Response to Reviewer comments, Anonymous Referee #2.

The reviewer has made some very insightful comments and posed a number of important questions that should be addressed. I have cut and pasted the reviewer's report below (Arial font), and embedded responses to the comments and questions (Times New Roman font).

But first it has come to my attention that the description of the smoothed wind shear on page 17554 was ambiguous. The text implied the wind shear was calculated before the smoothing was applied. In reality the wind was smoothed before the shear was calculated. This is an important distinction as the results differ by a non-trivial amount.

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.

Agreed.

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?

If the primary purpose is to identify the radius at which V/r is maximized, OW_norm has a distinct advantage over OW. For a profile such as in Fig. 1, OW is maximized closer to where dV/dr is maximized than where V/r is maximized.

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.

I have included an extra figure to address this point (Fig. 2 of the resubmitted manuscript). To ensure a fair comparison between OWZ and OW the OWZ shown in the figure has no contribution from the Coriolis parameter ($f = 0$), and the square root of OW (hereafter sqrtOW) is plotted for easier comparison. (Given that we are considering only flows in which OW is positive, sqrtOW would perhaps be a better indicator of low-deformation vorticity than OW, as it has the same units as vorticity.)

The tangential velocity is similar to the hypothetical profile of Fig. 1, but extends only to the radius of maximum wind. The dotted lines show the shear and curvature vorticity components that make up relative vorticity (thin green line), and OWZ and sqrtOW are indicated by thin blue and yellow lines respectively. As argued in the paper OW_norm is maximum and equal to 1 where there is local solid body rotation ($dV/dr = V/r$), which is also where V/r is maximized (r~142 km). Also at this radius zeta, OWZ and sqrtOW are identical (as expected).

The reviewer asked "by what % [does] OW differ from its maximum at the line where OW_norm is maximized". The difference in position is 30 km, which is 15% of the RMW, or about 21% of the radius of maximum *V/r*. The maximum value of sqrtOW is 21.4% greater than sqrtOW at the maximum *V/r* (which translates to a difference of nearly 50% for OW). For this wind profile there is an advantage to using OW_norm over OW.

As it turns out there is little difference between sqrtOW (thin yellow line) and OWZ (thin blue line) for the reason the reviewer mentioned above. Again if the purpose is to identify where V/r is maximized, then the maximum OWZ occurs slightly closer to this radius, but it is an insignificant difference, and one can only conclude that there is no real advantage in using OWZ over OW for this *particular* purpose.

Returning to the reviewers original point "…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…" the above discussion shows that OWZ is also not necessarily maximized where the flow is locally in solid body rotation. The text has been amended to reflect this. In fact a significant proportion of the text between pages 17547 and 17551 has been rearranged to better introduce the topic incorporating the arguments discussed here.

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.

To test this point an attempt was made to best reproduce the OWZP5 (Table 1) result using sqrtOW in place of OWZ. The thresholds were adjusted in increments of 5exp-6 s^-1 until the optimum performance was reached. A significant latitude dependence in the performance was discovered, which would be expected if Coriolis was important. For example, for TCs forming within 10 degrees of the equator there were 11 times more false alarms than misses. The thresholds were effectively tuned to the latitude range where the TC formation is most common.

I considered possible ways that the Coriolis parameter could be incorporated with sqrtOW. A simple addition is problematic because it gets included everywhere, not just where flows are likely to be closed. The same problem occurs for the square root of an absolute OW (where absolute vorticity is used instead of relative vorticity in the OW calculation). The way Coriolis is included in OWZ does appear to be the simplest method of incorporating the important contribution of the Coriolis parameter to a low-deformation vorticity TC-formation diagnostic.

A brief discussion of this comparison between OW and sqrtOW has been incorporated in the re-written part of section 2.

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.

Yes, OW alone can be used to *identify* low-deformation vorticity (and as noted above the square root of OW is probably a good measure of low-deformation *relative* vorticity. The reviewer's point highlights a subtlety that was not explained in the paper. OW alone can be used to identify the TC formation sweet-spot, but we were looking for a parameter that could be used globally to quantify TC formation potential. OW was originally dismissed because we were looking for a measure of low-deformation vorticity that incorporated absolute vorticity due to its theoretical importance in TC formation. OW had the wrong units and did not include Coriolis.

Three sentences in the first couple of paragraphs of Section 2 have been edited to read:

1. "In this paper we propose a new, alternative dynamic quantity to identify and quantify the TC formation sweet-spot that is Galilean invariant and thus identifiable in instantaneous data."

