

## ***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 #3**

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The manuscript describe the validation of the MFWAM wave model, in the context of the CMEMS-IBI ocean system. The authors claim that an improvement “on drag limitation” and of the model performance is observed introducing “a tail shape from the Philipps spectrum”.

General comments: The manuscript lacks of basic information needed to understand what the authors have done and want to present (e.g., what is the difference between version 4 and version 3 of the model? How did the authors estimate the drag coefficient? How can they relate the larger scatter in Cd for a given wind speed to the improvements they have introduced, based on a model-model inter-comparison?). But at the same time contains lot of information that is not useful for understanding and interpreting results and conclusions (e.g., SST and surface current observations are

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described but not used in the paper), and repetitions. Also, some of the claims and conclusions are not supported by data and by results (e.g., it is not proved that a better surface stress for high wind conditions has been achieved) and some of the results presented may be flawed (e.g., the scatter index and bias maps showing validation performance against 1 year of satellite data may suffer of spatial interpolation and of the very low satellite frequency, see below in specific comments). However, from the Figures presented, it is really hard to appreciate significant improvements with version 4, with respect to version 3. Finally, I do not see the reasons for having a separate paper for wave validation. For these reasons, I cannot recommend the publication in OS and encourage the authors to improve the quality of the results and of their presentation, and to consider having a single paper, merging part A and B .

Specific comments: 1) Improvement in peak wave period is mentioned in the abstract but not shown in the paper. 2) “Philipps” (largely used throughout the paper) has to be corrected with “Phillips”. 3) Jason-2 has repeat cycle of 10 days, it means that a given point is observed about 36 times over a year, 3 times over a month on average. Saral has a repeat cycle of 35 days, it means that a given point is observed about 10 times over a year, less than one time over a month on average. In this situation, are scatter index and bias maps over a year (and monthly variations) significant? 4) line 163: Table 2 is actually Table 1. 5) including best fit line slopes in Figure 3 would ease interpretation of the results. 6) SWH underestimate is imputed to wind speed underestimate, but this is not shown nor proved. 7) why is Mediterranean Sea mentioned/modeled in the IBI system? 8) what does “more consistency surface stress dependency” mean? 9) Figure quality needs to be improved. Labels in Figure 4 and 5 cannot be read. 10) English and grammar should be improved.

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