

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

### **Anonymous Referee #2**

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This paper presents the impact of inclusion of wave coupling in an operational or the North West European shelf region. Results of the system (based on NEMO and eventually WWIII) with and without data assimilation (DA) are presented. The paper is well structured with excellent level of language and quality figures. The methodology is clear, with extensive comparison of the results with and without waves coupling, for both free and assimilated simulations, and comparison with data when available. Effect on tracers and dynamical fields are presented.

Nevertheless I'm a bit puzzled with this paper. As stated above, structure and methodology are very good and it's pretty pleasant to read. Thus, I'm not comfortable with the

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general angle of the study, and can see two major weaknesses:

1- Looking at the results of the DA assimilation system doesn't seem fair, even if it makes sense in the context of Copernicus. As stated by the authors, the system as been tuned for ocean without waves, and more generally it should be adapted in the context of a coupled system in term of methodology. It seems obvious when looking at the results. When DA is very "active" (SST, SSH), results of DA system show very little sensibility. When less observations are available (SSS, vertical sections), the effect is comparable to the forced run. I may misunderstand the general idea, but there is little to learn from this, apart a demonstration that the current DA system is very efficient, but should be modified when adding different physics. The title of the paper is a question, and basically when reading, the answer is no, but is biased. I would be happy to be wrong on this and discuss, I'm not a data assimilation expert.

2- Some wave forcing terms are missing. Authors are very honest on this aspect too. I'm not expecting to have all effects included, especially the very fined scales ones, which would induce heavy additional tests and tuning of parameterisations, and may not be relevant for AMM15. But it' seems to me that some large scale effects (conservative and well posed) are missing: a. It's unclear from part 2.3 if Stokes velocities are included in tracer advection. If not, it's really unfortunate, if we consider the induced changed in the pressure gradient, and its balance with Stokes+Stokes-Coriolis. b. The vortex force (Uchiyama 201, Bennis 2011), taking into account interactions between stokes velocities and eulerian one, is not included. It would have been a real added value, especially because it has been neglected in previous studies with NEMO (Law Chune 2018, Brevik 2015). It would be interesting to see if it has an effect or not in forced regional configurations. Anyway it would be much more coherent. Considering the number of projects and initiative around the inclusion of waves effects in NEMO, I'm pretty sure those implementations exist.

Again, those two points are not hidden, the authors don't make any mysteries about it, so it's kind of difficult to state. It's more a question of editorial line and authors

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purposes. It may seem unfair to ask for substantial revisions and work for a very well written paper, but I think it worth it in term of scientific value and novelty. To summarise, I would be much more comfortable if some limitations (not all) pointed by the authors as future topics of investigations (part 5) were a little investigated when feasible.

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