

RESPONSE TO REVIEWERS

Please note that in this rebuttal, *italics* refer to the text of the reviewers' comments, our detailed response is in black, the new text of the revised version is in **bold blue**.

REVIEWER #2:

GENERAL COMMENT:

The article presents and application of flow of wave modelling nearshore Calabaia considering different sea level rise values and offshore wave conditions. I find the application shown in the article interesting but the validity of the applied models and the rationale behind the considered conditions lacking. As a whole the article needs restructuring, proof- and critical-reading. There are a few buzzwords being applied in a non-coherent way. For instance, what is the meaning of “global risk society” and “Climate change drives potential future sea hazards, as the greenhouse effect is expected to lead to global warming”? Furthermore, a lot of what is stated in the abstract and introduction can be removed as they are not directly related nor motivating the contents. I suggest that the authors rewrite the whole article. Below a few questions and suggestions. The list is not comprehensive, but at least these issues should be addressed before the article can be assessed.

We thank the reviewer for his/her constructive criticism. We seriously considered his/her suggestions, amended some points, and further improved the manuscript. Some parts of the paper have been rewritten and/or moved to improve the whole readability. Detailed answers are reported below.

- 1) *Line 14: What is the innovative aspect? Not the coupling between waves and hydrodynamics nor considering the effects of SLR on coastal waves and loads, for these you can find many references.*

The innovative aspect is the coupling of 2DEF and SWAN models. This coupling system optimize the computational cost by running SWAN at the same time step of the boundary condition data.

- 2) *Lines 17-18: If you are considering projections then it should be the “projected impact” or “impact projections”.*

Fixed, thank you for noting.

- 3) *Replace “Sea waves, caused by the effect of local wind climate” with “Sea or wind waves, forced by the local winds”.*

Fixed, thank you.

- 4) *What do you mean with “Interaction between sea and swell waves can cause unpredictably high waves”? How and why is it unpredictable?*

In the new version of the manuscript, we deleted this confusing sentences.

- 5) *Can “Extreme sea stormy conditions” be replaced with “Coastal extreme storms”?*

Yes, thank you.

- 6) *Lines 36-38: Please clearly state what you mean with: “sea storms”, “sea waves” and “swell waves”. How do you refer to (sea+swell) wave conditions?*

In the new version of the manuscript (lines 23 – 25) we clearly stated the meaning of the three terms:

Hereinafter, we refer to wind waves the waves triggered by the local winds, to swell waves the waves moving inshore from a distance, and to waves (i.e., sea waves) the superimposition of wind and to swell waves.

Thank you for the advice.

- 7) *Lines 76 to 83 can be removed as they do not contribute to the subject.*

Fixed. Thank you.

- 8) *Line 95: “Sea level and surface currents are driven by sea state,” this is not true. Please rephrase or expand acknowledging that sea levels and currents are driven by atmospheric and astronomical forcing.*

Fixed. Thank you.

- 9) *Lines 99-101: Please rephrase or expand. The Gumbel distribution could in principle be used to model the annual maxima, not to “identify the extreme sea wave conditions.” By “sea wave” you mean only wind sea?*

Fixed. Thank you.

- 10) *Lines 101-102: I assume that you mean ERA5 and not ERA-interim as it does not cover the full period given. Assuming that it should be ERA5, the quality of the ERA5 data is expected to be lower in the 1950-1979 period. Have you checked whether there are inhomogeneities in the ERA5 data before and after 1979?*

Thank you for noting the misleading term used in the former version of the manuscript. The homogenization of the data has been checked in our former work (Lo Feudo et al., 2022), cited in the manuscript.

- 11) *Line 140-141: Define “wave currents”. Are they only in the wave break-zone? Do they exclude direct wind and pressure forcing?*

In the new version of the manuscript, we wrote (lines 116 – 117):

...with a negligible interaction between tidal currents and longshore wave currents.

- 12) *Line 147: The ERA5 data are available hourly, why do you only consider 6-hourly?*

For climate studies, a 6-hourly dataset is the best compromise to produce a reliable climate analysis and the storage-cost.

- 13) *Lines 201-202: What do you mean with “model is forced by imposing (refer to Fig. 3a): (i) wave climate at the seaward boundary section (yellow dashed line) every 30 minutes”. By climate I understand long-term means or return values and these are generally assumed stationary, not changing every 30 minutes.*

In the new version of the manuscript, we refer wave height, period and direction to “wave characteristics” and wave climatology to “wave climate”. We corrected all these terms thorough the manuscript accordingly. Thank you for noting.

- 14) *Lines 144-146: I find the description of Lothar incorrect. Can you please check your references or https://en.wikipedia.org/wiki/Cyclone_Lothar?*

We checked but unfortunately we did not find any misleading description of Lothar.

- 15) *Line 258: Why “CFS reanalysis” and not ERA5, as you are using ERA5 in the study?*

In the new version of the manuscript, we replaced CFS with ERA5 reanalysis panels. Thank you for the advice.

- 16) *Section 2.6 should be significantly reduced, most of it is copied from the IPCC report. Just refer to it and state the values that will be considered further.*

In the new version of the manuscript, Section 2.6 has been significantly reduced.

- 17) *Lines 245-247: Please rewrite. It is not clear from the text that the models use different numeric schemes and which. Is also confusing to be faced again with “climate”, what do you mean?*

In the new version of the manuscript, we better stated the model setup thorough the manuscript. Thank you for noting.

- 18) *Section 3.1: The return value estimates are not used further. Why are they presented? To motivate the range of values given in line 395?*

The reviewer is right. We presented the return values to motivate the further synthetic analysis.

- 19) *Section 3.2: In this section the 2DEF+SWAN results are validated by comparing them with the results of another model and none of the models are validated against observations. The validity of the model results is therefore not verified. The only conclusion/aim appears to be to show that the results are comparable and running times of 2DEF+SWAN lower.*

We agree with the reviewer. However, the case study is, one hand site-specific. On the other hand, since at Calabaia Beach the morphology is very simple, this case study can have general application. Moreover, both SWAN and 2DEF (and MIKE) models and have been widely tested over decades (we provided some references thorough the manuscript). Finally, we highlight that the target of this work is to compare the two methodologies (SWAN+2DEF and MIKE modelling systems), ensuring that the results provided are similar on order to verify the effectiveness of the coupling system.

20) *Section 3.3: There are two sections 3.3. Please correct.*

Fixed, thank you for noting.