Responses to the comments received from Anonymous Referee #2 on os-2017-83

Our point-by-point responses are given in italic blue font following each comment.

Comments for the manuscript "Estimation of oceanic sub-surface mixing under a severe cyclonic storm using a coupled atmosphere-ocean-wave model"

General comments
This study presents a case study using a coupled atmosphere-ocean-wave model to investigate the influence of the very severe cyclone storm Phailin on mixing in the upper oceanic layers over the Bay of Bengal. An advantage of coupled model, i.e. interaction and feedback between component models, was utilized and mentioned in the study, however, was not highlighted as it should be. For instance, there is a lack of an analysis for wind speed and wind direction simulated by the atmospheric model which is an important factor to the mixing and kinetic energy in the ocean during the storm. A case study for one storm event and a time series analysis for only one location in Indian Ocean does not seem to be able to provide robust conclusions. However, the topic is interesting and the introduction provides a good overview about the topic. Therefore, I suggest accepting the paper for publication after major revisions are made.

We thank the anonymous Referee#2 for the constructive comments on the manuscript. As suggested by the Referee, we have now highlighted the advantage of using coupled model in better interaction between atmosphere and ocean and, therefore, better simulation of sea surface temperature and oceanic sub-surface features. A figure showing the validation of atmospheric model simulated wind speed, direction, and surface pressure is now added in the manuscript. A panel showing the wind speed simulated by the atmospheric model is included in the figure where the time series of temperature profile, u- and v- currents, and kinetic energy are shown (Figure 4 of Discussion paper, Figure 6 of Revised paper). As suggested, we have added one more location (on the track) for the time-series analysis in addition to the previously selected off-track location. The figures and text are modified accordingly in the revised manuscript.

Major remarks
- Abstract contains too many details which should be moved to the conclusion. Especially in the abstract as well as in the introduction, a clear statement is missing of what is new in this study.

As suggested by the Referee, we have moved the details to the conclusion section. An statement highlighting the novelty of this study is now included in the Abstract (lines 12-14) and Introduction (lines 122-127) in the revised manuscript (with track change).

- Configuration (i.e. horizontal resolution, vertical levels, integration time step, etc.) of used models should be provided.

The details of model configuration including horizontal resolution, vertical levels, integration time step, etc. are now provided in the revised manuscript (Lines 204-213).

- What is the reason for choosing only one location for time-series analysis? Is there any observation data at this location that can be used to compare with the simulation?

We have now added another location on the track of cyclone and compared results with the existing off-track location. However, the larger kinetic energy and mixing were found at the off-track location as compared to the on-track location. There was no observational data at the off-track location but the selection was based on the maximum surface cooling observed

- An evaluation of wind speed and wind direction simulated by WRF is missing although it's relevant for the analysis of D23, MLD, etc.

We agree with the Referee suggestion. A new figure (Figure 4 in revised manuscript) added to show the validation of the WRF simulated wind speed and direction with the buoy BD09 measurements.

- The simulated storm track of the stand-alone WRF for this event should be mentioned in the current section 3.2. And how is the performance of stand-alone ROMS in simulating mixing during the storm? One can ask whether the expensive coupled model is really necessary to simulate such event.

The stand-alone WRF model was found to simulate Phailin track almost similar (figure shown below but not included in the paper) to the WRF in coupled configuration. However, the intensity (surface wind speed) in WRF stand-alone model was higher as compared to the coupled model (Figure 4 of the revised manuscript). The WRF in coupled model configuration shows better performance in simulating the surface wind speed and pressure during Phailin. The exchange of wave parameters with the WRF model in coupled configuration provides realistic sea surface roughness that resulted in improvement of surface wind speed (included in section 3.1 of the revised manuscript).
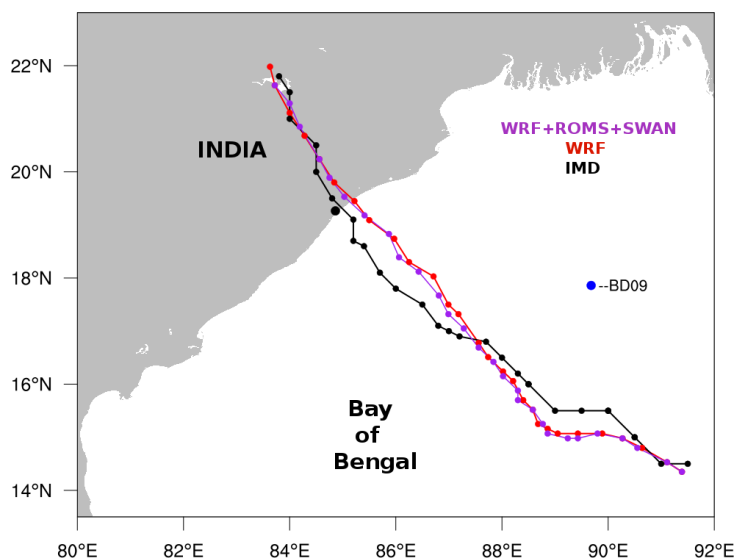
Figure: Comparison of Phailin tracks simulated by stand-alone WRF model and coupled model with the IMD reported track.

