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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 #1

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General Comments

This paper describes the set up and validation of a recent update to the IBI wave forecast model for CMEMS. It also provides accompanying evidence for another paper documenting the impact of wave forcing on the ocean. The paper therefore serves as a documentation of a state of the art operational wave model and can be usefully published as such. However, the recommendation in this case is that the authors should undertake some major revisions of the paper first. Please see the specific comments below.

Specific Comments

1. Please provide more evidence for, or discuss further, why the model's 'more consis-

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Discussion paper

tent' estimate of surface stress (body of paper) can be considered 'better' (abstract). I'm not contesting this claim, but it would help the reader to have more information.

2. Please remove, discuss further, or cross-reference (from evidence in the other paper), why applying the Philipps spectrum shape better estimates Stokes Drift.

3. In the abstract, the authors state that peak wave periods are slightly improved but do not provide any evidence for this. Demonstrating the improvement in periods, or just a straightforward documenting of performance, is important in supporting the final point in the conclusions that MFWAM is well suited to describe parameters for coupling with the ocean in anything more than 'in principle' (i.e. from what we know from existing wave theory). In the paper thus far, the authors have only demonstrated that the bulk energy of the wave spectrum is correct (significant wave height); validation of periods provides an initial indication that the energy distribution in frequency space is fit for purpose. So, please add some verification of periods, or caveat the statements about frequency based wave terms more carefully.

4. Section 3.1 'Wave Observations' presently contains a lot of text on satellite SST observations. Please remove as these are not being used here. Similarly, it is not clear to me whether the L4 wave satellite products were used in the validation.

5. As it reads currently, the paper describes the changes to the wave model as being in the dissipation source terms of ST4. However, Table 1 indicates that input terms (Betamax and wave sheltering Su) were also adjusted and the text should be updated to acknowledge this.

6. It would also be enlightening to illustrate how the change to the tail shape alters and example wave spectrum and subsequent Stokes Drift value (unless this is discussed in the accompanying paper or can be referenced from elsewhere in the literature?). This appears to be one of the more novel features of this model.

Technical changes

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1. Please use colours for v3 and v4 models consistently in Figures 6 and 9.

2. The English/grammar in the paper needs careful review – I spotted numerous typos and errors when reading through this version of the paper. I will be happy to assist this process in a subsequent re-submission of this paper.

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