



EGUsphere, referee comment RC1  
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## **Comment on egusphere-2022-481**

Anonymous Referee #1

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Referee comment on "Accuracy of numerical wave model results: application to the Atlantic coasts of Europe" by Matias Alday et al., EGU sphere,  
<https://doi.org/10.5194/egusphere-2022-481-RC1>, 2022

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Review of "Accuracy of numerical wave model results: Application to the Atlantic coasts of Europe"  
a paper by Alday and 3 co-authors.

### **Summary:**

The authors design an unstructured grid implementation of WW3 for the European coastline. They evaluate the model against buoys and altimetry. They perform sensitivity analyses to determine impact of various things. Topics studied include: simple wind adjustments, swell dissipation settings, tidal currents effects, directional resolution, and bottom friction sensitivity.

### **Recommendation:**

I'm not very familiar with the expectations of the journal, especially re: what types of papers are OK. I have a generally positive impression of the paper. So, I'd like to give the authors the benefit of the doubt, and recommend "accept with minor revisions"

### **General comments.**

Some of the comparisons seem rather workaday, like the sort of thing that would go into a tech report or dissertation. But it's OK, I think. And they are not just dry comparisons: the authors put substantial thought into them, so nothing comes across as especially unnecessary.

The literature review is sufficient.

The comparisons are careful, the discussion is straightforward/honest, and the authors don't over-sell the outcomes.

The findings are potentially useful for people doing similar modeling.

The use of English is very good. There are a few awkward or broken sentences (e.g. lines 310, 393-394) and minor style errors (e.g. lines 21, 27, 409, 412), but not more than  $\sim 1$  problem per page.

The purpose of the study meanders.

There are a lot of evaluations, but they are a grab-bag that are only similar insofar as they are for the same system. I put them in four broad categories:

- 1) differences vs. global model (geographic resolution)
- 2) impact of design choices, where choice is not clear a priori (swell dissipation setting, directional resolution, BC formatting, wind adjustments)
- 3) impact of including/excluding effect, where it is more or less accepted a priori that the effect should be included (bottom friction, tidal current)
- 4) general evaluation of the model in its final form (altimetry comparison).

I don't have a good suggestion for how to address this. I suppose the authors do not want to stick to a consistent type of content, since they would lose half the study.

I really like the evaluation of the model against altimetry, sorted by distance from the coast. Excellent.

There are a number of unexplained acronyms/initialisms. Perhaps a glossary would work better than rigorous in-line explanation. I leave this to the editor to decide.

When printed out, some of the figures are hard to see, like Fig 7 a-d, where lines are hard to distinguish.

### **Specific comments.**

52: "CFL...minimum time step". Wouldn't CFL dictate the maximum time step?

Same paragraph. Why is the implicit scheme not used? It should be mentioned, at least.

63: Boulders of  $D_{50}=15$  cm. This is too small to be boulders, which are at least 25 cm, according to wikipedia. Cobbles, maybe?

95: only tidal currents are used. Maybe it is OK to neglect general ocean currents, but it should be justified/explained here.

113: 30 sec time step is mentioned here, but we saw 13 sec limit earlier, so I don't understand the discrepancy. Please explain.

125: maximum frequency of buoy should be mentioned. And later, the frequency range used for calculation of  $Tm_{02}$ , etc.

Section 4.1. It should be made more explicit up front here that the impact of resolution is studied by comparison against a global model, not by applying two regional models of different resolution. (Initially, I guessed the paper was doing the latter.) Same problem occurs on line 383.

Figure 7. Is mean  $E(f)$  from an average over the 14 days? This should be specified.

Section 4.4. I don't understand the difference between 48D24BC and 48D48BC. Both are interpolating from the same global model to the same regional model, right? Is it a question of doing the spectral interpolation in WW3 vs. outside WW3? This should be clearer.

Figure 17. I didn't realize until this figure (at the end of the paper) that the authors ran this regional model for a period of  $\sim 16$  years. Was that mentioned anywhere? The comparisons prior to that are for much shorter periods, e.g. 1 month.

425: Is the satellite altimetry dataset mentioned here?