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comment**

Interactive comment on "Coastal flooding: impact of waves on storm surge during extremes. A case study for the German Bight" *by* Joanna Staneva et al.

Joanna Staneva et al.

Joanna.Staneva@hzg.de

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Answers of the reviewers' comments

Reviewer #1

General Comments As a detailed assessment of a coupled high resolution wave-ocean modelling system's sensitivities in an extreme case, this paper provides a useful addition to existing published evidence regarding coupled systems and is a valid extension of the work in Staneva et al, 2016. I would therefore recommend this paper for publication, but with some additions/corrections related to the points below.

Specific Comments

Section 2.1 Whilst this information may well be published in the authors' previous papers, it would be useful to those reading this paper in isolation if some extra details on the update frequencies of atmosphere and river forcing data were provided.

Authors: More information about the model setup, including a description of the open boundary forcing, atmospheric forcing and river runoff, has been included in Section 2.1. Additional references were also added.

Section 2.2 This is an extreme case in shallow water, so please could the source term parameterizations for bottom friction and depth induced breaking dissipation that were used in the wave model be stated?

Authors: Additional information about the parameterizations used in our model setup, including more references, was provided in Section 2.2.

Section 2.3 I found the statements that "<u> is the sum of the Eulerian current and the Stokes drift" and "Thus the divergence of the radiation stress is the only (to second order) force related to waves in the momentum equations." somewhat contradictory. In the equations, Mellor (2011) has been followed correctly and I see the basic point about radiation stress being the difference between coupled and uncoupled systems, so just wondering if the authors can review the text in this section for clarity.

Authors: We apologise for the confusion we created with this mis-formulation and completely agree with this comment. As described in the text, we follow the procedure of Mellor (2011). This inconsistency was also mentioned by reviewer #2. Both suggestions are exactly what was used in our study. We corrected the text regarding the statement of <u> accordingly, and a clearer explanation is given in the revised manuscript.

Section 2.5 As per the comment for section 2.1, can the frequency of coupling fields exchange be added please? Authors: Additional information about the coupler, coupling fields, etc.-, including references, has also been provided in the revised manuscript

Also, please note in Table 1 whether the NORIV wave model is one or two way coupled for consistency with the rest of the table.

Authors: We agree and modified Table 1 to specify that NORIV is a two-way coupled model, making the third column consistent with the rest of the text.

Section 4.1 It's not clear whether the wave model discussed in this section and associated figures is the two way coupled version or the stand alone wave model. Can this be made more explicit?

Authors: This point has been clarified in Section 4.1.

Section 4.1, p9, line 9, Looking at the figure I get the impression that the peak of the storm is simply mistimed rather than over predicted, unless the authors are discounting the measured peak for some reason. Please check.

Authors: We agree with the statement that the peak of the storm is slightly mistimed rather than over-predicted, as shown in Figure 4, and this has been changed accordingly in the revised manuscript.

Section 4.2 and later discussions. If I understand this correctly, the surge residual is defined by subtracting the same predicted tidal residual (generated via the T_TIDE package) from observations and model alike. The model residual is therefore a combination of both the model error in background tide prediction plus error in the surge prediction. In that case, I think it is important that any known systematic error in the model tide is stated in order to contextualise the benefits of the wave coupling. If these errors are not well understood, then I would recommend that the potential errors associated with the model tide are acknowledged and caveated in the discussion.

Authors: We agree that the nonlinear interaction of the storm surge signal with the systematic error in the tidal simulation may have an effect on estimating the difference between the observed and the simulated surge signal. We provided further clarification in Section 4.1 and in the discussion section.

Section 5.1 Please comment on whether the coupling improved results at all individual stations, or just most of them...

Authors: This topic has been discussed in greater detail in Section 5.1.

Section 5.2 Regards the comparison with the barotropic model: 1) One of the arguments presented by the authors relates to large scale inter-annual effects on background water level, which a barotropic model will not deal with; this is correct, but can be mitigated to some extent if the predictive system for water level comprises an astronomic prediction of water level based on observations (which will include these long term effects) plus the barotropic model's estimate of the surge residual - this approach is adopted operationally in the UK for example. In terms of this paper one question for the authors to address is whether they believe that these effects are not present in the T_TIDE data used to calculate the residuals they show?

Authors: The tidal analyses in the present study consider the bias and linear drift of the tidal signal, which for the length of analysed period, a few days, may be sufficient to fit the large-scale annual and interannual signal of the background water level. However, we agree with the reviewer that for the analysis of longer periods a more sophisticated approach is advisable.

2) Of more importance, the barotropic model presented does not include any barotropic coupled effects (which might be included due to both waves radiation stresses and also water volumes associated with river inputs?) - however, the text implies that the main difference is baroclinicity. In order to make this argument better it would be good if the authors could present why they believe that introducing some coupled processes to the barotropic model would not close the gap between this model and the FULL run?

Authors: Yes, when analysing the role of baroclinicity, we used the barotropic model that was not coupled to the wave model. The aim of our sensitivity studies was to demonstrate the individual effects of coupling with waves and baroclinicity separately. We agree that to some extent the introduction of coupled processes of the barotropic

model would partially reduce the gap between this model and the FULL run, which is discussed in Section 5.2. The possible advantages of including the wave-current interactions in the 2-D models to improve the sea level predictions were also addressed in the discussion.

3) Finally, please check Figure 14, where the surge line for station ST3 does not look consistent with that in Figure 10.

Authors: We apologise for the incorrect Figure 14a and thank you for noticing the error. In the revised manuscript, the correct Figure 14a has been included.

