

Interactive comment on “The impact of wave physics in the CMEMS-IBI ocean system Part A: Wave forcing validation” by Romain Rainaud et al.

Anonymous Referee #2

Received and published: 6 May 2019

General comments

The results of this paper focus on the validation of the IBI wave forecasting system, part of CMEMS. Considering that CMEMS products are open, free and widely used, it could have been a good reference but, in its present form, it is not, and a major revision is required.

A basic problem with this paper is that its introduction, including the rationale, the state-of-the-art and the objectives, relates almost exclusively to the Part B of this study. As a result, the state-of-the-art, the objectives and the authors original contribution regarding wave model validation in general and/or focusing on the IBI region, is missing. Also, later in the paper, when the authors describe their observational datasets, a confusing mix of Part A with Part B oriented datasets is presented. If the authors wish to keep

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Part A as an individual paper they should develop their introduction and methodology based on Part A results and objectives. Otherwise, they should probably merge the two papers (A and B) in one.

In addition to the above, the methodology and mainly the results of this paper will need to be better and more consistently developed. For a standalone paper, a more thoughtful validation would be desired. Finally, the conclusions will need to be better justified either by the results of this paper or/and the literature.

Specific comments

Introduction:

- There are more references on the important role of ocean waves in processes that govern the fluxes across the air-sea interface and in the upper-ocean mixing. It would be good to include some additional references. When a single reference or a limited reference list is provided as an example, put an "e.g." in front.

- The whole introduction focuses on the importance of atmosphere-waves-ocean coupling (related to Part B of this study). In particular, it focuses mainly on the impact that the waves have on the atmosphere and the ocean and thus on the importance of accurate sea-state simulations. However, nothing is mentioned in relation to the accuracy of the wave model itself and how this is enhanced in this paper (related to Part A). Consequently, the state-of-the-art on wave modelling in general and also on IBI region wave modelling is almost missing and thus the authors do not give the proper credit to related work while their own original contribution to the Part A of the study (i.e. this paper) is not clearly indicated. - Lines 88-89: is this a one-way coupling?

Section 2:

- In general, this section needs to be better developed.

- Line 103: "mean wave parameters": what is the meaning of "mean"?

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- Lines 106-112: it is not easy to associate the text with the related references. Are all changes detailed in Ardhuin et al. (2010)? I could not find something on Phillips spectrum in Janssen et al. (2014). Which parameterization follows which reference should be made more clear.

- Table 1: some description of the variables/coefficients is necessary together with some justification for the adopted values.

- Line 112: if you mention the extent in latitude you should also mention the extent in longitude.

- Line 114: give also the horizontal resolution of the wind forcing.

Section 3.1:

- In general, this section is particularly confusing (see below).

- Under the title "Wave observations" there are descriptions of SST and ocean currents satellite observations. Apart from the fact that the title of the section does not fit with the content of the section, I believe that SST and surface currents observations descriptions do not fit with the scope of Part A paper which is wave model validation.

- You should mention where Jason2 and Saral SWH altimeter observations are obtained from.

- Note that collocating every 0.1° is expected to retain spatial correlation present in successive 1 Hz observations and/or in neighbouring model grid points which can bias the validation statistics towards specific events. This also depends on the resolution of the wind forcing, which, for 2014, has been 0.141° . Normally, a spatial averaging of 2-3 times the resolution of the wind forcing should benefit the reliability of the statistics.

- Lines 142-144: again, the title is "wave buoys", however measurements of surface atmosphere and ocean parameters are also mentioned here.

Section 3.2:

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- Line155: give the resolution of the wind forcing.

- Lines 169-170: it appears that the validation of the wave model is done only against satellite observations, while Section 3.1 and Table 2 speak and list wave buoys.

Section 4:

- Line 180: scatter index is by default normalized, isn't it?. You should give a definition of scatter index since this can be ambiguous.

- Figure 3: the slope of V3 appears to be somewhat better than that of V4, in contrast to what is stated in the paper.

- Lines 184-185: it would be very nice and useful to show some validation of the wind forcing (scatter plots and maps) alongside the wave model validation results so that the link between the two can be better established. Otherwise, this link should be supported by the (rich) literature on related work.

- Lines 186-187: what is the "average scatter index"; Please, explain.

- Figures 4, 5, 7: the IBI domain is stated to be the one shown in Figure 1. However, the aforementioned figures present validation results outside of this domain to the east. Why is that?

- Figures 4, 5: these figures are essentially visually identical, I would propose to include only one out of the two and explain in the text that the two model versions produce results that are marginally different.

- A normalized version of bias would be definitely more useful to compare the performance of the model in space and in time.

- What is the rationale behind splitting into the 3 zones shown in Figure 7? Wouldn't be better to separate in sub-regions having similar characteristics? For example, North Sea, Med Sea, northern and southern Atlantic? Or coastal versus offshore? Now, zone 2 includes the Mediterranean Sea with a part of the Atlantic, thus it is kind of normal to

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have a higher SI. However, the results are neither representative of the Atlantic part of this region nor of the Mediterranean part.