2. "The Galilean invariant quantity vorticity, is used in most TC formation diagnostics…"

3. An additional sentence was added following "In contrast, enhanced levels of the Okubo-Weiss parameter in the pouch are only found surrounding the sweet-spot in the DMW papers" that reads "Indeed OW was used by these authors to diagnose circulations with formation potential in near real time during the PREDICT experiment (Montgomery et al. 2012)."

It is worth adding that the above discussion does not invalidate the use of OW as a quantitative diagnostic in the DMW papers, as the storms they analysed were all at similar enough latitudes.

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.

Much of Section 3.4 has now been moved to an appendix, and a number of figures have been added to improve comprehension of the complex detection and tracking procedures (as suggested by Zhuo Wang).

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.

The sentence the reviewer queried is actually incorrect. It reflects the method used in the early development stage of the OWZP, in which isolated points were discarded. At a later stage we decided to retain them in order to lengthen storm tracks. This change led to fewer tracks being counted twice, and a number of tilted systems were tracked for longer periods. (Tilting occasionally led to only one grid point being satisfied as less of the circulation overlaps). However, any isolated grid point that satisfies the initial thresholds, cannot satisfy the core conditions, which require at least two neighbouring grid points that satisfy the core thresholds.

The offending sentences have been removed, and the alternative explanation added at the end of Section 3.4.

In searching the text for the best place to add the alternative explanation it became apparent that the description of the core condition dealing with land influences on TC formation had been removed in some earlier reformatting. The land influence description has now been added to section 3.4.

It is worth noting that it was necessary to make numerous subjective decisions in developing the parameter, as it is not physically possible to investigate the entire parameter space. Further investigation of many aspects of the parameter will quite likely identify further improvements, but a line had to be drawn somewhere. We believe the parameter is very useful in its current form, and encourage others to further test the parameter space and publish any improvements.

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.

I'm not sure I understand the purpose of the suggestion here. I could include a column with numbers of clusters but it will not offer additional information on the false alarm rate. There are many clusters in a storm track, and for this test (OWZP1) only one cluster in the storm track needs to satisfy the core thresholds for it to be flagged as a potential developer.

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?

In short the answer is no. The identified circulations are quite distinct and easily trackable by the tracking code. The nature of the OWZ is such that everywhere with anticyclonic or zero curvature is immediately excluded, which means only regions of cyclonic curvature at both the 850 and 500 hPa levels can be included.

I actually do not think 1000% is very high. It's effectively saying that for every TC there are about 10 other circulations identified. I don't know what the number is but if the same calculation was performed using absolute vorticity I expect it might be up to an order of magnitude higher, and perhaps very difficult to track.

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.

As alluded to above, the regions of satisfied thresholds are distinct and transient, so at any point in time they would make up only a small percentage of an ocean basin. For example, on January 1 1989 (the very first day of the analysis period) there were 53 grid points in the southern hemisphere (-65 degrees to the equator) that satisfied OWZP1, including land points. These grid points were grouped into seven clumps.

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.

I agree that this is a very important aspect of TC formation. However, the point being made was that we require overlap between the central pouch regions, the region of near-solidbody rotation, whereas Raymond and Lopez Carrillo required overlap of the larger pouch region. Our requirement was thus stricter than theirs. The footnote was intended to remind the readers that the pouch includes the outer high deformation region, where the development of the core vortex will not occur. It was not meant to be a criticism of the DMW work. To ensure the reader does not interpret the footnote as a criticism, it has been edited to read: "The pouches of DMW include the high deformation (OW < 0) regions surrounding the formation sweet-spot that protect the central region of low-deformation vorticity. The TC development occurs inside this protective layer, which in Fig. 1 extends well into the 'Hostile' range."

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.

The contour values used in this figure match the thresholds used in OWZP5 (Table 1), which were tuned to best identify circulations with potential for TC formation. The lower values of OWZP1 are used to identify circulations worth tracking.

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.

This is correct. In reality every small scale circulation embedded in the monsoon trough would contain a central core of low-deformation vorticity surrounded by the high-deformation vorticity, leading to multiple positive centres of OW or OWZ. However, in interpolating to the 1 degree by 1 degree grid this finer structure is smoothed out, leaving behind OW or OWZ signatures that represent the larger scale circulation.

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?

Yes using 50 and 40 instead of 60 and 50 10^-6 s^-1 does still yield distinct circulations, including the Larry pre-cursor.

Does OWZ more clearly identify this precursor than OW?

I expect OW could just as easily be used to identify the TC Larry pre-cursor. As an indicator of low-deformation vorticity, OW is more difficult to compare with absolute vorticity than OWZ. Furthermore, we believe incorporating OW in a global predictive scheme such as the OWZP could be problematic if Coriolis is not somehow incorporated. (Although our limited experimentation with sqrtOW suggests that it may only be problematic within about 10 degrees of the equator.)