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Discussions

Interactive comment on "Parameter sensitivity and uncertainty analysis for a storm surge and wave model" *by* L. A. Bastidas et al.

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Response to Comments by Anonymous Referee # 2 General comments

âĂć The paper reviews the sensitivity of storm surge and wave models to different parameters, what is in principle an interesting and useful analysis for improving performance of these models for operational sea level forecasts as well as for sea level hazards risk assessment. However, it seems more emphasis is made on this than on the real capability of the model to reproduce both the sea level and waves observations. In fact, this capability seems to be poor in general and does not improve when increasing the resolution of the model, what is not really explained in detail. In that sense, if sensitivity to the parameters is the only objective of the paper it should be

clearly stated in the introduction. From a practical point of view, however, if validation with observations is presented, this should be the focus of the work and the most important goal at the end.

We have carried out an analysis to not only analyze the capabilities of the Delft3D model to reproduce actual observations but also to establish which parameters have a significant influence on the water surface elevation and wave height simulations specifically. We have also studied the influence of three different wave field formulations (see response to next comment) on the model simulation abilities. At last the impact of the model resolution was also studied. In fact, somewhat unexpectedly, it was concluded that the increase in resolution does not necessarily results in better simulations, although it is not entirely clear why.

“Parameter sensitivity” are the first words in the title of the paper. It was our main goal to establish which of the parameters lead to a significant improvement of model performance and the corresponding uncertainty that can be ascribed to them.

We believe that we have made a contribution that is applicable to a variety of models given that most of the parameters have a “physical meaning” and are not only “functional” parameters. It is clear, however, as it has been proved time and again in many different model intercomparison studies that one has to be careful about the extrapolation/transfer to other models of the results.

âĂć In this sense, looking for the more adequate meteorological forcing should be the first goal and, once found, the sensitivity parameter response of the models could be made just based on this “valid” meteo forcing.

The meteorological forcing used was the “best” available from public sources. That available information is not sufficient to drive the model though. For that reason we have analyzed three different formulations (models) of the wind fields, required to drive the model, and study their influence on Delft3D’s ability to match the observations. We make some conclusions about the quality of the results obtained from each of the three

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different, and widely used, wind field formulations.

Thus, the “reviewer’s first goal” was actually looked at in the paper.

Specific comments

âĂć Section 2.1, p.6 line 25: “few coastal surge observations at tidal stations (NOAA, 2015b)”: is this true? Are the authors just referring to very extreme surges? Tide gauges in principle have been in operation for so many years that this sentence sounds strange. I did not find the report from NOAA according to the reference.

We did check for available information concurrent with the simulated event, Hurricane Bob, and use all the available information.

There is no report in reference NOAA, 2015b. It refers to the NOAA Tides and Currents Websites, where the data about Tides is available. NOAA, 2015c, refers to the NOAA National Buoy Data Center with information regarding buoys.

âĂć Section 2.1, p.7, line 10: “the past 60 years of coastal surge likely do not contain an observed storm surge . . .”: what about Sandy in 2012? There was flooding at least during this event and is very recent. Why is not mentioned in the text?

Sandy was a category 1 hurricane when it hit the Northeast US coast. That is one of the reasons for choosing Bob as the event for the simulation. It is the only hurricane of category 3, in that region, with available information for winds and tide and wave observations.

âĂć Section 2.2, p.8, line 26: how many and what (source) tidal constants are imposed in the open boundaries?

The following 12 modes are considered:

Semi diurnal modes: M2: principal lunar S2: principal solar N2: elliptical lunar K2; declination lunar solar

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Diurnal modes: K1: declination lunar solar O1: principal lunar P1: principal solar Q1: elliptical lunar

Long period modes: MF: fortnightly lunar MM: monthly lunar SSA: semi-annual solar

Tide harmonic constituents were extracted at locations on the open boundaries of the coarse domain from TPXO7.2 tidal data solution (OSU, 2014). TPXO7.2 is a current version of a global model of ocean tides, which best-fits, in a least-squares sense, the Laplace Tidal Equations and along track averaged data from TOPEX/Poseidon and Jason altimeters on TOPEX/POSEIDON tracks since 2002. TOPEX is the topography Experiment for Ocean Circulation and the TOPEX/POSEIDON mission is Joint US NASA - French orbital mission to track sea level height with radar altimeters.

OSU, 2014, Oregon State University (OSU), "The OSU TOPEX/Poseidon Global Inverse Solution TPXO," Available at: http://volkov.oce.orst.edu/tides/global.html

âĂć Section 2.2, p.9, line 21: it seems the gravitational constant is treated here as a parameter of the model. It is a datum, a known value. It is not possible to say "to vary physical settings" applied to it.