The stand-alone ROMS model forced with the WRF winds in un-coupled mode overestimates the cyclone-induced cooling with -2.2 ºC bias in SST on 13-14 October. The stronger surface winds in stand-alone WRF cause the larger cold bias in stand-alone ROMS model. The SST comparison figure now includes stand-alone ROMS model SST as well (Figure 5 of the revised paper).

- Simulated SST should be analyzed in more details. Although the cooling was captured well during 12-14 Oct but obvious biases occur in 10 and 11 Oct. What is the reason for the biases?

The coupled model captures the SST spatial pattern reasonably well with about -0.5ºC bias in northwestern BoB on 13-14 October. This order of bias in SST could be resulted from the errors in initial and boundary conditions provided to the model. The biases on initial days of

10-11 October are due to the biases in ECCO2 data used to initialize the model. These points are now mentioned in lines 313-315 of the revised manuscript.

- Conclusion section: for which studies the results of this present study can be applied? Please give examples!

The coupled model found to be a useful tool to investigate air-sea interaction, kinetic energy propagation, and mixing in the upper-ocean. The proper representation of kinetic energy propagation and oceanic mixing have applications in improving the intensity prediction of cyclone, storm surge forecasting, and biological productivity. These points are included in the conclusion section of the revised manuscript.

- There is a lack of references at some parts of the manuscript, for instance for the data sets ECCO2 and ETOPO2 (lines 152, 153) or for the periodogram and Morlet wavelet methods (lines 160, 161).

References are now added for ECCO2, ETOPO2, periodogram, and Morlet wavelet methods.

- The English needs strong improvements. I suggest proof reading by a native English speaker.

We have thoroughly checked the manuscript for any English language or grammatical errors and corrected the same.

Minor Comments
- It's not necessary to use the abbreviation VSCS prior to Phailin.
The abbreviation 'VSCS' prior to Phailin has been removed.

- Line 40-41: what are other important factors to drive the ocean response to the tropical cyclone?
The sentence has been modified to make it clear (in lines 53-54 of the revised paper).

- Lines 72-79: The information does not seem to be important for the present study.
These lines are deleted.

- Citation rule of the journal was not kept in line 95, 98.
Corrected.

- Line 118: The simulation time period should be more specifically described, for example "period of 00 GMT 10 October – 00 GMT 15 October 2013" as "period of 10-15 October 2012" can be understood that the whole day 15 October is also simulated which is not the case in this study.

Corrected as suggested by the Referee.

- Line 164: where is Ef defined?
Ef denotes the inertial baroclinic kinetic energy, defined in line 261 of the revised manuscript.

- Section 3.1 is not a result of this study. It should be moved to the line 81, after the storm Phailin is mentioned.

Agreed, the write-up is now moved to lines 110-119 in Introduction (revised manuscript).

- Line 271: how to define the rotary spectra of near-inertial wave numbers? Either an

equation or a reference is necessary.

The wave-number rotary spectra provides a clear picture of wind energy distribution in the sub-surface water, which is used in the present study for the near-inertial oscillations. References are now added to the rotary spectra in line 448 in the revised manuscript.

- Some sentences are not clear: lines 57-60, 238-242, 247-249. Please rewrite them.
Rewritten the sentences to make them clear in lines 79-83, 392-397, 406-410, respectively.

- Line 280-284 should be moved to the conclusion and discussion.
The lines are moved (with suitable modifications) to the conclusion section.

- Too many colors shading steps are used in the figures, please use maximum 15 colors in each figure. For most of the respective panels using half of the number of steps is recommended.
Corrected as suggested by the Referee.

- Caption of figure 1: "is" is missing between "analysis" and "marked".

Caption of Figure 1 (Figure 2 in revised version) is corrected.

- Figure 2: It would be good to display the lifetime of the storm (daily should be sufficient) with colors of dates corresponding to the tracks. It will be helpful to see when and where the storm was generated and vanished.
The lifetime of the storm is now indicated in the figure (Figure 3 in the revised version).

- Caption of figure 2: "Validation" is not necessary here. Can change to: "The Phailin track simulated by the coupled model (black) and the IMD reported track (red)."
Corrected. (Figure 3 in the revised version)

- Caption of figure 3: Sequence of upper and lower panels should be switched. For instance: "Daily SST (oC) simulated by the coupled model (upper panel) and observed from the AVHRR satellite (lower panel)."
Corrected as suggested. (Figure 5 in the revised version).

- Caption of figure 4: "(d)" is missing
Corrected. (Figure 6 in the revised version).

- Figure 5: which time period is covered?
Figure caption is modified to make it clear. The power spectrum analysis was performed on the simulation period (10-14 October). The frequency ranges of near-inertial oscillations (f) and semidiurnal tidal constituent (M2) are shown with the vertical lines in the figure. (Figure 7 in the revised version).

- Figure 6: where is the white dashed line?

There was no white dashed line in the figure. Now the caption is corrected. (Figure 9 in the revised version).