Interactive comment on "Synergy between satellite observations and model simulations during extreme events" *by* Anne Wiese et al.

Anonymous Referee #2

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General Comments The paper deals with interesting topic of the performance of WAM model in the region of North and Baltic Seas. The authors analyses the sensitivity and the accuracy of the model WAM using different wind models (and considering different time steps and resolutions). They found that the time resolution is crucial (hourly data) in this region in order to reproduce the significant peak of the wave height and its correct location. Moreover, they describe which is the best (most accurate) wind forcing model in the region. The performance of the newly Sentinel-3A data is compared with other operational satellites (Jason-2 and Cryosat-2) showing the better results for Sentinel-3A in the coastal areas (mainly in the first 10km). The analysis also reveals that the Sentinel-3A quality is independent of the satellite flights direction, metocean conditions, and even for the wind direction relative to the flight direction. To my knowledge, this is the first study that describes at this detail this kind of process and Sentinel performance in this region. In general, the manuscript is well organized and written, allowing an easy reading and following up the discussion and conclusions. In my opinion, a set of few changes would allow the publication of the manuscript in the journal. I recommend minor revision.

Authors: We are very grateful for the kind considerations about our manuscript

However, still some points that the authors should consider.

1) Are some of the wind models used for the analysis already assimilating data? If this is true, it should be considered when comparing all the results. Moreover, the validation of the winds should not include the assimilated data.

Authors: We agree and this is now discussed in the revised manuscript. For the ECMWF reanalysis, near the surface, in-situ wind data were indeed part of the data provided to the 4Dvar. Sure the wind data enter in the system but so are all the other data and the vibrational approach will attempt to improve the fit with all these observations but it does not mean that one can expect a perfect fit. Actually, it is one of the key diagnostics when looking at the performance of the DA system, namely the fit to the data from the first guess and the analysis. Generally one expects to have a better fit with the analysis. So it is still worthwhile to look at the comparison.
Also, the short range forecasts by the ECMWF from the 4Dvar system have been influenced by the data assimilation because the assimilation is performed over a 6 or 12 window with data that can be more recent (by a few hours) than the start time of each forecast. For the DWD forecast, the in-situ wind data is assimilated into the analysis used to initialize the forecast but for the forecast itself, no data was assimilated.
For the coastDat-3 data set no data was assimilated at all.
This explanation has been added in Section 2.3.

2) About EOF. I am not sure that it gives more information than observed in Figure 4. By the way, looking at Figure 4 it is clearly observed the displacement of the location of maximum Hs to the NE in the 6h models. So basically, the first mode of the EOF as you have it now are related with this "displacement". It would be nice to apply the EOF only to the 6h (or 1h) and not only for the event of 29th September, but also for the entire simulation. In that sense you can describe how are the modes related to both the time and the space variability.

Authors: The EOF analysis is carried out for one event for the whole ensemble in order to estimate the differences between the model simulations with the different wind forcings. Sure, most of the differences can already be observed by looking at each ensemble member individually but this analysis combines all of them. We now did the EOF analysis for June till August 2016 for the difference between the two ERA5 model simulations in order to estimate the influence of the different temporal resolutions over time, but this only revealed again, that during normal conditions both model simulations are very similar with an explained variance of only 3.13% for the dominant mode of the EOF and even less

for further modes (Figure 1). The large differences between the model simulations appear only for extreme events, which is why we concentrated on that. We have now added the above explanations in the revised manuscript.

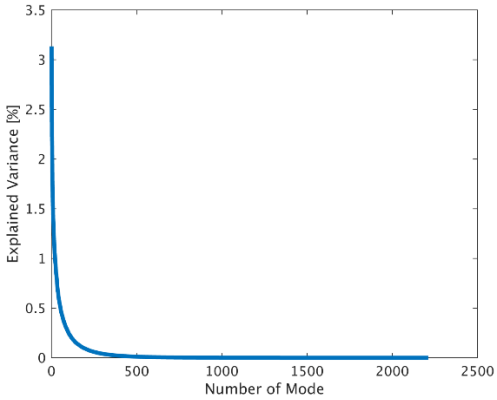

Figure 1: The explained variance for each mode of the EOF analysis for the difference between the model simulation with hourly and six hourly ERA5 wind forcing for summer 2016.

3) Could you define the statistics used? RMS, Bias and correlation are well known. But what about SI? Are you referring to the RMSE normalized by the mean observed values?

