

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

Anonymous Referee #1

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Overview:

This paper focusses on the issue of improving wave model performance by improving the driving wind fields. It is following many other papers on this topic but does add some originality in its assessment of the differences between wave model outputs. The wave model outputs are compared with in situ data as the ground truth. The results are similar to previous work in demonstrating that wave model results are sensitive to the wind forcing, which is not surprising or very new. A secondary part of the work is in examining the quality of satellite data on significant wave height, showing that the most recent satellite Sentinel-3A is superior to previous satellites, especially within 10km of the coast, but still requires bias correction of the significant wave height before any improvement in wave model performance could be achieved by its assimilation into an operational model.

Authors: We would like to thank to the reviewers for the valuable comments about our manuscript. Important points have been raised that helped us to improve the clarity and the understanding of our study.

General:

The paper is generally clearly written and readable, apart from a few errors (listed below). However it could be improved by some reorganisation and rationalisation.

Authors: We have revised our manuscript – reorganizing it following reviewers’ suggestions.

Authors: The revised manuscript reflects these comments and a point-to-point response to them is provided below.

The motivation for the study and the order of the presentation could be sharpened up.

Authors: This has been modified in the revised manuscript

The satellite data assessment is stated to be the main motivation but the bulk of the paper addresses the wave model forcing by different atmospheric model wind fields and assessing which performs better. This exercise is somewhat flawed as the wind fields come from different models and it is not clear what are the differences between these. This should be discussed further.

Authors: We agree. The first part of the paper is focused on the wave model forcing by different atmospheric model wind fields and assessing which performs better. This has been now stressed in the introduction and the abstract has been also modified, accordingly. The differences between the different models is discussed in more detail.

Some are reanalysis products and some are forecasts. Presumably these have different wind datasets assimilated into them. Thus it is not really a like-for-like comparison. The issue of model spatial and temporal resolution could be more rigorously assessed by sampling the same wind field at different resolutions, but this is not done.

Authors: We agree with the comment. We don’t aim here to make assessment and quantitative analyses of the different wind forcing provided by different centers and sources. The wind data sets 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. Our motivation is explained now better in the introduction. The aim in this study is to give an overview of the performance of the wave model over the considered area. These analyses were needed to 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.

The authors state that the WAM model performs well with all the datasets, which has already been demonstrated.

Authors: The only publication we are aware of comparing wave data with ERA-interim and ERA5 wind forcing is Nose et al. (2018) "Predictability of storm wave heights in the ice-free Beaufort Sea". This is, though, focused for a different region and for only two months. This has now been referred to in the text and added to the references. Since wind re-analyses of ERA-5 are still new there are still no sufficient publications about WAM performance under this forcing on regional scales.

It is also known that higher spatial and temporal resolution improves the wave model performance.

Authors: We agree. More discussion and references have been added. We demonstrate in our study the wave model performance for the considered area with the available wind forcing. This has been re-formulated now to make our statements more clear.

There is an interesting comparison between results, using an EOF analysis to show differences in wind speed, direction and location of the maximum affecting the maximum wave height in an extreme event. Using reference to a model ensemble where it is demonstrated that some wind fields are better quality than others seem somewhat perverse – can this be justified?

Authors: In our study we do not aim at stating that one wind field is better than the other. Rather we wanted to demonstrate with which wind field WAM produces the best results in order to compare the satellite data with the model. We rephrased this and made our statements clearer.

Overall this paper needs moderate revision before acceptance for publication.

Detailed comments:

1. The title should include 'of wind and waves' after 'simulations'

Authors: Thanks! It has been added to the title.

2. The first line of the abstract is somewhat misleading as only satellite wave data are evaluated. Also this part of the work is second to the study of the spatial and temporal resolution of the wind forcing for the wave model and so perhaps should not be introduced first.

Authors: We agree. The abstract has been re-organized and rephrased following this comment.

3. P 2 line 7, suggest inserting 'in determining' to replace 'of'

Authors: This has been inserted.

4. P 2 line 31 'flagging of (data)' – explain further, this is rather cryptic.

Authors: The word has been replaced by discarding.

5. P 5 line 11 – 30 times 15 does not equal 360 - check directional resolution

Authors: You are right. It is 24 directions.

6. P 6, line 6, why is wind data interpolated to 0.25 deg in this case? Is this a typo?

Authors: This is how the output is made available. We rephrased the paragraph.

7. On p 6 there is repeated use of increased resolution while referring to reduce grid size, this should be more clear.

Authors: This has been modified.

8. In Table 2 use consistent units for spatial resolution

Authors: Thank you. The units have been homogenized.

9. P 6 line 22, state extreme event is in September.

Authors: This has been added.

10. P 7 line 5 and subsequent – use of a strange reference to calm conditions – does this mean zero wind or less than a threshold? It seems superfluous to state the models give the same result when there is no wind!

Authors: Thank you. The word calm was not correctly used here. What we meant were wind speeds below 5 Bft or significant wave height below 2m. Therefore, we modified that.

11. Presumably there can be residual swell. Does the model use the same boundary forcing for each case? This seems implied by p 5 line 15. Are all the wind fields consistent near the open boundary? Could this be an issue – discuss.

Authors: Yes, you are right. In our study the boundary conditions are the same for all the model simulations. This has been added and discussed in the revised manuscript.

12. P 10 line 6 this sentence is not necessarily self-evident, why?

Authors: We agree. The sentence has been rephrased and we made our statements clearer.

13. P 11, lines 9-13 – the term ‘period’ of the peak is a bit confusing, maybe use ‘duration or persistence.

Authors: This has been changed.

14. P 11 lines 23-24, stating the obvious?

Authors: We agree. However we decided to keep these statements since they summarize the importance of our findings in the first part of the manuscript and provide a transition to the second part that is on the synergy analyses. We provided arguments for choosing the wave-model run for validation analyses. This is now clarified.

15. P 16, lines 9-10 – using ‘coupled model not demonstrated here’ – don’t state this unless using reference to other work

Authors: Our apologies. This has been deleted.

16. Watch out for missing spaces between words in a few instances e.g. p5, p11

Authors: Thanks and we are very sorry for this. It has been fixed.