Title: Impacts of a large extra-tropical cyclonic system in Southern Brazilian Continental Shelf using the COAWST model

Construction and Building Materials

Dear Editor and Anonymous Referees

The authors thank the reviewer #1 for the suggestions regarding the submitted manuscript. All comments were carefully considered by the authors and applied to the manuscript. The following pages show the changes performed. Comments from the reviewers are presented followed by the response from the authors and the changes performed in the manuscript.

Sincerely,

The Authors

Revision of the manuscript:

This study uses the COAWST model to study the influence of an atmospheric frontal system to the coastal ocean. The frontal system is associated with an extra-tropical cyclone. The model is based on the ocean circulation model (ROMS) and the weather forecast model WRF using nested grids.

The use of coupled modeling systems (COAWST) to study the ocean and atmosphere physical processes at mesoscale and sub-mesoscale has previously been justified in studies by some authors. Gronholz et al. (2017), showed that exchange processes generate notable changes to ecosystem modeling, since the coupled ocean-atmosphere model can better determine coastal circulation, the development of biological processes after a storm surge or simulate sedimentary transport over a continental shelf. In addition, the atmospheric circulation components of the coupled model are able to simulate local circulation patterns and exchange processes much more effectively (Cocke & Larow, 2000). The COAWST coupled model has been widely used in studies of cyclogenesis and storm tracks over the ocean and continental shelf. Authors such as Olabarrieta, et al. (2012), Zambon & Warner (2014), Liu et al. (2015), Du et al. (2015), Chen et al. (2017), Denamiel et al. (2019), and Reffit et al. (2020), were fundamental for choosing this model. The icing on the cake here is the ability of COAWST to simulate energy transfers from atmosphere to the ocean on a regional grid.

The paper presents some model-data comparison for an event happened in Sep. 2016 and shows the area's sea level anomaly which the authors conclude that it is "probably" due to coastally-trapped waves with a propagation speed of 480 km/day northward.

We believe that there was a misconception in writing, where the word "problably" should be replaced by "we suggest". Currently, there are few works with coastal trapped waves on the Brazilian coast, being recent and not conclusive. Due to the absence of an effective oceanographic observation network using buoys, tide gauges and drift data, we do not have access to a greater amount of in situ data. These data are essential for understanding the dynamic processes over the continental shelf and computing parameters of these special waves. Thus, we suggest that the observed and modeled free surface variations and wave propagation, associated with low frequency values of storm surge, close to observed in other manuscripts, such as: Wang & Mooers (1977); Schumann & Brink (1990); Camayo & Campos (2006); Schlosser, et al. (2019).

From what I read, the paper looks like a modeling practice and the results are presented without a clear discussion of the useful dynamics in detail with confidence. In fact, no quantitative analysis to the model output is done. The sea level anomaly should have been analyzed before concluding it is coastally trapped waves and if the propagation speed matches theory.

We appreciate the reviewer's suggestion, we added some paragraphs that collaborate with the reviewer points.

The figures are of low quality and captions not uniform (some are clear and the others are not).

We appreciate the reviewer's suggestion, and we will improve the quality of the images.

Some examples of problems, among many others, are this comparison showed a correlation higher than Line 28: "78% between sea level rise data and the model results," - sea level rise data? Isn't this a work for an extratropical cyclone that only lasted for a few days? Why was the short time event related to sea level rise which would be a climate data defined to be 30 years or longer. I guess the authors meant water level data.

We understand the reviewer point, whereupon "sea level rise" is predominantly used in studies of mean sea level, associated with climatological events. We appreciate the reviewer suggestion and we changed it in the manuscript.

Figure 1 is out of the context - it maybe useful for a conference for background but not needed as they have nothing to do with the dynamics and the coastally-trapped waves.

We appreciate the reviewer's suggestion and remove figure 1.

Figure 4, poor quality

We appreciate the reviewer's suggestion, and we will improve the quality of this image.

Figure 6, Caption is too brief and unclear.

We appreciate the reviewer's suggestion, and we will adjust the figure caption.

Figure 7, poor quality - but the presentation is odd - I would prefer to see direct comparison between model and data, not separating the tidal and non-tidal parts. If you need to separate them, put the data and model in the same frame and include quantification of statistics (e.g. correlation or R2 value).

The authors follow the reviewer's opinion and change the figure to a direct comparison between model and tide gauge data, not separating the tidal and non-tidal parts.

Lines 456 and 457: "The physical mechanism that explains the force of the coastal-trapped waves over topography is straightforward,..." then the authors basically referred to some previous papers and finishes their analysis for the coastally-trapped

waves. I would not call this paragraph an analysis. The work is not done with quantified analysis and any new finding in dynamics.

We added two paragraphs to improve the process ideas generated by low frequency waves under the influence of a storm surge. The use of literature to justify our results is due to the absence of a more in situ data and we tried to identify results observed in other authors to prove the effectiveness of our statements.

Similar problems exist for analysis around Figure 9. Figure 9 is presented in an odd way as well. It is not helpful in providing a clear picture.

The authors understand that the figure is simple, however the methodology accompanies other manuscripts, which used similar figures to visualize the propagation of the wave over time. Authors such as: Horsburgh & Wilson (2017); Brown et al. (2011); Brown, et al. (2013); Choi et al. (2013); Xie et al. (2016); Chen et al (2017); Song et al. (2020);

For the discussion around Figure 10: why the left panel has no tide while the right panel has tide? What is the reason to not include tide for the along shelf 50-m contour line transect but do include tide for the cross shelf transect?

Figure 10 shows the propagation profile of the low frequency wave, generated by the storm surge. In this way, we remove the tidal component to follow the wave propagation along the southwest coast of Brazil. Figure 10b is presented with the tide component to identify the influence of the low tides on the free surface level of the output model. This explains why the flooding on the Florianópolis island was observed in less than 24 hours. Although the elevation caused by the meteorological tide has a longer period, the M2 tide component is responsible for the semi-diurnal suppression of the relative sea level.

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