Authors: We agree. The statistical analyses that have been used in our study are have been defined in the Appendix.

4) While comparing the same model with different time resolutions seems appropriate, I do not have the same feeling when the spatial resolution comparison is done. You are comparing different models, not the same model with different resolutions. In this case, if you want to check the sensitivity of the model to different wind forcing resolutions I would suggest to do some spatial subsetting from the finer resolution.

Authors: Thank you for the comment. The wind data sets that have been used here indeed differ in their horizontal and spatial resolution as well as the data that are assimilated (or not). This is further discussed in Section 2.3. The aim in this study is to give an overview of the performance of the wave model over the considered area with different available wind products, so that other scientist planning to performed similar simulations have an idea what to expect. Our motivation is explained now clearer in the introduction. These comparisons were also used do define our "reference forcing" and further proceed with those model simulations and assessments of the satellite data of Sentinel-3A in comparisons with older altimeter data widely used for validations, in-situ observations together with the model simulations. This has been also emphasized in the Discussion.

Specific Comments

P2L10-L12: This sentence seem to indicate that the wave models always worked good and with high accuracy, and bad or good results are only dependent on the meteorological models. In my opinion, it should be rephrased.

Authors: In our manuscript, we refer to the paper by Cavaleri and Bertotti, 1997 and the state-of-the- art provided by that manuscript.

"Our experience as wave modellers, hence of users of surface wind products, strongly suggests the lack of sufficient resolution as a likely culprit. As pointed out in the introduction, advanced wave models are at present more accurate than the meteorological models, and they are therefore good indicators of the quality of the driving wind fields."
We have now re-written this in the revised manuscript making this clearer.

P2L18: define acronym SWAN

Authors: We have added the full name.

P3L19: what it this mail referred to?

Authors: We have changed the source of the data.

P4: some acronyms not specified: i.e. JCOMM, ORBCOMM, GOES and WMO

Authors: This has been fixed in the revised manuscript:

P5L14-L15: some information about the coarser model simulation? Spatial, frequency and temporal resolutions?

Authors: The information has been added.

P6L3-L11: there is a kind of a mess in these lines. All the models are "interpolated" to different resolutions (finner) than the original. However, the operational ECMWF of 9km (0.08◦) is interpolated to 0.125◦. Aren't you losing some information in this process?

Authors: Our apologies for the confusion. This has been re-written in the revised manuscript. Explaining that this is how the model output is made available. We have added also more information about the wind forcing data.

Table 2: are the models list following some order? I would ordinate them from coarser to finer (and homogenize horizontal dimension units).

Authors: We are sorry for the confusion. The units have been homogenized and ordinated.

P7L30-L32 : where have you seen this? is there any figure I am missing?

Authors: This is clarified in the revised manuscript. We calculated the bias and haven't shown any figure for this.

P8L15: change "time step" for "instant" or "event"

Authors: We agree. Thank you. This has been changed.

P8L18: I am not sure if the word ensemble here is the most appropriate. Probably you can use: numerical tests.

Authors: We decided to stick to the word "ensemble" here, because it better reflects our idea behind using the different model experiments.

P10L11-L14: could you mark the in situ measurements used here in the Figure 1?

Authors: They are marked with the white box. We rephrased the sentence to make this clear.

P11_L1-L3: why don't you analyze the event two days before? It is not 7m of Hs, but more than 4m.

Authors: The extreme event is analyzed as well in Section 3.2.3. We refer to the section and corresponding values.

P12-L14: do you take into account some kind of land-mask? For example, what happens if your mooring is inside a bay, and the satellite track is outside (with land between them)?

Authors: You are right. This has not been taken into account. We clarified this in the revised manuscript.

Table 3: number of data used for the analysis please.

Authors: This has been added.

Figure 1: explain the black and white boxes.

Authors: We have added an explanation in the figure caption.

Figure 2: what is M and R in the text boxes (also in Figure 3)?

Authors: We have added an explanation in the figure caption.

Do the 6h and 1h models have the same number of "entries"?

Authors: Yes, because only the wind forcing is 6 hourly, but the wave model still has hourly output.

Figure 4: the OBCs seems a bit different in some cases (4a and b shows wave heights in the NW corner not observed in the others…). Is this related only to wind forcing?

Authors: The OBCs are the same for all model simulations. The differences are solely because of the different wind forcings. We have now made this clear in the revised manuscript.