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 would like to thank reviewer #2 for the suggestions regarding the submitted manuscript. All comments were carefully considered by the authors and applied. His particular corrections were fundamental for the improvement of the manuscript. Reviewer's comments are presented, followed by the response from the authors and the changes performed in the new version of the manuscript.

Sincerely,

The Authors

Revision of the manuscript:

This manuscript deals with the impact of an extra-tropical cyclonic system in the Southern Brazilian Continental Shelf using numerical modeling. Although this is an important topic of study, this manuscript presents some problems that do not allow a dynamical understanding of the problem. The main problem of the manuscript is that the numerical simulations present results that do not well represent the observations. For this reasons, mainly, I suggest the rejection of the manuscript. Below, I present in more details the problems that I found:

-Line 69: By shelf waves, the authors mean Continental Shelf Waves?

Yes, we carefully make it clear by adding Continental Shelf Waves in this sentence.

-Line 132: Brazilian Current or Brazil Current?

We appreciate the reviewer's suggestion. It now reads "The Brazil Current".

-Lines 150-153: There are no references regarding the tidal amplitudes.

References have been included: (Truccolo et al., 2004), (Pennings et al., 2012) and (Benavente, et al. 2006; Maia, et al. 2016)

-Line 159: How near the coast of Brazil?

*It now reads: "*Explosive cyclogenesis over the southern Brazilian coast are the main causes of storm surges and occur throughout the year, but mainly during winter months."

-Lines 162-163: What do the authors mean by oceanic mesoscale gyration? And why does it cause accumulation of water in the coast?

We adjusted the citation by Pugh (1987). Now it reads: "The main mechanism responsible for transferring energy from the atmosphere to the ocean is through the friction of the winds on the sea surface (Pugh, 1987), which associated with the Coriolis Force and Ekman transport mechanisms, can induce the water accumulation on the continental shelf."

-Lines 164-166: The authors could give some examples of natural disasters and extreme events in Brazil to put the manuscript in perspective.

It has been added (lines 101-104 and line 166) to meet the reviewer's request.

-Line 174-175: Models do not provide data.

We accept the reviewer's suggestion and remove this sentence.

-Lines 180-181: The authors mention the importance of models in understanding coastal processes but do not mention any of these studies.

We appreciate the reviewer's point. We added some examples of existing studies on this topic.

-Line 250 (Data validation): The authors did not validate the data in this section.

We change "Data validation" to "Model output data analysis".

-Line 408: The authors present a correlation coefficient (Pearson's) of 0.78, but no significance level. I don't believe this correlation coefficient for SSH low-frequency variability is satisfactory. Other studies dealing with similar problems present much higher correlation coefficients for the low-frequency variability (e.g., Costa et al., 2019, Khalid et al. 2020, Ruiz et al., 2021). Why not computing some other statistical parameter that deals with the comparison between actual values and not the variability alone? (e.g., Willmott, 1981)

The use of Pearson's coefficient was due to its ability to measure the correlation (positive or negative) in a specific predefined interval. In this way, the method proves to be effective in comparing harmonic oscillations recorded in the low-frequency component. It assists in the identification of a possible delay of the model in relation to the tide gauge. We *believe that correlation coefficients such as Pearson or Skill are accepted scientifically and can present satisfactory results. We appreciate the reviewer for his observation. We identified that an error occurred in the writing of the calculated correlation coefficient. The calculation was redone, and the correct value is 0.87, which is close to what has been observed by Costa et al. (2019), Khalid et al. (2020), and Ruiz et al. (2021).*

Ruiz et al. (2021) explain in their's work: "The sub-inertial response to wind forcing is trapped in the continental shelf; sub-inertial elevations and currents variability are higher at the coast than at the shelf break (Dottori and Castro, 2009). The inclusion of the remote wind stress as forces increase the correlations between observed and modeled sub-inertial currents in the central and northern parts of the South Brazil Bight (Dottori and Castro, 2018)." In our study area, we observed that there is a change in the direction of the coastline, which induced a delay of 3 hours in the propagation of the sub-inertial wave. (Figure 6 updated). Another critical factor in reducing the correlation coefficient of the low-frequency wave is that the pre-frontal winds were more intense in the model's outputs compared to the weather station. It explains the lower levels observed in the numerical simulation.

-Page 15: The figures should present data and model in the same panel.

Figures have been modified. We gathered the data from the tide gauge model.

-Lines 509-511: The authors should present a figure with the locations mentioned in the text, as well as the with of the continental shelf in the domain.

The analyzed stations are mentioned in the text (Lines 509-511) and are shown in Figure 1 through the start icon. We appreciate the reviewer's suggestion and add the bathymetry dashed lines to mark shelf break in the child grid of Figure 1.

-Line 512: It is hard to see the agreement between data and model. The authors should provide a qualitative method and a better figure.

We understand that the figure is objective and straightforward; however, the methodology accompanies other manuscripts, which used similar figures to visualize the propagation of the wave over time. The authors understand that the more robust quantitative analysis may better represent the characteristics of the model's shelf wave. However, the absence of a greater number of observational data could assist in the comparative study of the data. Thus, we found other authors who used similar statistical analyzes 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); Khalid et al. (2020) and Ruiz et al. (2021), explaining its results with images such as figure 8 (Updated).

-Line 516: The delay of few hours is considerably high for these processes. These differences can lead to substantial errors in SSH prediction that do not allow a good understanding of the phenomenon. For instance, if astronomical tides and storm surge peaks are coincident, storm tides can occur. A delay of few hours in one of these processes can lead to wrong predictions.

We agree with the reviewer's observation. Studies like this are fundamental for a better understanding of coastal processes in the Southwest Atlantic Ocean. Although our results show differences between the simulated output and in situ data, the values are close to the real ones, considering the complexity of the parameterization that modulates the ROMS and WRF models.

-Lines 552-562: This part of the manuscript looks more like the Introduction.

This text fragment presents other works that have similar previous results. Due to the absence of an effective oceanographic observation network using buoys, tide gauges, and drifters data, we do not have access to an optimum amount of in situ data. These data are essential for understanding the dynamic processes over these particular continental shelf waves and their characteristic parameters. Thus, we suggest that the observed and modeled free surface variations and wave propagation associated with storm-derived low-frequency values agree with other published works. For this reason, one of our authors suggested that we finalize the manuscript results by showing other works that observed similar atmosphere and oceanic conditions to ours. We believe that this paragraph is essential to conclude our ideas.

We agree with the reviewer and replace the fragment in introduction.

-Line 566 (Conclusions): For the reasons given above, I believe the conclusions are weakly proved. In essence, the conclusions are very superficial and do not contribute substantially to the knowledge of the process in the region.

We are grateful for the reviewer's suggestion. They were fundamental to reshape our manuscript. We edited our conclusions in order to improve our arguments and the manuscript quality.