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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.

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Dear reviewer

Thank you very much for your thorough and constructive comments on our paper. Followings are responses to your comments and concerns: ———————————————————————

*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

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vaguely investigated or described:

We agree that the research question were not posed directly. However, they have been mentioned as gaps in science indirectly(pleases see #70-#75) as reads:

“Hence, more studies are needed to determine the appropriate ranges of application for each whitecapping method. Moreover, wave growth and dissipation can be significantly affected by variabilities in the wind speed and direction (gustiness), instabilities in the air-sea boundary layer because of air-sea temperature difference, and slanting fetches over the coastal areas (Ardhuin et al., 2007; Donelan et al., 1985).” Obviously our research questions are: what are the appropriate ranges of application for each of Komen and Westhuysen whitecapping formulations and what are the effects of different wind conditions and other factors like coastal geometry and instabilities in the air-sea boundary layer. Throughout the manuscript, these questions have been answered. We will mmention this question directly in the introduction part. Although our paper outline generally follows some previously published work (ex. Vledder et al. (2016), but with much more details about the source terms and justification of differences), will modify the manuscript to make the discussion points more clear.

This becomes apparent as the words ‘could be’ occur at too many places (9 counted).

Frequent use of ‘could be’ will be revisited and fixed. In some cases, we don’t even need to do the speculations since we have technical references to approve the point we made (ex. 205-210, 235-240, 240-245), and therefore by adding those references we will make the point absolute and clear. Some other cases were not actually speculation and only the sentence need to be modified since we had enough evidences to make the argument absolute(ex. 320-325, 350, 350-355, 365-370). In one case (355-360) it is not even speculation, but figure of speech. We will fix the manuscript to avoid these speculation-like expressions as much as possible.

**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.

For the case of the growth curves, we argued that there are two main reasons for deficiencies: 1. Cases of fully-developed events that deviated the results from the measured growth curves and 2) the frequent cases of duration-limited events that are not even included in the growth curves. The quantification in this part was presented based on the examined times t1 and t2 and comparing simulation results with the numerical values resulted from the growth curves of Calkoen (1992) (for fetch-limited growth) and Pierson-Moskowitz (1964) (for the fully-developed sea state). Quantification for the duration-limited case was not possible since by our knowledge and unlike the curves for other two sea states, measured growth curves for this sea state are not publicly available. However, the analysis related to figure 11 show the relative frequency of events for each case regarding the two examined whitecappinf models. We can elaborate more on interpreting this figure ex. the exact numerical values of the event frequency for each case and at each buoy. Regarding the presented results for studying the atmospheric instability, we agree that more analysis may be required. We will complete the discussion in this part by adding some statistical results for the correlation between the air-sea temperature difference and deviations of the simulation results from the measurements. 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.

The base for the analysis on the source terms is different wave height and wave periods that were resulted for same simulation conditions and two different whitecapping formulations. Hence, the spectra and source terms regarding specific times that exhibit significant differences between simulation results and observations were presented to figure out the reasons that cause the differences. This is a standard methodology that was followed by many research studies (ex. van der Westhuysen et al., 2007; Mulligan et al., 2008, and Vledder et al.2016). The rational is simple: at a specific time for similar conditions, wave height and periods should be the same, but since the energy spectra and in fact the source terms are quantified in different ways, deviations in the integrated parameters were resulted. Our main argument in this paper was identifying the reasons that contributes to these differences in the context of assumptions and limitations associated with each whitecapping model. ***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.

We appreciate that the mean wave parameters including the steepness are the main contributor to the shortcomings associated with Komen approach for the cases of seas-swell combination. However, in the present study only wind-generated wave with single-peaked spectra (like Figure 8) are simulated due to the selected study period in January 2009. The main point of he present study is although Westhuysen whitcapping approach was introduced to overcome the inaccuracies associated with Komen method during the seas-swell combination, its performance for the purely wind-generated cases with no swell is lower than Koem which is consistent with Vledder et al. 2016. Furthermore, the main goal of the present study was examining the performance of these whitecapping formula within a real simulation framework, not a detailed examining of of the theoretical basis and suggesting a new form of the formulation. We will mention it in the introduction part to make the objectives more clear and avoid further complication for readers.

