



EGUsphere, referee comment RC1
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Comment on egusphere-2022-508

Anonymous Referee #1

Referee comment on "Impact of wave-water level non-linear interactions for the projections of mean and extreme wave conditions along the coasts of western Europe" by Alisée A. Chaigneau et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-508-RC1>, 2022

Review of "Impact of wave-water level non-linear interactions for the projections of mean and extreme wave conditions along the coasts of western Europe" by Chaigneau et al.

The aim of this paper is to assess the impact of including time dependent water level variations in the external forcings of a regional wind-wave model of the IBI domain (i.e., an area of the northeastern Atlantic including the Iberian-Biscay-Ireland regions) on the wave setup estimated from modelled mean and extreme sea state statistics.

Two configurations of the same regional wind-wave model differing only in the water level forcing (i.e., one uses it while the other one does not) are compared. Numerical experiments include hindcast integrations covering the period 1950-2014 and projected simulations spanning the period 1950-2100 under two climate change scenarios.

I believe the idea behind the study is intriguing and of interest for the broad environmental-scientific community and I think the topic of the manuscript is relevant to Ocean Science journal. However, I do have few major concerns listed below that I think the authors should address before the study could be suitable for publication.

General major comments

1. Methodology:

- As stated by the authors (L60-61), “wave characteristics used to estimate wave setup are sensitive to water level changes in shallow waters, where waves interact with the ocean bottom.” From section 2.2.4 I understand that the wave model considers water-level variations only for the wave propagation (i.e., group velocity and wave number) while “coastal (depth-induced) breaking is not included” in the model (L92-95). My concern is that by not including the depth-induced wave-breaking the authors are missing a fundamental depth-dependent process, which can have a first order effect on the wave statistics in shallow water and hence on the wave setup. In addition, as the authors also explain in the introduction (L35-36), the wave setup is in fact due to the depth-induced wave breaking. So what I cannot understand is how the authors can assess the impact of water-level variations on the wave setup if the leading order physical mechanism driving the wave setup is not included in the model. I think the authors should carefully address this point in their manuscript.
- When assessing the impact of water-level forcing on the wave setup at a domain level the authors report an impact in few coastal locations. My doubt is: how much can we trust these results given the 6m minimum depth approximation?
- Also, could it be that the authors found a generally small (very few locations) impact because the depth-induced breaking is neglected and the minimum depth approximation is applied?

2. Validation:

- The title of section 3 is “Validation and projections of IBI-CCS-WAV, without waves-sea level interactions” and in fact the figures of this section report data only for IBI-CCS-WAV. However, at L259-261 the authors state that “The ability of IBI-CCS-WAV and IBI-CCS-WAV_ssh to reproduce observed distributions is assessed for the mean state and the 99th percentile of the significant wave height and peak period since these variables are then used to compute the wave setup scaling”. Is the IBI-CCS-WAV_ssh validated as well? If not (as I believe is the case), then I think the authors should also include the validation for the experiment using water-level forcing since, apart from the impact on the wave setup, it is also interesting and useful to know for the wave modelling community whether including this forcing can help to improve the accuracy of the model.

3. Manuscript structure:

- In general, I think the structure of the paper should be substantially improved before being suitable for publication. Below, a list of possible changes:
- Section 2: this section is quite confused and not logically structured in my opinion. I would first move L101-112 as in intro of Sec. 2, improving the text and Fig 2 (the colours are too weak). Then, I think the authors could

a) describe the numerical wave model (sec 2.1), avoiding the references to global and regional simulations (e.g. L85), since I think can confuse the reader.

b) describe the regional wave configuration IBI-CCS-WAV (sec 2.2): this is the real focus of this paper, all the other models are used to force this model in my opinion. In addition, I would move L185-190 at the beginning of this section just to state at the beginning what is the aim of this model.

c) describe the external forcings (sec 2.3) with three subsections:

*) Atmospheric forcing (sec. 2.3.1), describing and validating (L138-152) CNRM-CM6-1-HR model and the fields used to force IBI-CCS-WAV. Also, please avoid the acronym GCM which is typically used for General Circulation Model instead.

*) Hydrodynamic forcing (sec 2.3.2), describing IBI-CCS and the fields used to force IBI-CCS-WAV.

*) Wave forcing (sec 2.3.3), describing CNRM-HR-WAV and the fields used to force IBI-CCS-WAV.

d) Inclusion of water level variations in the regional wave model: IBI-CCS-WAV_ssh (sec 2.4)

e) Wave setup calculation (sec. 2.5): Please check the definition of the wave setup scaling – there is a delta in the definition (L243) that I think should not be there.

- Section 4: I would first describe the impact on the entire coastal domain and after on the specific locations. Also, I think the authors should clarify better what is the rationale behind the choice of those two specific locations. Why not for example the Bristol channel? The tidal range there is almost as large as in Mont-Saint Michel. Also, I would rewrite Sec. 4.2 and 4.1 (which are the most important sections in my opinion), trying to discuss more in depth what is the impact and to contextualise it, maybe moderating a bit the wording (e.g., “highly impacted”) which I think it is not fully reflecting the results of the authors.
- I would describe a bit better in the Conclusions and Abstract the limitations of your study.

Specific comments

- L13: you don't need the acronym EWL here, since you don't use it anymore in the abstract.
- The authors may want to add some references at L170 – 173. Here I am suggesting some possible references for the North Atlantic (which is the area I am more familiar with): the Atlantic coasts are subject to very energetic events in terms of significant wave heights, wave periods and energy flows (e.g., Masselink et al. 2016, Bruciaferri et al. 2021) whereas the Mediterranean Sea and North Sea are more sheltered areas. In addition, the zone also contains very different tidal regimes with both macro and micro tidal regimes respectively in the English Channel/Celtic Sea (Valiente et al. 2018, Stokes et al. 2021) and in the Mediterranean Sea.
- L219-220: "Limitations related to the use of parameterizations have been extensively discussed in Melet et al., 2020" -> can the authors do a summary of those limitations here so that the reader is aware?
- L224: please define "foreshore".
- L355: Figure 7 illustrates "projected **changes**" -> changes respect to what? Please clarify
- L422-425: please rephrase it.
- L453: the most significant impact -> quite strong wording, you have an impact (not so strong) only in one location out of two in Fig. 10.
- L461: however small -> to me seems nihil
- L484-485: I would be careful here. If what the authors are saying is true, then why it is not valid everywhere, e.g. Mont-Saint Michel? Please clarify.

References

- Masselink, G. et al. 2016: The Extreme 2013/2014 Winter Storms: Hydrodynamic Forcing and Coastal Response along the Southwest Coast of England, *Earth Surface Processes and Landforms*, <https://doi.org/10.1002/esp.3836>
- Bruciaferri, D. et al. 2021 - The Impact of Ocean-Wave Coupling on the Upper Ocean Circulation During Storm Events, *Journal of Geophysical Research: Oceans*, <https://doi.org/10.1029/2021JC017343>
- Valiente, G. N. et al. 2018: Role of waves and tides on depth of closure and potential for headland bypassing, *Journal of Marine Geology*, <https://doi.org/10.1016/j.margeo.2018.10.009>
- Stokes et al. 2021: Forecasting coastal overtopping at engineered and naturally defended coastlines, *Journal of Coastal Engineering*, <https://doi.org/10.1016/j.coastaleng.2020.103827>