Response to Reviewer 2

(Please note that the Reviewer's comments are in normal font and our response is in italics)

This paper documents the convectively coupled quasi-biweekly oscillation (QBWO) in the South west Indian ocean. The paper is easy to read and is rather descriptive in nature. The introduction is expansive and provides relevant background on the topic and ends with a clear statement of the goals of the paper. The discussion section recaps some of the physical mechanisms of the genesis of this oscillation.

Despite some promising initial discussion of background moisture distribution (the authors appeared to hint at some moisture mode type behavior) a vorticity budget was the route taken here. This does not address the organization and modulation of convection (perhaps a moisture or moist static energy budget would be useful for that). The key result here is that planetary vorticity advection accounts for the propagation of the wave and stretching to its amplification. The former is consistent with the notion that the wave is an ER type mode and the latter points to the vorticity generation by convergence/divergence associated with convection.

In section 5, the paper presents some material on tropical cyclone formation during the QBWO of 2008–2009. This is also easy to read and is again descriptive in its treatment with no calculations or diagnostics (beyond maps of filtered fields)

Over all, the paper provides documentation of the QBWO in a basin that has not received as much attention as compared to other basins. The results are not necessarily novel but will be useful reference points for future work (such as evaluation of theoretical and conceptual models of this phenomenon).

We thank the reviewer for the constructive comments. Indeed, our longer term goal in this project is to build towards a moisture mode like paradigm for the QBWO. The present work lays the foundation for this. As the reviewer has suggested, we have now included a moisture budget which refines our understanding of the mode along with our vorticity budget analysis.

Other comments:

1. The authors might wish to consider calculating statistical significance for their composite anomalies and only show values that are deemed significantly different from zero.

Statistical significance of the composite is included in the revised presentation. In fact, the composite figures have been revised and all values presented are statistically significant at a 95% confidence level. The comparison to red noise has already been performed by Kikuchi & Wang 2009. We should have referenced this in the text and have now done so. Indeed, a signal above red noise in the 12–20 day band (in south-east Africa and the southern equatorial Indian Ocean), that accounts for about 8% of the variance in a empirical orthogonal mode decomposition of OLR anomalies in the SWIO was demonstrated in Kikuchi & Wang 2009. As already mentioned, the box chosen has been varied to check for the robustness of results. Indeed, altering it within the pocket of high OLR variability in the southwest Indian Ocean does not affect the results.

2. The data and methods seem reasonable

Thank you.

3. Line 160: Just to be sure, can you add a few lines (connecting constant phase) on Fig. 2 to illustrate the wave (phase) propagation. Can you also estimate the southward phase speed and check if they are realistic and the patterns in the Hovmoller represent propagation.

The guiding lines for the phase speed are shown in Figures 5,6 & 7. Indeed, these allow for a estimation of the speeds in different directions.

4. Line 165: Same as above, but for the group speed.

See response above.

5. Line 207: How does an oscillation die? Is it being damped or absorbed by the background flow? Or is the "weakening" of the composite anomalies simply because one is averaging a band-passed field many days away from the reference time (lag 0).

The moist signal fades out as convective coupling in this mode dies out by about 35S. The reviewer's questions is very astute in that a dry signal can continue propagating without the accompanying moist anomaly. We see that the vorticity anomalies tend to merge in with the midlatitudes and be "carried away" from this region. We have not studied the implications of this tropical midlatitude interaction, though it would be a very interesting aspect to pursue.

6. Figure 8: Any idea why the structure changes from 1st baroclinic to a tilt? Is it really tilting or is that simply an artifact of the contouring/shading?

The first baroclinic signal fades as convective coupling weakens. In addition, the vertical shear in the background flow plays a role in the observed tilting of the system.

7. Line 484: OLR anomalies are the visible outcome of moist convection. Please rephrase this sentence to make it less redundant.

Removed.

8. Section 5 on the impact of the QBWO on troical cyclone formation is again very qualitative in the way it is presented. No real issues here but a more comprehensive study would need simulations with a full physics model and sensitivity experiements.

We certainly agree with the reviewer here. This is a first cut at the issue that clearly shows a qualitative association between the QBWO and cyclogenesis. A simplified or full model study would be required to probe the dynamical mechanisms involved and the pathway of nurturing cyclones.