

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

General comments

This paper presents results from a regional ocean-wave coupled system for the Iberian Biscay and Ireland (IBI) domain. Validation of modelled SST and surface currents from a 1-year duration simulation is performed with comparison to satellite and in-situ observations. Results are also separately discussed for Storm Hercules and Storm Petra cases. Ocean runs using wave forcing from two different wave model configurations are compared, MFWAM V3 and MFWAM V4.

Note this is Part B of a 2-part paper. Whilst I was not invited to review Part A in any detail, I note that it covers a validation of the MFWAM model wave simulation for the same domain and over the same period (i.e. an evaluation of the forcing used in the Part B simulations).

I was initially sceptical that the papers really served as 'Part A' and 'Part B' papers, and that each should be required to stand on their own scientific merit. I am still of this opinion. On closer examination, I found both papers to have large sections in common, which is not acceptable for publication. To my view, the Introduction (Section 1) of both submitted papers are identical up to their final paragraphs. The description of observed data (Section 3.1 of each manuscript) are also identical. I am a bit surprised this was not queried by the journal before the manuscripts were 'validated' for review in fact.

Given the strong cross-over between the Part A and Part B texts submitted, my recommendation is that both contributions should be rejected in their current form, and would require major revision before resubmitting after being adequately combined to a more coherent single manuscript.

This would better reflect the content currently in each manuscript, likely just requiring some reworking of Section 2 to provide a better introduction to the wave modelling (which is needed to aid understanding of Part B results), and some discussion of wave forcing validation based on Part A results. If, as likely, this requires some condensing of the results in Part A or Part B, I am confident this will lead to a more robust, clear and concise paper describing wave effects within the IBI system. For example, the authors should critically assess which figures could be combined and reworked without any loss of information.

The remaining comments are provided in review of the 'Part B' manuscript in isolation.

The main general comment on the Part B paper, reflected in the title, concerns the use of terminology for "coupling" and "forcing". At times the paper refers to "wave forcing" and most often to "wave coupling". I suggest for consistency with other papers in this field, the authors are clear this work is on the impact of "wave forcing" in the IBI system – i.e. information from an external system is applied as a boundary forcing, in this case, via file IO, with no feedbacks between the systems. "Wave coupling" in this context should more strictly refer to a more interactive evolution of ocean and wave systems, often achieved using a coupling library, as in Breivik et al. (2015).

The results section and number of figures could also be much more concise, in particular through combining discussion and presentation of the impact of wave forcing in the context of the

comparison against observations. Specific comments are included below around combining some figures. The text of the results could usefully start from around line 266, with earlier material more carefully included where relevant when comparing results to observations – i.e. place the impact of the wave forcing in the context of any model biases relative to observational products. Figures could be presented with more-readable colour scales to better distinguish differences from zero. Results should also be presented as relative differences in the context of the difference between a Ref run and observations. At present it is difficult to see the discussion in the text reflected in the Figures.

Specific comments

Title – see comments above, and need to be clear this is a study on the impact of wave forcing on the ocean model, not ocean-wave coupling as stated

Abstract - The paper would be better served with a more descriptive abstract, which should be substantially rewritten. It currently assumes a reader is already familiar with the modelling setup and for example how do the authors justify a “positive impact” on which “surface key parameters”. What are the “two wave physics versions” used. This is all better covered in the Conclusions.

Line 45-56: It is implicit here that the authors are discussing ocean modelling, for example “it is necessary to introduce an accurate sea state description”, but this is never made explicit to the reader.

Line 88-103: this paragraph could be much shortened, removing much of the CMEMS background, without loss of understanding of the scientific merit or requirement for their work.

Line 103: describes “NEMO v4 IBI-WAVE system”, but the study relates to NEMO v3.6 in Section 2.1. Clarify how this work feeds into v4 implementation, or restructure to emphasise more on the scientific aspects of the study, and perhaps relate to operational implementation later in the paper. I was then further confused by line 211, which introduced a NEMO-WaveV3 and NEMO-WaveV4 configurations – are these related to the NEMO V4 system mentioned or not?

Line 106: do authors mean “wave forcing” or “wave coupling” here? They refer to coupling elsewhere. This is a specific example of the general comment above on “forcing” vs “coupling” terminology.

Line 147: Figure 2 is replicated from Breivik et al. (2015), and does not need to be reproduced here. The reader would be better referred to the original text.

