24.02.2023

Author's response:

Dear editor, dear reviewers,

We finally decided to re-run the simulations with the coastal depth-induced wave breaking included for the SSP5-8.5 scenario. For the answers to the main comments, the first reviews we sent are no longer relevant. Please find in blue the answers to your main reviews taking into account the new version of the manuscript.

Best regards,

The authors.

RC1:

Main 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.

The simulations for the SS5-8.5 scenario have been re-run with the coastal depth-induced wave breaking following Battjes and Janssen, 1978 formulation. In consequence, major changes have been made on the manuscript:

- The analyses of the SSP1-2.6 scenario have been removed
- The analyses on the wave setup have been removed and the focus in only on the significant wave height H_s and peak period T_p. Indeed, the wave setup was calculated with a parameterization based on offshore parameters H_s and T_p that cannot be considered as deep-water when the coastal wave breaking is included.

- 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?

Sect 2.3 has been revised to better explain the constraints on the bathymetry with a figure added: "For IBI-CCS-WAV, a minimum water depth of 6 meters was chosen to be consistent with that applied in the forcing from the regional ocean simulations. In the regional ocean model, this value avoids the occurrence of uncovered banks in macro-tidal areas, especially around Mont-Saint-Michel in France and in the Bristol Channel. For IBI-CCS-WAV, since the depth is set to a minimum of 6 meters (Sect. 2.2), values less than 6 meters in the tables are not used. For IBI-CCS-WAV_ssh, since the local depth fluctuates around that of IBI-CCS-WAV, values less than 6 meters in the tables are used, for example at low tide. In this case, the values can be used up to a minimum of 3 meters which corresponds to the first term of the geometric series used to discretize the tables (Fig. 2)."

Sect. 5.1 has also been revised to discuss limitations, including those associated with the minimum depth:

"**5.1 Model limitations**

The use of a single forcing climate model does not allow to quantify the uncertainties of the projected changes. Here, the focus of the study is not providing a likely range of wave projected changes over the IBI domain but rather the focus is process-oriented. In our study, the estimation of the impact of including hourly sea level variations in the wave model is limited by several resolution aspects. The first limitation is the horizontal resolution of the wave model. The model resolution of 1/10 ° (~10 km) is conditioned by the computational cost due to the length of the simulations needed to address the question of extremes on climate scales. It does not allow a very fine representation of the coastline and of the bathymetry in the coastal zones. Moreover, the regional ocean model (Tab. 1 and Appendix A) used for the surface currents and sea level forcing does not allow for dry areas. Therefore, a minimal bathymetry is set to 6 m to run the ocean model with tides (Chaigneau et al., 2022). We chose to apply the same minimal bathymetry of 6 m in the regional wave model to maintain consistency between both regional ocean and wave models. In fact, because it would have been unrealistic to have a bathymetry of 1 m within a 10 km grid point, the minimum bathymetry (6 m) also allows to maintain a realistic balance between the 10 km horizontal resolution and the water depth. This results in fewer areas of shallow and intermediate water in the wave model and thus less effect of sea level variations on the waves. The implementation of "wetting and drying" (O'Dea et al., 2020) allowing for dry areas in NEMO version 4.2 should improve this limitation on the ocean model and therefore on the wave model. Another limitation is the resolution of the atmospheric forcing from the global climate model (Tab. 1). Given that winds are the major drivers of extreme wave events in our study, even with a relatively high-resolution climate model forcing, the resolution of 50 km for the atmospheric drivers implies that generated waves are more representative of a large-scale forcing than of coastal processes.

For all these reasons, the estimates provided in this study only partially represent the processes responsible for the non-linear interaction of sea level on waves and the results found in this study are not representative of any purely local situation at the coast but rather provide regional information. A second step of dynamical downscaling at higher resolution would be necessary to overcome such resolution limitations."

- 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?

With the coastal depth-induced wave breaking included, the impact of the sea level variations on waves is indeed larger than in the older version of the manuscript.

2. Validation:

- The title of section 3 is "Validation and projections of IBI-CCS-WAV, without wave-water 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.

The validation of the IBI-CCS-WAV_ssh simulation has been added only on the two scatter plots of Figure 4 and 5 which are the comparisons to wave buoys. A sentence has been added before Sect. 3.1.1: "In this section, it is rather IBI-CCS-WAV which is validated against the reanalysis because as IBI-CCS-WAV, the reanalysis does not consider hourly sea level variations as a forcing."

A sentence has also been added in Sect. 3.1.1 : "The IBI-CCS-WAV_ssh simulation is compared to the buoy data in the scatter plots (Fig. 3d,h) and Fig. 4d,h) but the performance of IBI-CCS-WAV_ssh is similar to that of IBI-CCS-WAV since the buoys are mostly located in deep waters (Sect. 4)." and in Sect. 3.1.2: "The focus is only on the IBI-CCS-WAV simulation since the two buoys are located in deep waters (Fig. 1a)."

