Review of **“Impact of wave physics on ocean-wave coupling in CMEMS-IBI Part B: Validation study”** by Rainaud et al. (2019)

General comments

This paper aims to present the validation of a regional ocean-wave coupled system implemented in the Iberian Biscay and Ireland domain. The modelling system includes NEMO ocean model and two different versions of the WAM wind-wave model, namely MFWAM V3 and MFWAM V4. The authors compare three one-year long simulations: a reference simulation using only NEMO model and other two where the ocean model is forced with MFWAM V3 and MFWAM V4 models, respectively. The validation is performed assessing the accuracy of modelled SST and ocean currents with respect to satellite and in-situ observations for the whole year and also for two distinct energetic storm events.

The topic of the paper is very interesting, both for a deeper understanding of the ocean dynamics and the practical improving of ocean models. In addition, this paper involves the modelling of two major storming events, making the study even more interesting. However, in my opinion the paper lacks both informations about the methodology and a proper scientific analysis/discussion of the results. As it is presented now, I do not understand what is the impact of forcing the ocean model with the wave physics in the IBI area. Therefore, in my opinion the paper should be rejected for publication as it is now.

The aim of this paper is to describe ‘the impact of wave physics on ocean-wave coupling ’. Therefore, Section 2 (Model Description) is very important, since it is where the authors should describe and explain the physical and numerical choices they adopt for the coupling process. However, in the document the authors describe only some of the characteristics of the ocean model, totally ignoring the wave model and the differences between MFWAM V3 and MFWAM V4 versions. In addition, the section about the coupling process includes only a simplistic and very general list of the wave-induced processes considered in the study, missing a more useful description of how the coupling has been implemented and actually works (see e.g. Staneva et al. 2016, Clementi et al. 2017) .

A more detailed section about the coupling process could maybe clarify that this study is not actually using an ocean-wave coupled system, but an ocean model forced by atmospheric and wave models – there are no feedbacks between the models.

Regarding the numerical results, first the authors present a limited description of annual averages maps (see specific comments below regarding the quality and usefulness of the pictures). However, there is no attempt to explain the dynamics behind the differences in model results and, as it is now, the text and pictures of this section seem quite not relevant.

Useful information starts with the validation of modelled SST and current velocity against satellite observations. However, the authors limit their discussion to ‘patches of differences’, with limited or any scientific insights regarding any possible explanations. After reading that, the overall feeling is that the modelling skills of the three modelling systems are pretty similar, and there should be no reason for using wave-forcing for an ocean model.

The inclusion of a separate analysis for the two very energetic storm events during 2014 is a peculiarity and strong point of this study. However, I think the authors could present it better, with a clearer structure and a deeper analysis.

Finally, this paper is submitted as part B of a companion document (part A), where the authors validated the wave models used in this study. While I can see the logic behind this choice, I think it is not acceptable that paper A and paper B present almost the same text in the introduction and in the description of moored/wave buoys.

Specific comments

1) I think the paper is not very well written and I recommend to improve the logical/scientific layout as well as the English before resubmitting.

2) The title is not clear and should better clarify that this study does not use an ocean-wave coupled-system but instead it uses wave forcing on the ocean model.

3) Sec 2.1 should include more details about the NEMO model configuration used for this study or give some references if they used a configuration already validated in another study.

4) In Sec 4.1 the readers not familiar with ocean modelling might have difficulties in understanding what is the turbocline – I think a definition is needed. What is the threshold value for the vertical diffusivity that you use to define it?

5) In general, the pictures can be highly improved, both in the choice of the plots themselves (i.e. which variable showing) and the colorbar/style. In the specific:

*) I really do not understand the purpose of Fig. 1 – adding the bathymetry for example?
*) Fig. 2 is exactly a copy of the one of Breivik et al., 2015: what is its aim?
*) What is the rationale of using a jet colorbar when plotting differences between models?
*) Why plotting zonal and meridional components of currents velocity separately?

6) I think the purpose of Sec. 4.2 is very interesting – isolate the effect of a particular wave-induced process on the hydrodynamics. But, why the authors investigated only the wave-induced modification of the wind stress driving ocean currents? What about the Stokes drift and the wave-induced TKE?

REFERENCES

Staneva, J., Wahle, K., Günther, H., and Stanev, E.: Coupling of wave and circulation models in coastal–ocean predicting systems: a case study for the German Bight, Ocean Sci., 12, 797-806, https://doi.org/10.5194/os-12-797-2016, 2016.

Clementi, E., Oddo, P., Drudi, M., Pinardi, N., Korres, G., Grandi, A. Ocean Dynamics (2017) 67: 1293. https://doi.org/10.1007/s10236-017-1087-7