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Comment on os-2021-97

Anonymous Referee #1

Referee comment on "Interannual variability of sea level in the southern Indian Ocean: local vs. remote forcing mechanisms" by Marion Kersalé et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-97-RC1, 2021

This study examines the impact of local and remote wind forcing on interannual sea surface height (SSH) variability in the south Indian Ocean, based on numerical experiments with an ocean general circulation model (OGCM). Many past studies worked on this issue using a 1.5-layer, reduced gravity long Rossby wave model that possibly suffers adopted approximations. The current study uses an OGCM and has an advantage over past studies. It is potentially worth publication, but I have a question about the setting of numerical experiments. This and other comments are listed below. I recommend major revision.

Major comment

1) In the ITF-off experiment, the Indonesian archipelago is blocked by land. This eliminates propagation of oceanic waves from the Pacific to the Indian Ocean, but possibly allows wave propagation from the equatorial Indian Ocean to the west Australian coast along the eastern boundary. This route is unrealistic, because waves in the equatorial Indian Ocean intrude into the Indonesian Seas (Durland and Qiu 2003; Syamsudin and Kaneko 2004) and leak out of the basin (Wijffels and Meyers 2004) if the topography is realistic (i.e., there is an opening in the Indonesian archipelago). ENSO excites zonal wind variability in the equatorial Indian Ocean via changes in the Walker circulation (e.g., Xie et al. 2002), which excites equatorial waves (e.g., Chambers et al. 1999; Feng and Meyers 2003). These waves might propagate to the west Australian coast in the ITF-off experiment. Thus, I wonder if ENSO impacts SSH variability along the west Australian coast through zonal wind variability in the equatorial Indian Ocean in the ITF-off experiment, which is an artificial process owing to the experimental setting and does not happen in observations. Please check this possibility and add discussions to the manuscript.

Chambers, D. P., B. D. Tapley, and R. H. Stewart, 1999: Anomalous warming in the Indian Ocean coincident with El Nino. J. Geophys. Res., 104(C2), 3035-3047.

Durland, T.S., and B. Qiu, 2003: Transmission of subinertial Kelvin waves through a strait. J. Phys. Oceanogr., 33, 1337-1350.

Feng, M. and G. Meyers, 2003: Interannual variability in the tropical Indian Ocean: a two-year time-scale of Indian Ocean Dipole. Deep-Sea Res. II, 50, 2263-2284.

Syamsudin, F., A. Kaneko, and D. B. Haidvogel (2004), Numerical and observational estimates of Indian Ocean Kelvin wave intrusion into Lombok Strait, Geophys. Res. Lett., 31, L24307, doi:10.1029/2004GL021227.

Wijffels, S., and G. Meyers, 2004: An intersection of oceanic waveguides: Variability in the Indonesian throughflow region. J. Phys. Oceanogr., 34, 1232–1253.

Xie, S.-P., H. Annamalai, F. A. Schott and J. P. McCreary, 2002: Structure and mechanisms of south Indian Ocean climate variability. J. Clim., 15, 864-878.

2) As is stated in the manuscript, winds along the Australian coast explain only 11 or 15% of local SSH variability (Line 311-313 and 446-448). This low ratio can be visually confirmed by the discrepancy between solid and dashed lines in Figs. 6c and 6d. The authors do not discuss what explains the remaining variability. I suggest more analysis should be carried out to specify the cause of SSH variability along the west coast of Australia in the ITF-off experiment. As is mentioned in the previous comment, there can be an unrealistic process in the ITF-off experiment.

3) In the current study, SSH variability in the ITF-off experiment is attributed to the effect of the atmospheric bridge. However, the authors do not describe how the atmospheric bridge causes wind variability near the west coast of Australia, but guessed its effect from results of the ITF-off experiment. I suggest that they should conduct additional analysis (such as a correlation analysis between an ENSO index and atmospheric pressure and winds near Australia) and discuss how ENSO impacts surface winds near Australia.

4) It may be better to mention the advantage of the use of an OGCM in the Introduction. The 1.5-layer, reduced gravity, long wave model has been used by many previous studies, but it adopts many assumptions and arbitrary parameters. OGCMs cover a far wider range of dynamics in comparison to the 1.5-layer model. The uniqueness of the current study will be further clarified by mentioning this.

Minor comments

5) I am not sure what "with a gap from October 2008 to August 2009" at Line 220 means. There is a gap in mooring observations between 2011 and 2013 in Fig. 2b (magenta line), but no gap in 2008 and 2009.

6) I suggest that "remote forcing at mid-latitudes" at Line 259 should be replaced with "remote forcing", because remote forcing is not located at mid latitudes. ("Remote forcing" mainly refers to wind forcing in the tropical Pacific Ocean in this study, which is located at the near-equatorial regions in the Pacific Ocean.)

7) I suggest that discussions presented at Line 409-417 should be moved to the Introduction. This paragraph summarizes results of past studies and does not mention results obtained in the current study.