Line 206: there is no description of the differences between MFWAM-V3 and MFWAM-V4, or an introduction to the MFWAM configuration. I understand this is covered in Section 2 of Part 1 (see earlier comments). On reading that section, it would still be useful to have a qualitative description of the physical interpretation of parameter differences between V3 and V4. Is V4 considered more accurate in some way? Do the schemes behave differently in certain conditions? On reading the Part A paper in more depth, this is covered from line 157, but this was not at all clear for the purposes of a reader of Part B paper on its own merits.

Line 214: clarify that the “ECMWF atmospheric system” referred to here is the ECMWF operational forecast system, or similar, rather than making use of reanalysis products for example.

Line 227: I did not see details of the simulation dates covered beyond '2014' – this appears to be more clearly explained in Part A text.

Line 236: Description of results needs more care – e.g. “is colder or warmer” should be rephrased. Similarly in line 239 with “alternately colder and warmer”. Are the authors trying to say that annual mean differences are within 0.5 deg C for impact of coupling, with impact of wave physics of comparable magnitude, and no clear geographical distribution in the differences?

Figure 3: Why is such a broad scale chosen for displaying SST differences here? Suggest replotting to more clearly highlight the scale and distribution of differences. For example, the SST scale in Fig. 13 is much tighter.

Figure 4: Similarly, the differences seem to be swamped by the colour scales used, and should be reconsidered.

Line 240: Comment on whether SSS differences are addressing any biases in the current IBI system. No mention is made in this paper of (e.g. in-situ) observations for validation. A number of BSH sites may be of use in the region of greatest differences. Also make reference to previous work in this region (e.g. Shloen et al., 2017).

Line 251: What information is lost by combining Figure 5 and 6 to illustrate the impact of wave forcing on the current speed rather than separate components? It is hard to justify the detail of +/- 0.2 m/s 'dipoles' in these results – rather, the surface current is a noisy field, and the different simulations have different annual mean fields? Is there anything systematic in the impact of wave forcing here (e.g. net increase or decrease in surface velocities?).

Figure 8: Could be combined with Figure 3, to show difference of NEMO control relative to OSTIA, then show impact of forced results relative to the control.

Figure 9: Authors should show error bars – are the differences between model simulations shown statistically significant? In general, the quality of the plot could also be improved (e.g. overlapping axes labels).

Line 279: The abstract states “results show a positive impact of the waves forcing on surface key parameters”, but the discussion of Figure 9 does not seem to support this – in general, there is a cooling impact of wave forcing, which exacerbates a generally cool system relative to OSTIA, and winter-time RMS is slightly increased with wave effects? I suggest the overview description of results needs a more careful assessment, and more honest appraisal.

Figure 10: Could be combined with Figures 5 and 6, to focus more again on control relative to observed, and then relative impact of the wave forcing. This would make a clearer and more compelling description of the impact of forcing for the reader.

Line 307: In general, the results looking at 2014 as a whole seem to be talking around statements that the results from each simulation are broadly similar (within statistical significance?) when viewed as an annual mean? Results for some buoys showing one system to improved relative to the other, and a different signal from other buoys suggests either working with variability and consistent results, or perhaps a need to be more precise on the location of these differences. Discussion of the annual results could be summarised more concisely, and see again comments on combining the comparison with observations into the discussion on the relative impact of wave forcing. In contrast, the effect of wave forcing is really more apparent when considering certain events, as highlighted later in the results section.

Line 324: It was hard to match up the description of the impact of wave forcing on currents with Figure 14, which seems to show biases of comparable magnitude for both systems. Perhaps a clearer presentation of the results (e.g. presenting relative differences, choice of different colour scales) would help. It is not immediately clear from Fig. 14 that Nemo-WaveV4 has near zero bias across the domain, while the Ref is 30% underestimated.

Line 370: Is the NoAtm run simply just removing the 'tauoc' parameter variation here? The neutral drag coefficient is mentioned explicitly here, but not discussed explicitly in Section 2.2. The NoAtm results might also be usefully better combined with earlier discussion of results, and combining Figure 22 with earlier figures. In general, the discussion on p.12 is lacking any particular physical insights, and seems to repeat a sense that results were similar when averaged over an annual mean. It is not currently clear what new insight the section discussing NoAtm results is bringing to the community, or of its relevance in the context of the IBI development. This should be better explained.

Line 379: typo "illusyrated", and later "he" rather than "the".

Line 410: The conclusions might better highlight the different modes of validation – namely the annual mean differences and the storm case studies. It should be better explained what each view is providing, and whereas the impact of wave forcing is generally small in annual mean difference terms, the impact can be more substantial for certain periods. Some discussion of the extent to which the IBI system has already been tuned in 'ocean-only' mode, and the effect this might have on the impact of wave forcing could be of value.