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Ocean Science Discussions

Interactive comment on "Ensemble hindcasting of wind and wave conditions with WRF and Wavewatch III® driven by ERA5" *by* Robert Daniel Osinski and Hagen Radtke

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Dear reviewer #2,

Thank you for your review and your comments. Additional supplemental material was prepared and uploaded regarding the calibration/validation procedure of the WWIII model and ensemble hindcasts of the storms Rafael and Toini. Please find in the following answers to your comments.

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1 Major comments:

This manuscript provides an interesting insight into possibilities of the construction of a large ensemble of hindcasts of wave properties in the Baltic Sea region. On the one hand, this approach is thought-provoking in itself as the pool of similar studies is very limited in this area. On the other hand, it is not clear beforehand how large is the potential of this approach to improve the hindcast as most of the discrepancies of the wave field reconstructions seem to stem from uncertainties of the driving wind fields. In particular, even small variations in the trajectories of low pressure systems may lead to large changes in the wave properties in the study area. It is thus important to understand how the possible uncertainties in wave reconstruction can be "distributed" between the variations in the driving fields and the specific ways of the description of wave physics. The topic thus clearly fits the scope of Ocean Science.

It is a pity that the approach is applied to an event in February 2002 for which essentially no ground truth about wave properties is available in the area of high waves. While thewave buoy of the Finnish Meteorological Institute was removed because of possible ice impact, the bottom-placed device at Almagrundet (Broman et al., 2006) did not provide any data in February 2002. However, as it is said both in Abstract and Conclusions that the event "provoked a severe storm surge in February 2002" it is necessary include at least some numbers and locations to substantiate this information. For example, nothing specific happened in Latvian waters.

Concerning the first remark about the applicability of this approach, it has to be mentioned that there is especially an interest from the insurance sector to produce large samples of historical events to get a more robust estimate of, for example, the 200 year return level as defined by the Solvency II directive. Often statistical methods are applied to enlarge the samples producible from datasets like reanalysis. Osinski et al. (2016) used the archive of EPS forecasts from the ECMWF to produce an enlarged ensemble of historical events. The problem with operational forecasts is the inhomogeneity and limited period. With our approach, ensemble hindcasts back to

1979 (eventually 1950 if ECMWF extends ERA5) can be created in a homogenous way. Our later application is a simulation of particle transport with an ocean model and a study of the impact of the metocean uncertainty on the transport pattern and amount of material.
Regarding your second remark about the missing observations, two storm events (Rafael and Toini) were hindcasted additionally. Information about the calibration procedure, validation of the model and the presentation of the two storm events is added in form of supplemental material. The results of the 2002 storm event were compared in the article to ERA5 wind and wave data. Based on a single event, it is not possible to judge if the ensemble spread is reasonable. For this reason, we compared it with the uncertainty measure provided with the ERA5 reanalysis to get a rough idea about it.

The method for the construction of the ensemble is rational and interesting. It is reasonable from the viewpoint of wind fields but seems to run into problems in terms ofwave properties. It is of course worth of trying to construct as large ensemble as possible in order to examine the spread. However, it is not a good sign that some membersof the ensemble lead to unrealistic wave heights. Both Fig. 2 and Fig. 3 indicate thatmaximum wind speeds in the northern Baltic proper are mostly in the range of 20–22m/s and only for a few members reach the level of 25 m/s. Such winds speeds onlycover a small part of the northern Baltic Proper. Even though the wind direction wasfavorable for the generation of high waves in this area, it is unlikely that significant waveheights substantially exceeded 7 m in this storm. Wave heights exceeding 8 m are veryinfrequent in this region. Even in the extreme storm Gudrun/Erwin (January 2005, 10-min wind speed >28 m/s in large sea areas) wave heights most likely did not exceed 10 m anywhere in the Baltic Sea (Soomere et al., 2008).
Therefore, I guess that wave heights between 11 and 12 m in Fig. 6 are completelyunrealistic for the February 2002 storm. It seems that the entire ensemble severely(by almost 2 m on average) overestimates wave heights in the northern Baltic

proper. Thus, I recommend to extensively comment this feature and to include a short insightinto measured or modelled wave heights in this area for storms of comparable prop-erties. Ideally, I would recommend to include a paragraph or two about extreme wave properties in the study area, following either (Tuomi et al., 2011) or (Björkqvist et al.,2018).

