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Interactive comment on "An atmosphere-wave regional coupled model: improving predictions of wave heights and surface winds in the Southern North Sea" *by* Kathrin Wahle et al.

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Answers of the Reviewer #1 comments

Dear Hans de Vries, Thank you for reviewing our manuscript and for the constructive comments and suggestions. In the revised manuscript your comments and suggestions for improvements have been carefully considered.

R#1: The subject of this manuscript is the 2-way coupling of atmospheric and wave models in the German Bight. Atmospheric forcing is supplied to the wave models, and the wave model sends back the surface roughness to the atmospheric model. The results of 2-way coupling are compared to 1-way coupling for a 3-month period

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that includes one of the most severe storms, named Xaver, in the last decades. The subject is highly relevant and the technique promises a seizable improvement of wave forecasts in especially shallow areas with complex topography. And the authors indeed show an overall improvement of wave forecasts and also in particular for the Xaver storm.

Authors: Thank you for this nice appraisal.

R#1: It is, however, sometimes difficult to get to the message the authors try to convey. One of the reasons are the many errors in the english language, especially incorrect or missing articles, inconsistency between singular and plural, inconsistency in tenses, and missing commas. The manuscript should therefore be carefully checked and corrected.

Authors: The revised manuscript has been carefully checked by a native speaker, typos and errors in English language have been corrected.

R#1: Chapter 3, on results, is very fragmented and lacks a clear wrap-up and conclusion at the end. Moreover, especially Section 3.1 is very long and deals with a number of more or less separate items. It would help to put these in separate subsections. Sometimes the conclusions are contradictory to what the figures or tables suggest.

Authors: We agree and the revised manuscript has been re-structured. Chapter 3 with the results has been divided in two new Sections: Chapter 3 dealing with validation and Chapter 4 discussing the impact on wave-atmosphere coupling (one-way versus two-way). The new section 3 “Validation” has been also divided into three sub-sections describing separately and providing more analyses on the validation of altimeter data against in-situ data; model validations against satellite data and model validation against in-situ measurements. We think that the manuscript now is better structured.

R#1: Comments in more detail: 1. Introduction The reference Lionello (2003) is not

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in the references list. But the remark about what it states seems very odd here. The formulation suggests that the 2003 paper already describes the current work.

Authors: The Introduction section has been re-structured and carefully revised (following also a similar comment of the Reviewer's #2). The state-of the art has been better presented. More information about the previous studies has been provided (incl. Lionello, 1998, 2003). The novelty in our work compared with previous studies has been described and argumentsfor performing our study has been presented.

R#1: Towards the end, Staneva et al. (2016) is referenced. I would say that the subject of this paper has more relevance to the present paper than just the fact that wave heights are overestimated or the description of the used models. I would expect a discussion on coupling just waves and the atmosphere and including also circulation. And in the end you will probably want to couple all three together.

Authors: e agree and in the revised manuscript a comprehensive discussion about the coupling between waves and hydrodynamics (referring also to our recent developments in Staneva et al. (2016) have been included. The perspectives and future plans towards implementing a fully coupled atmosphere-wave-circulation model, based on the developments and findings described here, as well as those by the wave-hydrodynamic coupling studies, have been discussed.

R#1: 2.4 Integration Period and Data Availability At the end, you refer to Figure 1b for the wave rider buoys, but they are in Figure 1c.

Authors: We apologize for this mistake and we refer to the right Figure in the revised manuscript. Even more, we removed from Figure 1, the figure showing the satellite tracks (old Fig.1b) and the new Figure 1 shows only the domain and bathymetry of the model areas.

R#1: 3.1 Validation of models As in the final paper the tables will be close to the text, you might consider leaving out the values themselves here. It would make the text

more easily readable.

Authors: We agree and left out the values that are given in the Table from the text while discussing the validations. We additionally reformulated this description in Section 3, making it clearer with additionally stressing on major conclusions from each sub-section.

R#1: Line 221: "due to the reasons explained above" is not clear which reasons you are aiming at.

Authors: We agree and changed accordingly. Similar comment has been also pointed at by the second reviewer, the revised text is re-phrased and we clearer refer to the introduction.

R#1: Line 230: Change "It is well known that..." into "Passaro et al. (2014) established that...".

Authors: The suggested revision has been made.

R#1: Line 240: "... wave heights are in good agreement." I do not agree. In the calm case, both models underestimate the wave height by approximately 1 m over a large part of the track.

Authors: The suggested revision has been made. We discussed the comparisons commented between the model and satellite wave heights and commented the discrepancies.

R#1: Line 242: "... however,[!] the reduced wave height...". Change "however" into "although". Differences between the models are much smaller than the difference with the observations.

Authors: The suggested revision has been made. We agree with this comment – the differences between the model runs are most significant and indeed smaller than between model and observations. This has been now revised in Section 3 and also

stressed in the discussion section.

R#1: Line 246: The peak of the storm, at least the highest wave heights, are at the edge of the domain of the wave model. Any differences with observations will therefore strongly be influenced by the boundary conditions. And, actually, Figure 3b does not show a maximum, just an increasing wave height towards the North.

Authors: We agree with the comment and the discussion of the comparisons (Fig.6a in the revised manuscript) has been re-phrased, making it clearer. The maximum that we referred is observed by the satellite data. Indeed for the both model runs the wave height increases northward. This has been additionally commented in Section 3.2.

R#1: The comparison of these two tracks is a very useful illustration. You must have looked at the other tracks as well. Without giving any details here, it would be relevant to say something of the general picture that emerges from that. Does it agree with these two examples, or are there also other features there?