- Figure 8: please refer to the experiment you are using to produce these results.

- Line 207: it is stated "...the best performance is obtained during summer months June and July." I do not see that. For example, in this figure, the SI of December, January and February is overall better. Also, what is stated contradicts what is shown in Figure 6, where we see that the model has a better performance during winter. In addition, there is a number of studies that have found that SI and Correlation between wave model SWH and observations is generally better in 'stormy' conditions (e.g. Cavaleri and Sclavo, 2006; Ardhuin et al., 2007; Bertotti et al., 2013, Ravdas et al., 2018). Regarding Bias, as I have mentioned above, you can not readily infer relative performance in the different months without using a normalized form of the qualification metric.

- Lines 207-208: I do not see what is stated in this sentence either. Actually, it appears that this is mostly the case in summer.

- Lines 209-215: In Figure 1 and Table 2 a list of over 15 buoys are shown. However, it appears that only 2 buoys are used for the wave model validation. This is confusing. Also, this is a domain rich in wave buoys. To enhance the quality of this paper, the use of the numerous wave buoy data is recommended.

- Line 213: I cannot find buoy BI in Figure 1. Also, when I plot the coordinates in Table 2, it falls in middle land France.

- Lines 212-213: "...despite a slightly increase of MFWAM-V4"?? When? In June and September? And what about winter months when the opposite to what is stated in the paper appears to be true? In Figure 9, what is more obvious is that winter and spring perform better than summer and autumn. The interpretation of the results should be developed more carefully.

- Line 214: during the entire year (except March).

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- Line 215: "...due to a more variable sea-state" instead of "due to storm occurrence" seems to be more appropriate. At BelmA, are there no storms? (Note that, in some previous studies, winter has been associated with better overall stats).

- The number of observations used for the computation of the statistics should be included. Also, in the wave-buoy case, at least the bias should be included to enhance the validation and to facilitate comparison with other studies. Other metrics like RMSE and correlation would be useful in addition to mean and standard deviation values.

- Line 219: it appears to be more of a marginal increase in quality rather than "...a general increase".

Section 5:

- In general, this section includes several statements that are not well justified in the text (see below).

- Lines 225-227: since the advantage of using the Phillips spectrum is not shown in this paper, I believe it should be better justified through your state-of-the-art literature.

- Lines 227-228: No improvement in Bias is shown (contrary to what is stated), also, SI improvement appears to be marginal and generally less than 1%.

- Lines 228-230: Some results and/or some references that justify the link between the wind forcing and wave output quality should be presented.

- Lines 230-231: Yes, but this does not mean much regarding the relative performance of the wave model in the different seasons. In high winter waves we normally have a higher model bias than in low summer waves.

- Lines 231-233: In Table 4, I see exactly the same improvement (0.1%) in all zones with no improvement in bias.

- Lines 233-234: the improvement is generally less than 2%, thus I would not use the word "significant".

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Technical corrections

In general, the English in the present manuscript require improvement. Please carefully proof-read spell check to eliminate language errors. Examples of some obvious mistakes follow but there are several more: - line 91: "...where there were a by several storm events..." - line 97: "Results of the validation is given ..." - lines 97-98: "...concluding remarks will discussed..."

Line 61: "...surface temperature.." you mean "... sea surface temperature"?

Lines 76-77: SST abbreviation should be explained when it first appears in the document.

Line 87: "...an evaluation of the impact...". Change to "...the incorporation of the impact."

Line 89: "...to provide reference" ??

Line 93: "...using NEMO V4 IBI-Wave system": this is not clear for the reader which is not involved in CMEMS versioning or terminology. Could be something more like "in preparation of the coupled NEMO-WFAM system for the operational suite of the Copernicus ... "?

Line 96: "...the different wave observations..", change to "...the wave observations used in the validation and the performed..."

Lines 67-68: change to "Finally, Section 5 includes a summary and concluding remarks".

Line 107: change "...which is called ST4" to "...known as ST4"

Line 146 and Table 2: "nationality" is not normally used for objects. Please replace (e.g. country, operator, operated by).

Lines 152-153 (and 157): refer to Table 1 (since you have already introduced this table

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in Section 2). Also, remove any reference to version numbers apart from the names of the experiments. You can refer to them as previous version and/or more recent version of the IBI system.

Line 159: Connect better the sentences (e.g. the relevant tuneable physics parameters include...)

Line 165: change to "...surface drag coefficient (cd)..".

Line 197: Is it Figure 7 instead of 8?

Lines 201-203: Refer to Table 4 as soon as you start describing the relevant results.

Lines 209-215: refer to Figure 1 when you give the buoy reference code.

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-165>, 2019.

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