Tory and Frank (2010), but the numerical model simulations in Nolan (2007) and Wang (2012) did not suggest that this is a necessary condition for TC formation. Nolan (2007) emphasized the importance of nearly saturated column near the core, and Wang (2012) emphasized the mid-level moistening at the inner pouch region. Wang (2012) also showed that a mid-level theta_e minimum is still present even after genesis in model simulations and field observations. Both studies show that downdrafts strengthen as approaching the genesis time. What really matters is that the updrafts are dominant so that the net upward mass flux and the low-level mass convergence increase with time. Downdraft -free convection is thus not necessary for TC formation.

I agree with this comment. Our summary of the topic did not mention that downdrafts increase as TC formation proceeds despite the reduced DCAPE. This omission is likely to lead the reader to believe downdrafts decrease as TC formation proceeds. The following sentences have been added to address the issue. “However, modeling studies (e.g., Nolan 2007, Wang 2012) have shown that the downdraft mass flux increases approaching TC formation, but at a much lesser rate than the increasing updraft mass flux. Presumably the increasing availability of moisture to be evaporated, in the intensifying and expanding convection, more than compensates for the less favorable conditions for evaporation.”

1. P17543, L27-29: “It follows that in a breaking wave the enhanced LDV must occur over a large enough area for the streamlines to rotate through 360.” This is true for coarse resolution data. For high-resolution data, one would see high LDV is very localized and associated with convective vortices or their remnants.

The following sentence has been added to address this and the next point. “In reality convective regions contain a mix of low and high deformation flow on the scale of convective vortices. Smoothing of the wind field is necessary to identify LDV on the pouch scale.”

2. P17543, L1-3: A region of enhanced and sustained LDV is important because it gives enough time for the thermodynamic transition to take place. But what does this have to do with whether fluid parcels complete a full rotation or not?

The distinction was being made because the identification of instantaneous LDV is being proposed as an alternative to the DMW pouch. The DMW pouch identified nearly circular flow, but more specifically recirculating flow. These sentences were acknowledging that a measure of LDV reflects instantaneous flow, but we can assume it is recirculating if the LDV is sustained for a suitable period of time.

The circulation may help to mix moisture inside the pouch but mixing does not happen for solid body rotation.

I suspect most of the mixing occurs on the convective scale, within the larger pouch-scale LDV.

3. P17544, L6-8: "It follows that the larger in horizontal extent and the greater the magnitude of an enhanced cyclonic LDV anomaly the greater the potential for TC formation." I agree that strong LDV is favorable, but a large area may not always be. The pouch needs to be large enough so that the dry air entrainment or intrusion at the pouch periphery does not readily affect moist convection at the inner pouch region, but if a pouch is too large, it may extend to the dry regions and directly advect dry air from the north (Wang et al. 2012; MWR, 140, p1144–1163).

This is a good point. The sentence has been edited to read "…the greater the dynamical potential for TC formation." It is worth noting that the LDV identifies the inner pouch region and not the pouch periphery.

4. P17544, L14-15: I would like to draw the authors' attention to a recent study by Wang et al. (2012; MWR, 140, p1144–1163). This study examined the vertical structure of the wave pouch for some named storms during 2008-2009, and it was suggested that a deep, diabatically activated wave pouch extending from the middle troposphere (600– 700 hPa) down to the boundary layer is a necessary and highly favorable condition for tropical cyclone formation. Wang et al. (2012) also discussed why a deep pouch is important for TC formation (first paragraph of section 4d). I would like to hear the authors' insights in this. Could you elaborate on why the depth of enhanced LDV is important?

