

Interactive comment on “Can wave coupling improve operational regional ocean forecasts for the North-West European Shelf?” by Huw W. Lewis et al.

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1/ General Comments

This study presents the recent works of the Met-Office teams related to ocean-wave coupling on their AMM15 oceanographic prediction system operated for CMEMS. Currently this system does not explicitly contain the wave effects other than by the standard parametrizations used in the NEMO OGCM. The authors explore a coupling strategy with WW3 wave model for a wave-dependent surface stress, Stokes-Coriolis forcing and a wave height dependent ocean surface roughness. The strength of the paper is to study the impact of ocean-wave coupling vs forcing in both assimilated and non-

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assimilated framework, which was not be done so far and open perspective for near future operational use. Comparisons to in-situ observations on key oceanic variables such as temperature, salinity, SSH and currents are carefully explored. Without showing any strong improvement, the results are consistent with previous works related to the subject and quite encouraging for a system that has not be tuned to be coupled with a wave model. The document is well structured and written. The figures are all clear and well illustrate the text. I am in favor of its publication, but have some minor questions for the authors.

2/ Specific Comments

SC1: p6, l20-25: Using the stress from the wave model directly to force the ocean effectively eliminates the inconsistencies in the BULK formulas between the wave model and the ocean model, which is practical and efficient for coupling the two models. Nevertheless, this raises two problems: 1/ the momentum flux is then calculated for neutral conditions 2/ the momentum flux is no longer in equilibrium with the turbulent fluxes of heat and humidity. Could you tell me your feelings on these two points please? Thanks.

SC2: p6, l27: For the introduction of turbulent wave-related kinetic energy, you keep the Craig and Banner parameterization with a coefficient equal to 100, but many studies have shown that this value is highly variable. On the other hand, you modify the dissipation length according to the significant wave height. Have you done any preliminary tests about these choices that are not included in the study? I am just curious why you have favor this way instead of the other, thanks.

SC3: p11, l30 and figure 8: Data assimilation erases the differences between coupled and uncoupled runs, especially in summer in the mixing layer. For this period, the assimilation heats the surface water to compensate for the cooling caused by the enhanced mixing forced by the waves. Does this mean that this summer cooling produced by the wave forcing is finally unrealistic? I'm a little confused because it's one of the main results that everyone finds for summer. I would have been happy to see the

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comparison with evolution of observed profiles to make a call, especially with regard to the heat sent under the mixing layer.

SC4: I have not seen any comments on the interaction of waves and currents with tide, even though the area is known to be subject to these phenomena. I think it's worth saying a few words about it in your manuscript.

3/ Technical Corrections

TC1 : Figure 17 : typo in the Figure description : (c,d) → (b,c). Maybe “12:00” instead of “1200”.

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-148>, 2018.

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