+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.

We are aware of Babanin and Westhuysen(2008) paper when reviewing the theoretical bases on the saturated-based model, although eventually we did not mention it in the manuscript. One reason that we did not mention it was that the focus of our research was not identifying the pitfalls in the theoretical background of the method, but examining the validity of both whitecapping approaches in a real simulation over an extensive region and prescribing the practical remedies for modelers. In the theoretical sense, Komen method has also several pitfalls in terms of inconsistency with observations, but again it was not our focus. The background behind our research is an extensive long-term wave modeling along the U.S East Coast for characterizing wave energy resources. We used SWAN for this modeling since due to complex coastal geometry we could benefit from the flexible mesh option made available by SWAN. The only whitecapping formulations that are available by SWAN+ADCIRC(we had to use this coupled version to implement the domain decomposing used in the high-performance coputation) are Komen-type and Westhuysen approaches and none of the physics packages incorporated in WWIII (ST2, ST4, and ST6) are available by this coupled version of SWAN. Although the observation-based physics package of ST6 has been recently added to the SWAN version 41.20, it has not been incorporated in the coupled SWAN+ADCIRC by the ADRIRC group. Hence, we need to deal with the available whitecapping approaches including Komen and Westhuysen if we want to use SWAN with the HPC ability which is vital for long-term modeling over an extensive modeling area with over 4,300, 000 computational nodes(the same modeling area of this manuscript, Figure1). To optimally use SWAN, sensitivity analysis on available whitecapping methods was absolutely necessary and the present manuscript shows a part of those extensive tests that we did to finalize the whitecapping parameters. By our experience, the results from this research are highly relevant for wave modeler all over the world simply due to the fact that SWAN is still being widely applied for wave modeling for different area. It could have saved many times for our simulation if such a research study was available during our modeling efforts. It should be noted that although more advanced model are consistent with observations and were not just developed based on solving the radiation transfer equation as an unknown, yet their application in the real world simulation could be challenging and with lower accuracy compared to older models. One example is the study of Yan et al(2017) for the U.S West coast(the paper is attached to the response). Their one-year simulation showed that using ST6 physics package which is the newest in WWIII, does not actually improve wave parameters and resulted in lower model performance statistics compared to ST4 which is an older physics package. Regarding the WWIII physics packages (ST2, ST4, and ST6), we agree that we should have mentioned them in the manuscript. The related short summary about these packages will be added to the modified manuscript. 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 authors have thoroughly verified the wind field versus wind measurements at the location of NDBC buoys (Allahdadi et al., 2018). Some verification results will be included in the modified manuscript.

++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

As mentioned above, the diagnosis about improving the two whitecapping approaches incorporated in SWAN; especially Wethuysen were regarding the fact that the more advanced whitecapping formulations like those are being used in WWIII, have not been incorporated in tne HPC-compatible version of SWAN yet, while SWAN is still being widely used all over the world. It means that still we need some reliable documentation of the shortcomings for each method based on the real-scale model simulations and the practical ways to overcome them. We will add notions in conclusion about the necessity for incorporating more advanced physics packages in SWAN to be more consistent with observations of whitecapping rates in the lab and field. Please also see the response to + in the above.

+++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 timstep corresponding the presented snapshots has not been selected arbitrarily, but selected based on a specific behavior of waves over the modeling area regarding the wind field. Please see 170-175 for details of the storm corresponding to time t1. The storm at this time resulted a fully-developed sea state at buoy 44011 that was the base for further analysis related to wave growth curves as presented in section 6.2. Based on our hypothesis on effect of sea state on simulation results, we examined several spectra at times with different sea states and observed similar behaviors for cases with fully-developed sea states at different buoys. The presented snapshot is a representative for these investigated case. We will make it more clear in the modified manuscript. Using just one snapshot as a representative of a specific behavior has also been used by other researches, ex. van der Westhuysen et al(2007), Mulligan et al.(200), and Vldder et al. (2016)

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 mean wave period used in this manuscript is the model-equivalent of zero-crossing wave period(T02). We will present the spectral definition on both mean wave period and mean wave direction used in this paper in the modified manuscript.

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).

