This paper demonstrates that low-deformation vorticity is a necessary requirement for tropical cyclone formation. The OWZ parameter is a combination of the familiar OW parameter and absolute vorticity. This parameter along with a set of thermodynamic conditions give statistical evidence that nearly all TCs develop in an environment of low deformation vorticity. This extension of the marsupial pouch concept of Dunkerton et al. (2009) provides further statistical evidence of the requirement of low-deformation vorticity over a broad data set spanning all ocean basins. The results are interesting and important and should ultimately be published. I do, however, have some concerns that I would like to see addressed before the paper is accepted for final publication.

Major Comments:

1. The theoretical arguments for solid body rotation providing a more favourable environment for TC genesis are important. However, the advantage of OW_norm over OW from a theoretical perspective is that it is maximized for solid body rotation in axisymmetric flows while OW is not. This is because the magnitude of OW depends on the magnitude of vorticity. Since OWZ multiplies OW_norm and absolute vorticity, it is then also dependent on the magnitude of vorticity. Given the same hypothetical tangential wind profile in figure 1, does OW_norm have an advantage over OW? How do OW and OWZ differ in this situation? A figure comparing OW and OWZ for a hypothetical flow may demonstrate the differences between OW and OWZ, and may show by what % OW differs from its maximum at the line where OW_norm is maximized. Since the results in Sections 4 and 5 are sensitive to the choice of OWZ, the improvement in OWZ over OW from a theoretical perspective should be sufficiently large that any improvement is not lost as noise in the choice of parameters.

When OWZ is tested as a necessary condition for TC formation, were similar parameters for OW tested? If there is a real advantage of OWZ over OW, showing that there is not an OW parameter which is necessary for TC formation would significantly strengthen this paper.

Section 2: You say that OWZ is an alternative to vorticity, which is the Galilean invariant quantity used in every TC formation diagnostic. There are many studies that have used OW as a diagnostic for TC formation, though not in a statistical sense (see Montgomery et al. 2012 BAMS). You suggest that OWZ measures low deformation vorticity, but so does OW, which should be noted. OW also diagnoses the tendency of the flow to undergo solid-body rotation through a linear stability analysis, see Schubert et al. (1999) for a derivation of OW.

Section 3: The length of this section lessens the impact of the main results, and should be condensed. For example, a disproportionate amount of text was used to describe the cluster tracking techniques. If the authors consider it necessary to include all of these details, an appendix may improve the readability of the paper.

Specific Comments:

Page 17555 line 3: Why is any isolated point discarded? Does this requirement of minimum storm size change the hit rate or false alarm rate? If so, the choice of threshold is then very important for cluster identification. I would suggest expanding Table 1 to show how many clusters were identified with each set of parameters to better show the false alarm rate.

page 17555 line 22: "or both do not satisfy the core threshold limits, then the larger or more intense clump is retained." If the conditions are not satisfied, then why is either clump retained?

page 17559 line 17: Though 96% of TCs satisfied this condition, a false alarm rate of >1000% seems very high, even to demonstrate that the condition is necessary though not sufficient. Given that the other conditions are in Table 1 row 1 are "almost always" satisfied, is this condition almost always satisfied in regions where the other conditions are satisfied? To show that the condition is necessary, it can't hold true everywhere. What % of the TC formation basins surveyed typically have this characteristic threshold of OWZ, including areas that were discarded due to having only a single point or nearby clusters that were grouped together.

page 17559 footnote 8: The pouch in DMW includes the high deformation regions surrounding the sweet spot that extend well into the hostile range. While this is a true statement, the pouch of DMW serves to aggregate vorticity into the center of the pouch while shear deformation encloses the center. The regions of shear deformation are only hostile to regions that they interact with. Since the center is near solid body rotation, there is little interaction between the center and shear layer surrounding it. The shear layer is hostile toward particles that would enter the circulation from the outside, disrupting their ability to enter the circulation center. In this sense, the shear layer is protecting the region of solid body rotation.

page 17565 line 14: In the comparison of absolute vorticity and OWZ in Figure 4, the contours for OWZ do not match the conditions given in Table 1. For monsoon trough cases, there are typically many OW centers in very close proximity, and it is often difficult to distinguish which center is the actual precursor to a TC.   Would plotting the 50 s^-1 contour at 850 hPa and 40 s^-1 contour at 500 hPa used for the necessary condition in Table 1 still allow the precursor to Larry to be identified? Does OWZ more clearly identify this precursor than OW?