Calibration and validation of WWIII driven by the UERRA/Harmonie-v1 forcing dataset against observations and the hindcast of the additional two storm events showed that the waves predicted with the Baltic Sea setup with UERRA/Harmonie-v1 and the unperturbed WRF-ARW hindcasts show reasonable wave heights. Our WWIII setup was calibrated with UERRA/Harmonie-v1 forcing, for this reason it was checked whether this calibration gives also reasonable results when driven with WRF-ARW. The wind in WRF-ARW is slightly stronger over land and near the coast, as it can be seen in Fig. 4. We adapted the roughness length over land according to the Corine land cover data set, but this gives only a small effect for the waves in the western part of the Baltic close to the land masses. The roughness length over the sea surface is assumed to be constant in the applied WRF-ARW setup. Under severe storm conditions, the roughness of the sea surface should increase in reality, resulting in a reduction of the wind speed due to higher momentum transfer. Reduced wind speeds limit the growth of the wave height. With a coupled WRF-WWIII setup, this effect could be taken into account, in our setup it is neglected. Perhaps this is one reason for the extreme wave heights in some representations. Based on one extreme event, it is also not possible to tune the perturbations of the model physics. This is why the WRF-ARW ensemble is potentially overdispersive, which we also mentioned in the manuscript. As can be seen in Figure 4, the wind speed over the Baltic proper in the extremest representations is above 28m/s. The time series shown in Figure 2 is at a different location. We will make this clearer in the manuscript and will discuss it more in detail that the extreme representations are potentially unrealistic. As the two additional storms were hindcasted with a 7km newer WRF-ARW version, which we will apply for our later application, a recalculation of the 2002 storm shows a maximum hs

of 9.5 m from an 11-member ensemble. The spread is larger than for the other two storm events which shows that the event is much more sensitive to perturbations.

In particular, I recommend extending the message on page 5, line 5–6 towards a sound explanation that the model is essentially uncalibrated for the Baltic Sea conditions. This is mentioned in the last sentence before conclusions on page 14. The point of this sentence should be made very clear from Abstract to Conclusions. I stress that such a bias in the evaluated wave heights does not undermine the validity of most of the results but it should be made clear to the reader that single values of wave height (and even the ensemble average) do not necessarily match the wave properties in this storm.

We will refer to the supplemental material and make it clearer that the WWIII setup was calibrated for the application with UERRA/Harmonie-v1, but a test with WRF-ARW wind also shows a reasonable performance. Concerning the extreme representation in some ensemble members, an additional discussion will be added about the uncalibrated ensemble spread in the WRF-ARW ensemble and about the fact that the effect of the roughness of the sea surface is not taken into account in the applied WRF-ARW setup.

For the listed reasons I recommend moderate to major modifications to the manuscript. It is essential that the reader is informed (i) about some basic features of wave climate and extreme waves in the Baltic Sea and also (ii) that the simulations probably strongly overestimate wave heights and (iii) are performed specifically to study the spreading properties of ensembles, with no exact relevance to the actual wave heights during the simulation interval. An absolute must is to inquire the modelled data from a properly calibrated run (e.g., from the authors of Björkqvist et al., 2018) for the underlying location of Fig. 6 to give a minimum flavor of the possible bias.

As proposed by both reviewers, additional information about the wave climate in the

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Baltic Sea including citations to existing studies will be included into the introduction together with the mentioned points from the previous remarks about the potential overestimation of the spread in the atmospheric ensemble data resulting in potentially unrealistic wave heights in some members. We believe that a more detailed comparison to observations makes an inter-model comparison no longer a requirement.

The text is written in fairly good English but reveals slight German accent in the form of very long sentences at places and missing of some articles in the text. It is mostly clear but still needs extensive polishing, especially closer to the end of the manuscript. As I am not native speaker, I only include a list of clear typos below.
We will revise the text.

2 Minor comments:

The paragraphs are at places very long. For example, the first paragraph of Introduction extends over 28 lines. It is recommended to split long paragraphs into shorter ones.
The paragraphs will be splitted into shorter ones.

The style of calendar days ("21. February 2002" on page 6, line 4 and "22nd to 24th of February" on the next line) should be unified.
The style of the calendar days will be unified.

The first two sentences of Abstract seem unnecessary
The second sentence explains issues in wave modelling and is required for the third sentence which claims that we address these by the presented method. The first sentence shall put the second one into context. We believe this sort of introduction is

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required to grasp the intention of the manuscript.

Page 1, line 17: probably should be “and is described“.
Will be changed to “and is described“.

Line 23 and some other locations: some journals require comma after "e.g.“
Will be revised according to the requirement of the journal.

Page 2, lines 32–34: the sentence does not make sense; possibly because of too strong German accent.
Will be replaced by "‘ At the moment, the ensemble datasets in this project are limited in their temporal coverage or spatial resolution. It can be advantageous to be able to produce hindcasts of events whose spatiotemporal resolution is adapted to the requirements defined by a research objective."’

Page 3, line 13: C3S has already been explained on page 2, line 22.
Only the abbreviation will be used here.

Line 20: probably full stops are not necessary in "21. February 2002“ and similar expressions.
Will be revised.

Line 23 it is better to say that 0.36deg and 1deg denote the resolution of the relevant grid. Please do so also in several locations below where the size in degrees is given without any explanation.
Will be adapted.

Page 4, line 10: please specify the meaning of "writing 15 minutes output".
Will be changed to: "and the model output interval is 15 minutes."

