Responses to the comments received on manuscript OS-2017-83

Responses to comments from Anonymous Referee #1

The authors investigate the effects of sub-surface mixing in the ocean under severe storm conditions. The introduction and the reference give the impression that the authors know very well the relevant publication and the overview they give is very nice. To my understanding the novel approach of the article is the use of a coupled atmosphere-ocean-wave model to investigate to simulate the atmospheric and oceanic properties on a very fine scale. The focus is on the generation, propagation and dissipation of kinetic energy in the ocean.
The review improved the paper a lot although some things still have to be corrected.

In detail:
Please avoid capital letters when it is not a special name. And it would be nicer if a native speaker would have checked the language.

Response:
We sincerely thank the anonymous Referee for his suggestions. We thoroughly checked the manuscript for any grammatical errors and corrected the same.

l 76: kinetic energy
Corrected

l 80: The NIO is found to decline
Corrected

l94: “discussed” is better than “accessed”.
Corrected

L 148: the Phailin
Corrected

L 164: grid-scale
Corrected

L170: “starching” parameter. This is a major error. There is no such thing as a starching parameter. Please read the model manual again and correct this word!

It was a typo. We replaced the word ‘starching’ with ‘stretching’.

l 239: stand-alone and coupled WRF
Corrected

l241: simulated a larger pressure drop
Corrected.

Figure 4: Did you discuss the high frequency variability which can be seen in the figures somewhere? Why are both model configurations not able to reproduce this feature?

Response: The high frequency variability in mean sea level pressure (MSLP) is primarily due to radiational effects (explained by Pugh, 1987) which are noticed in the buoy measured data. The models are not able to capture this radiation-dependent high frequency variability *in the mean MSLP over the cyclone-influenced region. However, the pressure drop associated with the passage of cyclone (during 10-12 October) was well simulated by the model, particularly by the coupled model. This description is now added in lines 246-249 in the revised version.*

L 246ff: Please mention first what the top level boundary conditions in the stand alone ROMS simulation are, so it is nicer for the reader to understand what is going on. And what about a reanalysis driven simulation? Might be that the SST would also be represented well by the ocean model then.

Response: The following sentence is added in lines 256-257 of the revised version. 'The stand-alone WRF simulated parameters were used to provide surface boundary conditions in the stand-alone ROMS model.'

For a trial, we performed the cyclone Phailin simulations with a reanalysis data and found large bias in SST and currents in the ocean. The intensity and tracks of strong cyclones are not properly represented in the reanalysis data and, therefore, the reanalysis driven simulations are not expected to provide better results than a dynamically coupled atmosphere-ocean model.

L 410: of the severe cyclonic storm
Corrected

Fig 7: hard to see the lines against the dark colours.
The lines are now made thicker to make these clearly visible in Figure 7.

Reference:

Pugh, D.T.: Tides, Surges and Mean Sea-Level, John Wiley & Sons, Chichester, 472 pp., 1987.

Responses to comments from Reviewer #3

Comment:
Figures 4 (atmosphere validation) 5 (SST) demonstrate so obviously the better performance of the coupled model over the uncoupled model that one could think of mentioning this more prominent (maybe also in the abstract). Especially the improvement in SST and extreme weather phenomena has also found in coupled modeling studies for totally different regions (e.g Europe, Baltic, Jeworek et al., 2017, Hagemann, et al., 2017, Gröger et al., 2015). However, I understand that the added value of coupling is not the focus of the present study. On the other hand this could make the study more interesting to research communities outside the Indian ocean and would likely increase the potential for referencing in future oceanoraphic literature. The publication is however acceptable as it is now.

Ho-Hagemann, H.T.M., Gröger, M., Rockel, B., Zahn, M., Geyer, B., Meier, H.E.M, 2017, Effects of air-sea coupling over the North Sea and the Baltic Sea on simulated summer precipitation over Central Europe, Clim Dyn,49: 3851. https://doi.org/10.1007/s00382-017-3546-8

Jeworrek, J., Wu, L., Dieterich, C., and Rutgersson, A., 2017: Characteristics of convective snow bands along the Swedish east coast, Earth Syst. Dynam., 8, 163-175, https://doi.org/10.5194/esd-8-163-2017.

Gröger M, Dieterich C, Meier HEM, Schimanke S (2015) Thermal air-sea coupling in hindcast simulations for the North Sea and Baltic Sea on the NW European shelf. Tellus A Dyn Meteorol Oceanogr 67(1):26911. doi: 10.3402/tellusa.v67.26911

Response:
We sincerely thank the anonymous Referee for this suggestion. We have added following sentence in the Abstract,
'The coupled model found to improve the sea surface temperature over the uncoupled model.' In lines 9-10 of the revised version.

The suggested references are added in line 55 of the revised version and included in the References list.