Yes, it is a datum, a known value. However it changes slightly based on the latitude, around 0.5% from equator to poles. That is why the standard value of the gravitational constant is defined at sea level and at a latitude of 45 degrees. We are simply describing the possibilities included in the model. The value used was 9.806 m/s2, i.e. the standard value.

âĂć Section 2.2.2. p. 11, lines 8-9: "As we have selected a model grid resolution of 5 km, the horizontal eddy viscosity should be a significant consideration": it seems a vague sentence, specify here how this parameter varies with the resolution or grid size if this parameterization is used in this study.

The horizontal viscosity is included as one of the parameter set for analysis, i.e. is varied according to the Morris algorithm to study its influence. That is stated in the text

in the same paragraph. A reference to the model manual is made for further details if the reader (reviewer) is interested.

The text has been modified to say:

“These additional hydraulic losses are accounted for within Delft3D simulations through modification of a horizontal eddy viscosity term . The larger the model grid, the more the smaller losses are neglected. The horizontal eddy viscosity term is considered a calibration parameter for Delft3D-FLOW which is commonly a function of the model grid size (Deltares, 2014b). As we have selected a model grid resolution of 5 km, the horizontal eddy viscosity should be a significant consideration and is included in the sensitivity analysis.”

âĂć Section 2.4, p.15, lines 20-25: it is contradictory: it says that there are no wave measurements at the tidal stations but the effect of model parameters on peak significant wave heights are evaluated: in this sense, it should be said: “we evaluate the effect of model parameter values on simulated peak significant wave heights, although we can’t validate them with observations”. Other possibility is to evaluate their effect on the peak observed and simulated storm surge.

The reviewer is not stating the complete sentence. The complete sentence already present in the paper addresses the reviewer’s concern:

“Though no measure can be given for reproduction of observed wave characteristics at these locations, we evaluate the effect of model parameter values on peak significant wave heights.”

âĂć Section 3.4: a more detailed analysis of the discrepancy between models and observations should be made, e.g., why the tidal amplitude is so different in Bar Harbor?. Has the bathymetry, for example, been checked?

The text has been changed to say:

“The only location with a significant improvement, over the coarse resolution, in model

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performance is Bar Harbor. The RMSE and MAE are reduced by almost a factor of two. At this location, a significant increase in the precision of the simulations is also observed. This may be related to significant changes in the bathymetry."

âĂć Section 3.5, p. 21, lines 24-25: I don't see such an improvement in Woods Hole, however it seems Bar Harbor improves. I don't see that the RMSE and MAE are so reduced.

The reviewer is right, the sentence should have said Bar Harbor instead of Woods Hole. It has now been fixed. See response to comment immediately above.

âĂć Section 4 (Conclusions), p. 23: "overall, Delft3D shows an ability to reproduce the observations with reasonable precision and accuracy". This is not really the case, it should be a less positive sentence in this case.

The text has been changed to say:

"Overall, Delft3D shows an ability to reproduce the water surface observations with reasonable precision and accuracy at most of the locations considered. However, the performance in terms of the wave height is of a lesser accuracy with the precision significantly decreasing at the tail of the simulated event. As expected, the simulations are dependent on the wind fields driving the model. "

Technical corrections

âĂć Increase the size of the text and the quality of all the figures, especially figures 3 and 4

The figures provided to the publishers were of sufficient size. The original idea was that they will have a 1 page size to guarantee clarity and quality for all the details. They were downsized during the typesetting process to half a page. A request will be made to the typesetters to keep the size.

âĂć Better explanation of the figures: how is the sensitivity index calculated?

To avoid confusions it is now stated in the caption of the figures that the indices are computed according to Campolongo et al., 2007. It is now stated in the text (Section 2.4, second paragraph) that we refer to those specific sensitivity indices as the Campolongo indices.

It is our belief that a description of the specific calculations for the sensitivity indices are not necessary here. A succinct summary description of the methods is provided in Section 2.4 Additionally, it should be noted that the reader was (and currently is) referred for details of the method to the original papers by Morris 1991 and Campolongo et al., 2007.

âĂć Figures 8 and 10: indicate units of RMSE, MAE and Max diff. Units of the error performance measures now indicated in the caption of the figures.

âĂć Page 3, line 10: “limited” instead of “limiting” fixed

âĂć Page 7, line 10: “likely do” instead of “likely does” fixed

âĂć Page 13, line 6: delete “of” in “over of the current” fixed

âĂć Page 13, line 20: “triad model is” instead of “triad model as” fixed

âĂć Page 21, line 3-4: delete beginning of sentence “At the Woods Hole location..”. Start with “It appears that the . . .” fixed

âĂć Page 21, line 20: delete the parenthesis in “Campolongo et al., 2007” Parentheses removed.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/3/C3133/2016/nhessd-3-C3133-2016-supplement.pdf

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6491, 2015.