Authors: We looked also at many other tracks and the results were similar to the ones for the calm situation. In general, the measured wind speeds were in slightly better agreement with the modeled ones as can be seen also from the statistics in Table 1. The track for storm 'Xaver' was the only one taken under such extreme conditions. A discussion on this has been added in Section 3.2.

R#1: More or less the same remark on the comparison with the wave buoys. You show Helgoland and Westerland, but you should at least mention whether the results for Fino and Elbe are similar.

Authors: We have also looked at the comparisons between in-situ and modelled data for Fino and Westerland stations. Those comparisons look similar to what we show. The general picture and the conclusions agree. We added additional discussion on that, as well in Section 3.3.

R#1: Line 280: As the results for both wave models are different in shallow water, what

does that mean for the 2-way coupling? Should you not get sea surface roughness from the model that includes wave breaking?

Authors: We agree with the comment that it is difficult to differentiate between effects coming from wave breaking and from two-way coupling. This has been thoughtfully discussed in Section 3.3 and additional references have been provided.

R#1: Line 289: "were provided by the DWD" is a remark that should be in Section 2.4.

Authors: The suggested revision has been made.

R#1: Line 296: "Even though differences ... decrease ..." That is not what I see. Differences in biases in Table 2 are not really different between 50 and 100 m and the difference in standard deviation is even larger.

Authors: We apologize for this incorrectness and fully agree with the comment. Only the bias slightly decreases. The description of those results has been corrected in the revised manuscript and we made the explanations clearer.

R#1: Line 303: The word "either" gives a choice between two possibilities. So it is not correct where you want to combine 4 things.

Authors: The suggested revision has been made.

R#1: Line 307: It would help to give RMSE also in Table 3, if you refer to that instead of the standard deviation.

Authors: That was a mistake in writing and we changed ‘rmse’ into ‘standard deviation’ in the text.

R#1: 3.2 Impact The first paragraph suggests that in 1-way coupling the coupling to the waves is too strong. If you would decrease this coupling, e.g. by a smaller Charnock constant, would that not give similar results for the waves? Then, what is the added value of the 2-way coupling?

Authors: We agree that by calibrating the parameters one can achieve better results compared to the measurements even using only a one-way coupled experiment (e.g. as in Zweers et al., 2002). This has been discussed in the instruction in the revised manuscript. However, the aim of our work was also to perform a process oriented study by considering the feedback of the bottom roughness by the sea state dependence of the surface stress via the two-way atmosphere-wave coupling. These developments were needed for future extension of the coupled model system by integrating atmosphere-wave–current interactions to further investigate the effects of coupling, especially during extreme storm events.

R#1: You claim that the argument you infer from Figure 5 for wind speed differences is supported by the effect on wind stress (Figure 6). But the wind stress has a rather straightforward relation to the wind speed, so this is really the same argument. Just the fact that the wind stress is more that quadratic in the wind speed makes the effect only seem stronger.

Authors: We agree and the suggested revision has been made. In the revised manuscript we removed the redundant figure with the horizontal patterns of wind stress rmse and bias (Figure 6 in the first submission).

R#1: Line 332: "... which tends to fill the low" is not what Janssen and Viterbo (1996) claim. They claim that the disturbance will grow less, what is not the same.

Authors: We agree and this has been re-phrased in the revised manuscript.

R#1: Line 336: "... indicates a shift of the pressure low minimum". That should be easily seen directly in the pressure fields. Why then an indirect argument?

Authors: We agree with the comment. More information has been added in the discussion of the pressure filed in Section 4.

R#1: Line 340: "such effects". What effects?

Authors: The suggested revision has been made. We changed this into ‘to observe

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this impact'.

R#1: Figures: The use of figures with several subfigures is not always an advantage. Some of the graphs, especially time series in Figures 3, 4 and 8 are already small and will be smaller even and more difficult to read in a printed version. The authors might want to split some apart.

Authors: We agree and the organization of the figures has been changed in the revised manuscript. The sub-plots have been split in different figures. The figure patterns were made larger. Additionally, the quality of the individual figures has been improved. Some of them have been re-ordered following the logics in the text.

R#1: Figure 1: The explanation is not logical: first 1c and then 1b. Figure 1b: I am not sure if a figure with all of the tracks is useful. They are not at the same time, and it is rather obvious that the pattern would look like that. Figure 1c: The name Westerland is unreadable.

Authors: We agree – actually we removed Figure 1b with the satellite tracks from Figure 1. The size of the figures has been changed and their quality improved.

R#1: Figure 2: Units are missing on the wind speed scale.

Authors: The suggested revision has been made. We added the missing units to the figures caption and in the figure.

R#1: Figure 3: The subfigures are not really similar enough to combine all of them. 3c and 3d could be taken together, but the way in which they are presented now suggests that 3a belongs to 3c and 3b to 3d. As 3c and 3d are mentioned first in the text, they should be before 3a and 3b. It would be useful to limit the area of Figure 3a to the same area as Figure 3b. Now a comparison is difficult. It would help to indicate the buoys in 3a and 3b. The color yellow in 3c and 3d is hardly visible.

Authors: The suggested revision has been made. We made two separate figures out of it: first one with the comparison of measurements and model results and a second

one with the distribution of Hs in the model area. We also separated the figures for the different events. The quality and presentation of the figures (lines, colors, fonts) has been improved. The Figures have been then re-numbered.

R#1: Figure 5: The use of "bias" and "rmse" in this figure is confusing, as there terms are mostly used to indicate the difference with observations. Suggestion: "average difference" and "RMS difference". Also in Figure 6. The figure might be explained more clearly. Either in the subscript, or in the text.

Authors: The suggested revision has been made. We introduced the terms as suggested in the text and in the figures caption and tried to explain more clearly what is shown in the figures caption.

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