Section 6 Items to consider for addition to the discussion: 1) the wave model, via the atmosphere model I expect, has over-predicted during period T2 and then been about right for period T3. In support of the comments regarding atmospheric uncertainties, how did the comparisons of modelled and observed surge vary during these periods for the FULL run?

Authors: We agree with the suggestion and added comments on this issue in an additional paragraph in the discussion section.

2) in these simulations, there is no feedback to the atmospheric model from the waves, so the coupled system is not fully closed. In terms of the argument being presented here, where the waves are strongly affecting the ocean model in a shallow water region, I'd imagine that the sensitivity to the atmosphere-wave-ocean coupling is not too big a consideration at these scales; however, it might be useful to acknowledge this point more than has been presently done on line 9,p15.

Authors: We completely agree. The atmosphere-wave (COSMO-WAM) interaction is a subject of another study (Wahle et al, 2016). Our aim is to study and understand the wave-current interactions (the current manuscript) and wave-atmosphere interactions separately for our coupled model system before proceeding to fully three-way atmosphere-wave-ocean interactions. The latter will be the subject of forthcoming de-

velopments and studies. We included an additional paragraph addressing this issue.

3) Is it possible for the authors to discuss/speculate further on the role and potential uncertainties of the shallow water terms in the wave model? My impression in this case is that the region with strongest wave-ocean interactions will see strong contributions from these terms in such a large storm and shallow depths.

Authors: The role and potential uncertainties of the shallow water terms in the wave model have been discussed in the final section.

Technical Comments/Proposed text corrections

Page 2, para 2 There are a number of typos and the grammar could be improved significantly in this paragraph and, if kept, the authors need to review this carefully.

Authors: We completely agree and carefully revised the manuscript for typos English grammar.

However, in the context of the paper I think that the arguments being made about climate changes effects and other reasons for improving model accuracy can be taken as read (or just briefly expanded upon in the first paragraph); so I'd suggest removing this paragraph altogether.

Authors: We agree with this comment and removed this part from the Introduction.

p2, line 2, "predictions of the sea" -> "predictions of sea" Authors: The suggested revision has been made.

p2, line 6, "demand of improving" -> "demand for improving" Authors: The suggested revision has been made.

p2, line 8, "role at shallow area" -> "role in enhancing sea-surface elevation in shallow water areas" Authors: The suggested revision has been made.

p2, line 12 "mixing to circulation model." -> "mixing to a circulation model." Authors:

The suggested revision has been made.

p2, line 17 "of biogeochemical" -> "biogeochemical" This has been re-phrased, following Reviwer#3 comment

p2, line 18, "radiation stress that accounts" -> "radiation stress accounts" Authors: The suggested revision has been made.

p2, line 20, "by number of studies like" -> "by a number of studies, such as" p2, line 30, "distributions s" -> "distributions is" Authors: The suggested revision has been made.

p3, line 30, "area are substantial hazard" -> "area are a substantial hazard," p3, line 31, "The coastal" -> "Coastal" Authors: The suggested revision has been made.

p4, line 22, "in details" -> "in detail" p4, line 24, "are-sea" -> "air-sea" Authors: The suggested revision has been made.

p6, line 10, "equation of motions" -> "equation of motion" p8, line 1, "The effects on using" -> "The effects of using" p8, line 6, "rive" -> "river" Authors: The suggested revision has been made.

p8, line 11, "regions available" -> "regions are available" p10, line 4 "in good consistency" -> "are consistent" p10, line 10, "analyses on model" -> "analyses of model" p10, line 13, "new examples on" -> "new examples of" p10, line 28, "on Tables 2" -> "in Table 2" Authors: The suggested revision has been made.

p11, line 14 "reached reaching" -> "reached" p12, line 16, "comparissons" -> "comparisons" p13, line 11, "in direction to" -> "directed toward" p13, line 31, "Even more" -> "Furthermore" Authors: The suggested revision has been made.

p14, line 7, "the three dimensional model" -> "three dimensional models" p14, line 30, "hazard is" -> "hazards are" Authors: The suggested revision has been made.

p15, line 1, "has been gradually mature" -> "has gradually matured" p15, line 2, "defective to satisfy the" -> "unable to fully satisfy these" p15, line 3, "the real time storm

forecasting" -> "atmospheric storm forecasting" p15, line 3, "is not perfect in practical use." -> "is not perfect." Authors: The suggested revision has been made.

p15, line 4, "It always" -> "This" Authors: The suggested revision has been made.

p15, line 5, "is depended" -> "depends" Authors: The suggested revision has been made.

p15, line 5, "accurate landfall position" -> "accurate prediction of landfall position" p15, line 6, "tide may has a huge tide range" -> "tide may have a huge range" p15, line 7, "forecasting cause" -> "forecasting can cause" Authors: The suggested revision has been made.

p15, line 9, "increasing the knowledge on" -> "increasing knowledge of" p15, line 10, "weather forecast" -> "weather forecasts" Authors: The suggested revision has been made.

p15, line 13, "that wave-dependent approach yields to 25% larger surge at" -> "that the wave-dependent approach yields a 25% larger surge over" Authors: The suggested revision has been made.

p15, line 14, "German Bight reaching a contribution of about 40% is some coastal area" -> "German Bight and reaching a contribution of about 40% in some coastal areas" Authors: The suggested revision has been made.

p15, line 15, "The contribution of the fully 3-D model in comparison with a 2D barotropic one yield to" -> "The contribution of a fully coupled 3-D model in comparison with an uncoupled 2-D barotropic one yielded up to" Authors: The suggested revision has been made.

p15, line 23, "demand of disaster relief" -> "demands of disaster relief" Authors: The suggested revision has been made.

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2016.