We agree that the effect of the "slanting fetch" is an interplay between the source terms since the source terms' quantifications are related to each other (especially whitecapping and wind input). We inferred expression of #26 from page 926 of the Ardhuin et al

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(2007): “the wind input term of Janssen (1991) is found to be too narrow in direction, at least at the peak frequency, while the forms proposed by Snyder et al. (1981) and Tolman and Chalikov (1996) have a more appropriate directional shape in that range”

, but we will modify this in the manuscript based on the reviewer’s comment that makes more sense based on the whole idea of 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.

Please see the above responses on + and ***

#61 What are small coastal areas? We meant a single coastal area with limited fetch length, like the case studies by va der Westhhuysen et al(2007) at Virginia-North Carolina coast with maximum studied fetch lengths in the order of 100 km and smaller. It will be mentioned in the manuscript

#64, #67 Reference missing in list of references, or is there a typo? Velder et al. (2016) Is a typo, the right one is Vledder et al(2016) that will be corrected in the manuscript.

#78 which specific formulations are meant. This is the whitecapping source term S_dsˆJHHK that was tuned for the WAM Cycle-4 model. This will be mentioned in the modified manuscript #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.

As mentioned in this part of the paper, January is the representative of winter season. It was shown in Allahdadi et al(2018) that wind patterns and therefore wave and energy patterns from November to March are similar. During this time, the dominant wind field is generally winter storms that makes an offshoreward wind pattern over the model. However, we will try to be more specific just about January and less focused on the seasonality in the modified manuscript.

#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

We implied this sentence about delta based in the SWAN 41.1 technical manual pages 20 and 21. We should have mentioned that it may also change the wave height and we need to change it in agreement with C_ds . We will make it more clear in the modified manuscript.

#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.

The presentation is also available online through the Rsesrchgate. This file will be attached to the response to comment.

#154 A wind rose would reveal in proper detail what is dominant. We will include wind roses of those stations for January2009 in the modified manuscript.

#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.

Our rational behind the argument of using this January 2009 for an almost swell-free analysis of whitecapping was based on our former long-term simulation for the East Coast and examining observed frequency-spectra. In that sense, during the simulation period, the dominant spectra at all four stations were single-peaked(please see for example Figure 8 of the original submission) that along with the dominant offshoreward wind direction shows just a wind-generated peak and no swell is present. We will include more descriptions in the modified manuscript to make it more clear. Also we can include more evidences of the single peaked spectra at different times. #182 Also convergence criterion important, like number of iterations per time step Will be mentioned in the modified manuscript

#201 The added value of the referenced papers is unclear. You do not need such references for basic statistics. The reference will be removed. #209 'Could be' An example of speculation We remove 'could be' from here and just use 'is' since we proved later that it was due to the slanting fetch effect.

#216 Snapshots are insufficient to draw conclusions

These snapshots in figure 7 are corresponding to a specific behavior of the wave field(fully-developed) that is representative of a group of incidents and are consistent with the wind field presented in Figure 3 of the original submission.

#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

We used E(fp), fp, and bulk ware parameters(Figures 4-7) together to conclude about the model behavior. E(fp) was only one of the parameters that we considered for making conclusion about the model behavior. It has also been used by van der Westhuysen et al (2007) to compare two whitecapping models. About the one timestep it was clarified before (that it is a representative of a group with similar behavior.

#238 'could be ' another example of speculation. The expression will be removed by using some references to make it definite.

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

This will be modified. We meant that the simulation using Komen and Westhuysen whitecapping resulted closer values of quadruplets when compared to differences between other two simulated source terms by two models.

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

Calculation of actual fetch lengths that resulted the fetch limited and fully-developed wave growth curves are described in Kahma and Calkoen (1992) for fetch-limited growth and Pierson-Moskowitz (1964) for fully-developed cases and is not in the scope of the present study. In our analysis we calculate the fetch length from land to the location of each buoy in the direction of the dominant wind that lasted for hours before times t1 and t2.

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

Default values for each whitecapping approach will be added to the modified paper.

#346 'Presumably', again speculation

Since the effect of coastline on wave spectrum has later been discussed in section 6.3, we can remove presumably and make the argument definite.