3. Manuscript structure:

Thank you very much for this comment which has significantly improved the structure of the manuscript. Sect. 2 has been completely revised. To reduce the length of the paper, we moved some information from the downscaling methodology in an Appendix A as suggested by the editor, notably all the part on the external forcings (global climate forcing, wave forcing, ocean forcing). A table has been added in Sect. 2 to better explain the different simulations.

- 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 to 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 rational 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.

Sect. 4 has been rewritten. The impact on the entire domain is provided before the specific locations. The two specific locations are chosen in France as it is our country of interest here but in terms of processes, indeed, the Bristol Channel would also have been relevant for the effects of the large tidal range.

Some insights have been added in Sect. 4.2 to explain the observed impact of sea level on waves: "In both cases, it can be pointed out that the increase in wave height occurs at high tide. These results are in agreement with Lewis et al., 2019 and Calvino et al., 2022 who both showed a significant increase in wave height at high tide at a finer scale. In Calvino et al., 2022, this impact seems to be explained mainly by the effect of bottom friction, which is less important at high tide as the water column is higher. In the case of Arns et al., 2017, waves are higher when sea level increases (e.g. at high tide) because they break closer to the shore. In our case, additional analyses would be needed to understand which process included in the model (Sect. 2.1) is the most responsible for the non-linear interaction of sea level on significant wave height."

Another section has been added in the Discussion to discuss more deeply the results:

"**5.3 Implications of the results for coastal flooding**

The results obtained in this study have shown a large impact of sea level variations on extreme significant wave heights. Wind-waves and swell contribute to extreme sea levels at the coast via wave setup and runup (Dodet et al., 2019), combined with tides, storm surges and mean sea level changes. Marine flooding hazards cannot be quantified based on wave contributions alone but these contributions can locally partially enhance sea level changes at the coast (Melet et al., 2020). Our results show that extreme significant wave heights are strongly influenced by the effect of sea level on waves in coastal areas subject to large sea level variations or on wide continental shelves.

Depending on the region (wave regimes, sign of the extreme wave projected changes, local ocean processes involved, amplitude of projected changes in local sea level), the impact of the sea level changes on waves could be important to consider for present and future flooding hazards (e.g. for threshold exceedance calculations). For instance, future waves conditions and therefore coastal flooding could be affected in areas where large changes in tides are projected such as in the China Sea and Gulf of Saint Lawrence (Pickering et al., 2017; Haigh et al., 2019). Future extreme waves could also be significantly impacted in areas subject to large relative mean sea level rise, such as along the eastern coasts of the United States, the Gulf of Mexico and the Caribbean Sea where a rise of +1.4 m is expected by the end of the century under the SSP5-8.5 scenario (Fox-Kemper et al., 2021)."

I would describe a bit better in the Conclusions and Abstract the limitations of your study.

A sentence has been added at the end of the Abstract: "The estimates provided in this study only partially represent the processes responsible for the sea level-wave non-linear interactions due to model limitations in terms of resolution and processes included."

A sentence has been added at the end of the Conclusion : "Moreover, as the regional wave model does not have a very fine representation of the bathymetry, of the coastline and does not include the feedback of waves on sea level, the estimates provided in this study only partially represent the processes responsible for the sea level-wave non-linear interactions."

Specific comments

- 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.

Some references have been added: "the Atlantic coasts are subject to very energetic events in terms of significant wave heights, wave periods and energy flows (Masselink et al., 2016; Bruciaferri et al., 2021) whereas the Mediterranean Sea and North Sea are more sheltered areas dominated by wind waves (Chen et al., 2002; Bergsma et al., 2022). 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., 2019; Stokes et al., 2021) and in the Mediterranean Sea."

RC2:

Main comments:

I think this paper brings an interesting contribution to the field, but before publication I would like the authors to clarify the following major point:

- The model used does not include shallow water processes such as wave breaking (line. 93), and cannot represent important interactions with the seabed in shallow regions, as the minimum depth is 6m (line. 206). These shallow water processes, as stated by the authors in the introduction, are important for wave setup and set down; yet this work estimates the setup from data that exclude them. Can the estimation of the setup, calculated excluding important shallow water processes, be trusted? Is it a reliable approximation?

The analyses on the wave setup have been removed so the comments are not longer relevant.

Specific comments

Section 2 Methods: model and simulations: I find this section hard to read. I appreciate that all information required on the models are provided in section 2, and figure 1 is helpful to understand the simulation used, however it is easy to get lost in the nomenclature of the multiple simulations, and in the mere amount of information laid out. It may be worth considering simplifying the reader's work by adding a table containing a list of all simulations ran, including which forcing were used and the main details (resolution, period etc.) for each. This would improve the readability.