Thanks for bringing this paper to my attention. My insights are consistent with the ideas raised in Wang et al. (2012), although "insight" might be overstating it. When coming up with the two necessary levels of 850 and 500 hPa for the OWZ thresholds (about three years ago) I had perhaps a naïve conceptual model of TC formation, based on system-scale tangential momentum budgets (e.g., Montgomery et al. 2004, Tory et al. 2006b) that showed deep layer vorticity convergence from the low- to middle-troposphere. Numerous other studies have shown that horizontal vorticity convergence is a significant contribution to the system-scale vortex spin-up from the B.L. to about 500 hPa or beyond. Above this height I believe tilting plays a dominant role, especially where the horizontal flow is divergent. So, if as argued in Section 2 vorticity convergence and associated balanced vortex spin-up is most efficient in a region of high solid body vorticity, then we should look for enhanced LDV at both 850 and 500 hPa. (The assumption being that it will also be enhanced on the levels between.)

It is interesting to note your definition of the deep-layer pouch (950 to 700 hPa) does not immediately exclude the non-developer PG26L, which had only a weak OW signature at 700 hPa and nothing above. If my estimate of the OWZ from your Fig. 13 is correct, the 500 hPa threshold would not have been satisfied throughout the entire period. Thus, an OWZ analysis would have dismissed PG26L purely on dynamical grounds. Your suggestion that upper level dry air may have inhibited the systems further development is also likely to be important, as indeed very dry air is present above 600 hPa. It's interesting that our analysis would have missed this aspect because the RH at 700 hPa exceeds 70%

for almost the entire time. Initially we included a 400 hPa RH threshold, as Kingsmill and Houze (1999) showed deep-broad tropical convection was observed to be more humid at that level than deep-narrow convection. However, we found it to be a poor indicator. I suspect the capacity for evaporation is not that great at these heights, which could mean that large variations in RH result in relatively small variations in evaporation potential.

5. 3.3 Detector: i) Why did the authors choose 850 and 500 hPa for OWZ? Do these levels produce the best results or there is some dynamic basis?

See previous response.

ii) For the 850-200 hPa shear, did you calculate the vector difference between the two levels or the total shear with respect to a reference level averaged over the layer?

The vector difference between the two levels was calculated. This should now be clear in the description in section 3.3.

6. P17557, last paragraph: It may be worth pointing out that a "miss" rate is irrelevant for "sufficiency" test.

   I agree that the miss rate is irrelevant for a truly sufficient condition, because there would be no misses. But, as we try to edge closer to sufficiency, both the miss and FA rates are measures of how close we are getting to sufficiency. Thus the miss rate remains relevant in this context.

7. Footnote #8: What is the typical value of "a" in Fig. 1? The inner pouch region emphasized by Wang (2012) is likely within the green area in Fig. 1 based on a quick look at the tangential wind profile.

   I'm not sure what a typical value of "a" would be. Yes, I also estimated the inner pouch region of your paper would be in the green area. In one of Eric Rappin's idealized genesis simulations the "a" radius collapses to about 50 km (from about 125 km, as determined by the prescribed initial circulation) just prior to the rapid drop in MSLP.

8. L17563, L18: "11 circulations"?

   What I am trying to communicate is that there were 11 times as many "features" identified by the OWZP1 as there were TCs in the observed database.

9. L17564, L6-9: Is the sustain time period checked following the propagating clumps or at a fixed location? The former should be more reasonable.

   Yes, the analysis is performed following the storm track.

10. It is a little hard to follow the detection procedures. It would be helpful to create a flow chart.

An additional four figures have been added to make it easier to understand the detection, tracking and verification procedures. These include schematics and a flow chart.

11. P17568, L20: The thresholds, especially OWZ thresholds, must be sensitive to the model resolution. For high-resolution data, such as ECMWF 25-km forecasts, the OW field and vorticity field are very noisy. Do the authors plan to convert all the model data to the 1X1 degree resolution?

Yes, this is what we have been doing with ACCESS (our local NWP system).

12. P17571, last paragraph: when applying this index to GCMs or operational forecasts, the performance of the TC detector inevitably depends on the performance of the model.

Exactly. One reason for developing the TC detection procedure independent of climate models is to avoid compensation between model and detection error. See our discussion of this topic in two subsequent papers in J. Clim., pending minor revision.