Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-112-RC1, 2018

Interactive comment on "Predicting Ocean Waves along the U.S. East Coast During Energetic Winter Storms: sensitivity to Whitecapping parameterizations" *by* Mohammad Nabi Allahdadi et al.

Anonymous Referee #1

Received and published: 24 November 2018

The authors present the results of a sensitivity study of applying two wave model settings concerning the whitecapping parameterisations. As the results of the different settings show (as expected) different results, the authors performed an analysis to explain the differences and determined the range of applicability. Although the authors show some knowledge about spectral wave modelling, they fail to make a sound manuscript and I cannot recommend it for publication. One of the reasons is that no proper research question is posed. Just comparing different model outcomes is a trivial exercise, but the lessons learned are now too vaguely investigated or described.

This becomes apparent as the words 'could be' occur at too many places (9 counted). In general, the manuscript is too descriptive and too less an in-depth analysis. For instance, in the discussion of growth curves, a possible problem is suggested, but no work has actually been done to make a further step. A similar remark can be made about the effect of atmospheric stability.

Another objection is that a large part of the analysis is to try to explain differences in source term behavior. There are some nice observations, but as the spectra themselves are already different it makes no sense to draw conclusions about source term differences. A sounder comparison is to start with equal spectra followed by investigating the source term response.

Concerning the choice of whitecapping source terms, various aspects are missing in their description. An essential part of the Komen type whitecapping is the use of a mean wave steepness. This is briefly mentioned when introducing the Westhuysen term, but hardly any systematic analysis is done. It is known for many years that this gives problems in bi-model seas with a swell and a wind sea component. Playing with the delta parameter, see for instance, Rogers et al. (2003, JPO, 33) and Pallares et al. (2014, CSR, 87) discuss this effect. Concerning the Westhuysen setting, the author do not seem to be aware of Babanin and Westhuysen (2008, JPO, 38). Moreover, recent developments in source terms, for ease referred to as the ST4 and ST6 versions in Wavewatch, are not mentioned at all.

The quality of any wave model hindcast is largely dependent on the quality of the wind fields. In this study no attempt has been made to validate the wind.

The notion that source terms should be recalibrated for different type of fetches or limitations is an interesting notion, but a more important conclusion is that the chosen formulations suffer a number of (already known) shortcomings, which can be remedied by introduction of more sound physics, viz. the parameterisations. In that sense, the present manuscript adds nothing new and recommends the wrong approach to improve spectral wave modelling in coastal areas.

The use of snapshots of spectra and then draw conclusions about model behavior is not sound science. This is statistically seen of no value.

The mean wave period is not defined. It is not clear whether this is Tm01, Tm02 or even Tm-10 (also referred to as TE). Same, for the definition of mean wave direction.

The authors suggest in the introduction (#26) that certain slanting fetch effects can not be generated due to short comings in the wind field. This notion is wrong, as it is a complex interplay of all source terms, see Ardhuin et al. (2007).

#41 Quantifying the source terms still is a challenging task, just look at the developments around more modern source terms like ST4 and ST6. Having said that, I feel the choice of the present authors is outdated.

#61 What are small coastal areas?

#64, #67 Reference missing in list of references, or is there a typo?

#78 which specific formulations are meant.

#87 The authors discuss seasonality as being important, but their analysis is based on only one month of data. Again, the authors miss an opportunity to study their problem in depth.

#95 The description of the role of the delta parameter is wrong. Yes, it has an effect on periods, but this parameter is not the related to another basic effect

#145 Looking up Allahdadi et al. (2018) gave only an abstract. This makes it impossible to judge its content and how it relates to the present manuscript.

#154 A wind rose would reveal in proper detail what is dominant.

#156 Local wind and distant swell are unrelated. Therefore, this reasoning is flawed, unless the others refer to different phenomena. Authors could also have used spectral

partitioning to select events of interest. Now, it is possible that their data are full of 'swell' noise.

#182 Also convergence criterion important, like number of iterations per time step

#201 The added value of the referenced papers is unclear. You do not need such references for basic statistics.

#209 'Could be' An example of speculation

#216 Snapshots are insufficient to draw conclusions

#232 The E(fp) is not a suitable parameter to draw conclusions, moreover as only one event is shown. It has only one degree of freedom. More robust methods are using bulk statistics of different period measures like Tm01 and Tm-10, which have different emphasis on weighting frequency bands

#238 'could be ' another example of speculation.

#284 quadruplet terms cannot be predicted by Komen or Westhuysen source terms.

#305 it is unclear how the actual fetch lengths are estimated in the open ocean.

#311 what are the defaults. As they may change, it is better to specify them for instance in an appendix.

#346 'Presumably', again speculation

#355 'may cause', speculation

#362 why are small variations in speed and direction relevant in this context

#368 I doubt whether revisiting the calibration process is the proper way. This can be a short-term fix, but rather difficult to obtain sound results. This notion is more an indication that the present physics in Westhuysen and Komen is incomplete. Sticking to outdated source terms does not seem a viable option

#380 Concerning land-sea effects on the development of wind speed, the authors can refer to papers from e.g. Dobson et al. (1989, Atm_Ocean, 27) and Taylor and Lee (1984).

#402 it is not how the 2D-spectra were reconstructed from the measurements in Figure 12

#420 The discussion on temperature effects is interesting, but nothing is done with this. Moreover, this is effect is known already.

#452 Do these so-called intense wind speeds occur in the present hindcast, and does the Yan parameterization really have an effect at the buoy locations

#455 I do not understand the reasoning about the computation of the quadruplets.

#476 What makes a coastline complicated? It all has to do with scales. The interpretation of the causes of the slanting fetch effect is incomplete. Directionally dependent fetch-lengths and the quadruplet interactions also play a role.

#479 See Ardhuin et al. (207) for an in-depth discussion

#480 Reasoning is incorrect. In case low-frequencies are underestimated, this may be due to the effect of the whitecapping term. Now the authors suggest it leads to an overestimation.

#488 What do the authors imply mean 'a spurious effect'

#490 Good idea to study all seasons. As in this manuscript only one month is used, the conclusions are not backed with sufficient data.

Table 2 Just providing data for one time instant is statistically insufficient and completely meaningless. Furtherm the mean wave period is not defined

Figure 2 A wind and wave rose would be more informative.

Figure 3 The snapshots are too similar to be of value. A possible link with the determi-

nation of a fetch length could add some value to this manuscript.

Figure 5 The causes of the discrepancies can also lie in incorrect winds. But nothing is said about the quality of the winds. Some details, add grid lines and close the rectangle.

Figure 6 Same as for Figure 5

Figure 7 The arrows with the mean direction are too small. Further, what is the definition of mean wave direction?

Figure 9 Nice pictures, but of little value as the underlying spectra are also different. Then the source terms will also be different and no firm conclusions can be drawn.

Figure 10 Why are the Snl4-terms oscillatory?

Figure 13 This figures confirms known concepts, but nothing is done in this study.

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