Sect. 2 has been completely revised. To reduce the length of the paper, we moved some information from the downscaling methodology in an Appendix A as suggested by the editor, notably all the part on the external forcings (global climate forcing, wave forcing, ocean forcing). As you suggested, a table has been added in Sect. 2 to better explain the different simulations.

Line 217-223: 'Therefore, at first order, wave setup and runup can be predicted via empirical formulations […] wave setup estimates are based on an empirical formulation (Stockdon et al., 2006).' This is the paragraph where you should convince the reader that runup estimation is reliable, despite the model's approximation. Please give more details on the parametrization limitations and how the empirical formulation you use affects results (i.e. what processes you are missing out). Explain why you think this first order approximation is good enough, even though you are not including sallow water processes.

The analyses on the wave setup have been removed so the comments are not longer relevant.

Line 259-261 '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 (Sect. 3.2, 4).' Has the IBI_CCS_WAV_ssh been validated? The section title seems to imply it hasn't ('Validation and projections of IBI-CCS-WAV, without waves-sea level interactions').

The validation of the IBI-CCS-WAV_ssh simulation has been added only on the two scatter plots of Figure 4 and 5 which are the comparisons to wave buoys. A sentence has been added before Sect. 3.1.1: "In this section, it is rather IBI-CCS-WAV which is validated against the reanalysis because as IBI-CCS-WAV, the reanalysis does not consider hourly sea level variations as a forcing."

A sentence has also been added in Sect. 3.1.1 : "The IBI-CCS-WAV_ssh simulation is compared to the buoy data in the scatter plots (Fig. 3d,h) and Fig. 4d,h) but the performance of IBI-CCS-WAV_ssh is similar to that of IBI-CCS-WAV since the buoys are mostly located in deep waters (Sect. 4)." and in Sect. 3.1.2: "The focus is only on the IBI-CCS-WAV simulation since the two buoys are located in deep waters (Fig. 1a)."

Section 5. Discussion: Important points are discussed, but I would strongly recommend adding a section on the implications of your results. For example:

The authors found an increase in the wave set up and a large impact on the wave-water level interaction in regions of extreme tidal range. In the introduction, the authors talk about coastal hazards and flooding during extreme water level to motivate the study. You cannot quantify hazards based on wave setup alone, but there is a lot to discuss. Considering that the tidal range will also be affected by sea level rise, are the regions where you predicted an increase in wave setup the same regions that are at risk from extreme wave events today? Are there other regions in the world that these finding could be relevant for (eg. Regions where the tidal range is expected to increase significantly)? The number of extreme events is also expected to increase in future, and your results show that these are periods in which the wave setup is particularly affected by the water-level changes; this could also be discussed. Which are the limitations of this study?

A section has been added in the Discussion to discuss more deeply the results:

"**5.3 Implications of the results for coastal flooding**

The results obtained in this study have shown a large impact of sea level variations on extreme significant wave heights. Wind-waves and swell contribute to extreme sea levels at the coast via wave setup and runup (Dodet et al., 2019), combined with tides, storm surges and mean sea level changes. Marine flooding hazards cannot be quantified based on wave contributions alone but these contributions can locally partially enhance sea level changes at the coast (Melet et al., 2020). Our results show that extreme significant wave heights are strongly influenced by the effect of sea level on waves in coastal areas subject to large sea level variations or on wide continental shelves. Depending on the region (wave regimes, sign of the extreme wave projected changes, local ocean processes involved, amplitude of projected changes in local sea level), the impact of the sea level changes on waves could be important to consider for present and future flooding hazards (e.g. for threshold exceedance calculations). For instance, future waves conditions and therefore coastal flooding could be affected in areas where large changes in tides are projected such as in the China Sea and Gulf of Saint Lawrence (Pickering et al., 2017, Haigh et al., 2019). Future extreme waves could also be significantly impacted in areas subject to large relative mean sea level rise, such as along the eastern coasts of the United States, the Gulf of Mexico and the Caribbean Sea where a rise of +1.4 m is expected by the end of the century under the SSP5-8.5 scenario (Fox-Kemper et al., 2021)."

Line 587, Impact of waves on sea level. Please discuss what this means in relation to your results: how would you expect the impact of waves on sea level to affect your results?

A sentence has been added: "More importantly, they also reported a large contribution of wave induced processes to sea level extremes which are up to 20 % higher on the European continental shelf due to these wave processes. By taking these processes into account in the ocean model, as the sea level would be higher, the impact on the wave model would be larger, meaning an increase in waves-sea level feedbacks."

Section 6. Conclusion. The main conclusion is not clear. I would rephrase it a way that answers your main aim reformulated as a question. For example, answer specifically: How is the sensitivity of historical and projected sea states for the IBI region coastlines affected by the non-linear interactions between wind-waves and water level changes, notably during extreme events?

The section has been rewritten.

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