Line 12: please explain what is meant under "the temporal impact" (probably the dependence of the solution on the time step).
Will be changed to: "the dependence of the solution of the wave model on the temporal resolution of the wind data."

Line 17: please specify the meaning of "Eta layers".
Will be explained as a specific vertical coordinate system used for atmospheric models.

Line 18–19: consider replacing the jargon-like expression "until fine scales develop" by a more explanative one. Please do so also in several occasions below to avoid clash in the meaning of, e.g., "finer scales are not represented" on page 6, line 9.
We will replace "'scales"' by "'structures"' to avoid jargon.

Page 5, line 1: to avoid misinterpretation, I suggest to mention that nesting of the wave model to the Baltic Sea is not really necessary for the hindcast of wave properties in the central and northern regions of this water body because very little wave energy penetrates through the Danish straits.
The wave model output will be used as input for a model of the entire Baltic Sea which also covers a part north of the Danish straits. In this region, we also want to have reasonable wave parameters. This will be made clear in the manuscript.

The reasoning on lines 2–6 is only partially relevant for the conditions of the Baltic

Sea.
It explains the procedure used for setting up the wave model. ERA5 is a global reanalysis and the procedure could be applied also to other regions in the world.

Line 7: while most of the model setup is obviously fine for the Baltic Sea, please com-ment on the adequacy of the use of the chosen frequency range for this water body. Wave modellers usually substantially extend the frequency space here. The team of the Finnish Meteorological Institute normally uses 35 frequencies (Laura Tuomi et al.,many papers) and some research in subbasins of the Baltic Sea even 42 frequencies (0.0418–2.08 Hz, Soomere, 2005). It is probably not necessary to cover such an extended range. However, insufficient coverage of short waves may lead to too slow wave growth under rapidly increasing wind conditions.
The discretization with 42 frequencies (0.0418–2.08 Hz, Soomere, 2005) together with a finer resolution of the directions (36 every 10deg) was tested. In the supplemental material, the outcome is visible. It brings additional 10cm in the significant wave height for the Rafael storm, which is underestimated by about 90cm with the UERRA/Harmonie-v1 wind. The shortcoming of this finer discretization is a prolongation of the calculation time, which was 4 times of the one with the ERA5 equivalent discretization. For computational reasons, we used the ERA5 discretization.

Lines 14–16: the message of the entire sentence is technically clear but seems misplaced or even irrelevant.
Will be revised.

Page 7, line 7: "these".
Adapted.

Page 10, lines 19–20, the sentence “Compared to ERA5, the overall spatial pattern is comparable“ does not make sense to me.
Will be replaced by "‘The overall spatial pattern of the significant wave height is comparable between ERA5 and the WRF ensemble members."’

Page 11, lines 2–5: the reasoning is almost irrelevant for the Baltic Sea conditions andshould be left out. Instead, it should be emphasized that strong swells are infrequentin the Baltic Sea (see, e.g., Broman et al., 2006; Soomere et al., 2012) and thus deviations in the hindcast or forecast driven by the accuracy of the representation of swells are usually not very large in this water body.
We see the manuscript as a demonstration for the procedure to produce ensemble hindcasts. ERA5 is global and the procedure is applicable in general worldwide. This is why we also have to mention potential shortcomings if applying the procedure to other regions. We will add a subsentence "‘, which should, however, be more relevant for different regions of interest where swell plays a larger role."’

Page 12, line 14: something is wrong with "500 choose N possibilities exist”.
This is an expression from stochastics, we will replace it by the mathematical notation $\binom{500}{N}$ to avoid confusion.

Page 13, line 10–12: the sentence is unclear.
Will be revised.

Line 13: “developed”; also, the entire sentence remains partially unclear starting from “why”.
Will be revised.

Lines 16–17: the concluding sentence of the subsection should be made clearer.
Will be changed to "'Depending on the application, the ensemble size needs to be selected by a compromise between the robustness of the uncertainty estimate and the computational cost."'

Line 18: use "on" instead of "onto".
Will be changed.

Page 14, line 7: please specify what is meant under "The higher temporal resolutions do not differ so much." Also, the subsequent sentences contain too much jargon.
Will be revised.

Line 14: "orography of the coastlines" sounds weird as the height of the coastline is just zero; also: use "Baltic Sea".
Will be revised.

Line 15: spatial pattern of what?
of the significant wave height.

Line 5 or another appropriate place: please stress that an uncalibrated (for the Baltic Sea conditions) wave model was used but still the results about the spread are valid.
We will refer to the supplemental material and the issue with the spread will be discussed there.

Page 16, line 1: remove "by this fact".
Removed.

Lines 1–2: the message of the sentence "As the first twelve hours are not used, be-cause of the model spin-up, this is not really a shortcoming." remains unclear.
Will be revised.

Line 14: correct "atmopsheric".
Corrected.

Line 20: correct "possbile".
Corrected.

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-76/os-2019-76-AC2-supplement.pdf

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