

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-66

Anonymous Referee #1

Referee comment on "Southwestward propagating quasi-biweekly oscillations over the South-West Indian Ocean during boreal winter" by Sambrita Ghatak and Jai Sukhatme, Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-66-RC1>, 2021

The authors present evidence for the existence of a quasi-biweekly oscillation in the southern Indian Ocean. They show some composites, a vorticity budget analysis, and show how the mode may modulate TC activity. If confirmed, this study could reveal our understanding of these quasi-biweekly modes. However, I have multiple concerns about the contents of the manuscript, which I outline below. Because of this I recommend major revisions.

Major comments:

- The manuscript feels long and disjointed. There is a lack of organization of the manuscript which makes reading it difficult and exhausting. Mean state plots should be shown together, as well as the plots about the vorticity budget. Some plots could be coalesced to save space or even gain new insights. There is also some parts of the manuscript that feel unnecessary or are not well-justified. For example, why discuss equatorial Rossby waves in the Introduction? The mode shown here is not an equatorial Rossby wave. If there's a point to this, the authors should be more clear about it. Overall, I think the authors can trim a lot of the content that is currently shown and focus on the essentials, as well as focus more on the two other major concerns below.
- Statistical significance of the mode: The analysis shown here is based on a composite analysis on a box over the southwest Indian Ocean. It is unclear why this box was chosen, and no attempt is made to show that the quasi-weekly mode is statistically significant. This could be done by showing that the power spectrum of vorticity or OLR is above the red spectrum at the 99% confidence interval. An EOF analysis showing that the eigenvalues corresponding to this mode are statistically distinct could also be shown. However, the analysis as shown in its current form is not sufficiently convincing. This is important, as it is otherwise unclear why the authors chose the filtering process outlined in the paper – it seems ad hoc.
- The vorticity budget is not enough to justify the main points of the paper. There is discussion about moisture advection throughout the paper yet not discussion about a moisture budget. This should be included. Even better would be an MSE budget or a

weak-temperature gradient balance-based moisture budget (see Chikira 2014, Wolding et al. 2016, Adames and Ming 2018a). The authors should also check whether the water vapor explains most of the precipitation variance. On when examining the evolution of moisture can we better understand how convection is modifying the evolution of the vortex.

Minor comments:

Figures: The contents of the figure should be shown in the title. The color bars should say what fields its showing, and the abscissa and ordinate should be labeled. They are not labeled in most figures.

Figure 1: Grid lines are obstructive. Consider removing.

Fig. 3: Same comment as Fig. 1. Lags should be shown in the title to make it easier and more intuitive for the reader.

176-177: outlined in the Data and Methods section. This part of the sentence is unnecessary. Please remove.