

## ***Interactive comment on “Numerical Investigation of Typhoon Waves Generated by Three Typhoons in the China Sea” by Qing Shi et al.***

### **Anonymous Referee #2**

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The manuscript describes a hindcast of typhoon generated waves in the China Sea using a third-generation wave prediction model. Attention is given to the choice of the wind data source and comparisons are made of hindcast data against nearshore buoy data and satellite data. My general comment is that the manuscript is too descriptive without having a proper research question. Expected differences between different model setups are noted and trivial conclusions are drawn. Reading between the lines it appears that the simultaneous occurrence of 3 typhoons is the challenging part of this work. This could be the starting point of a more in-depth analysis addressing the challenges of performing a hindcast for such a situation and giving attention to the blending of wind fields, choosing the proper wave physics and extending the analysis also to other wave parameters like mean/peak wave direction, and wave period measures.

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The chosen frequency range is 0.04 Hz – 1 Hz. I wonder whether the low frequency limit is properly chosen as this relates to waves with a period of 25 s. It could be possible that under typhoon conditions longer wave periods occur. This issue should be discussed in combination with wave period information (e.g. Tm-10, Tm01) As 3 typhoons interact with each other, the resulting wave field is expected contain various swell fields and an analysis of the 1D- and 2D frequency spectra could provide relevant insights.

More detailed comments are as follows:

The title contains twice the word typhoon and it misses the word simultaneously. Please reconsider the title.

The numbers 1509, 1510 and 1511 in line 7 should be introduced. I assume it is some local numbering of typhoons?

Line 29: it is not clear what kind of evolution laws are meant in complex situations. I do not think parametric growth laws apply here.

The description of the wave model should be revised. Eq. (3) is not needed as no currents are part of this study. Part of the model settings are default, but of more interest are the physical settings relevant for typhoon modelling like the choice of wind forcing, wind drag relation and whitecapping dissipation. Further, as the model is (presumably) run in non-stationary mode, details should also be given about the time stepping, convergence settings (applicable to SWAN), and the spatial resolution. Evidently, no tuning of the wave model is done. That could be a good starting point, but in view of the noted discrepancies, tuning should be mentioned, if possible, as a way to improve modelling results. Evidently some nesting is applied, but no details are given of the extent of such nest areas. This should be made more clear in Figure 2.

Figure 3 should be supplemented with a table providing the positions and depths of the various wave buoys.

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In line 168 and elsewhere it is not explicitly stated that the wave height is the significant wave height. Moreover, providing only information on this wave parameter is too limited. Especially directional and period information supplement the analysis.

Line 175 mentions diffraction and fragmentation. I do not think diffraction will play a role on the scale of the China Sea, and it is unclear what is meant with fragmentation.

Line 186 contains a negative bias where 2 lines earlier the phrase absolute values was used. This is confusing.

As a general comment here and elsewhere I notice that BIAS and RMSE are given in 3 decimals. I do not believe this to be realistic. The number of decimals should reflect the accuracy of numbers.

The legends of many figures are incomplete. Please make these more descriptive.

The results in Table 2 show differences in performance. This can be expected, but the consequences of these differences, related to the still unknown research question, are unclear. The number of hindcasts is too limited to draw sound conclusions about the best wind data source.

Figure 5 shows many discrepancies between measured and modelled winds. Do the authors consider this a sufficiently good basis for performing the hindcast?

The superposition of the various wind fields in the Holland model is still unclear. Of particular interest is whether the combined wind field (in combination of the superimposed pressure fields) is physically possible. Hinting at Wang (2017) is not helpful as this is in Chinese language and not accessible for the interested reader. This manuscript could be a place to make an accessible presentation of this topic. See also my previous comment on the challenges.

Line 254 mentions some numbers. It is unclear in what way these numbers were obtained.

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What is the basis for the statement in line 264 that the blended wind is more accurate?

The lines 268 and 275 (as well as at other places) use rather qualitative terms 'are similar' or 'more reasonable'. The criteria for making such statements are missing. And using more quantitative measures would improve the content.

Figure 8 shows the paths of the 3 typhoon that more or less seem to occur simultaneously. It would be good to plot a few parameters of each typhoon in one plot with time along the horizontal axis. Then more attention can be given to the mutual timing of each typhoon.

Same comment for Figure 9.

What is meant with 'mutual rotation' in line 332?

The abundance of subplots in Figure 12 is not needed. These figures show differences, but what is exactly the message. Why not pick a few time instances to answer this and discuss those in detail. In addition, plotting vectors with the mean wave direction may reveal some of the complexity of such a hindcast.

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