#355 'may cause', speculation

This speculation was made to show that future research studies need to treat fully-developed and fetch-limited conditions independently.

#362 why are small variations in speed and direction relevant in this context If time variations of wind speed and direction are too much, the conditions for reaching a fetch-limited or fully-developed sea state are violated(Coastal Engineering Manual, 2000). More details along with the reference will be mentioned in the modified manuscript. #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 Please see the answer to + and ++ above. #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).

The references and related descriptions will be added and discussed in the modified manuscript.

#402 it is not how the 2D-spectra were reconstructed from the measurements in Figure 12 Measured and modeled spectra are different since model partly or completely fails to simulate the high frequency part of the spectrum generated as a result of the slanting fetch effect.

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

For the first part of the comment please see the answer to ** Although this effect is already known, it has not been examined for the study area and here it was used as a complementary part along with other discussion points

#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

Yes, we have large wind speeds over the modeling area. Please see #173 and Figure 2. The effect of Yan parametrization at buoy location is obvious looking at Figure 9d. This figure shows wind input term for the westhuysen formulation that uses Yan parametrization. Comparing it with Figure 9a for parametrization using Komen shows how the effect of high speed wind events are considered in Yan(1987) formulation. #455 I do not understand the reasoning about the computation of the quadruplets.

It is a standard way of examining source terms in simulations as used by van der Westhuysen et al(2007), van Veldder et al(2016), and several other studies. Here, it demonstrates that, although both simulation cases use the same computational approaches for calculating the quadruplet, still the amount of energy that is redistributed due to four-wave interaction is different between to methods due to different whitecappind approaches used for each simulation.

#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.

By “complicated coastal Geometry”, we meant “variations in the coastline direction”. We will modify it in the manuscript. “Directionally dependent fetch-lengths” that the reviewer mentioned is actually another manifestation of “deviation of the wind direction from the shore-normal direction” that we already mentioned in the text. We will directly mention this in the manuscript along with some discussions about the effect t of the quadruplet.

#479 See Ardhuin et al. (207) for an in-depth discussion More details will be added to the manuscript based on Ardhuin et al(2007).

#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.

We will modify the :underestimation of wave parameter” to “deviating simulated wave parameters from observations”.

#488 What do the authors imply mean ‘a spurious effect’ We meant the lower or higher of whitecapping dissipation than the real value that is produced in the presence of swell waves (please see van der Westhuysen et al(2007))

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

We will limit our conclusion to only January 2009.

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

Please see the answer to +++ Wave period is T02 that will be mentioned in the modified manuscript.

Figure 2 A wind and wave rose would be more informative. Wind roses will be included in the modified manuscript

Figure 3 The snapshots are too similar to be of value. A possible link with the determination of a fetch length could add some value to this manuscript. Similarity of the wind field for the times before t1 and t2 as shown in Figure 3 is the main point of these snapshots to demonstrate that wind field was almost persistent so that can produce fetch-limited or fully-developed sea states. We will clarify this and the way we calculated the fetch length based on these wind fields. 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.

Wind field evaluation will be added to the modified manuscript. Figures will be modified.

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? Figures will be re-plotted for larger vector sizes. The definition of the mean wave period will be added.

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.

Presenting the spatial variations of main source terms simulated under two different whitecapping approaches is a significant part for demonstrating that different source term representation by these whitecapping approaches and their wind input counterparts are different. They show that although the wind input counterpart of the Westhuysen approach (Yan(1987)) resulted in larger input to the sea surface than that of Komen, larger whitecappin dissipation balances the excess input. But in the case of our study period this dissipation from Westhuysen approach is more than reality so that it may cause underestimation of the spectral energy in the wave spectrum. This implication will be added to the manuscript in section 6.1.

Figure 10 Why are the Snl4-terms oscillatory?

The oscillatory variations of the Snl4-term with frequency, especially the one for Westhuysen simulation could be due to oscillation of the Whitecapping term between frequencies of 0.1 to 0.35 Hz (please see Figure 10b). This pattern has also been simulated by Mulligan et al(2008).

Figure 13 This figures confirms known concepts, but nothing is done in this study. Please see the answer to #420

Bets Regards Allahdadi et al.

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2018-112/os-2018-112-AC3-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2018-112, 2018.