

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

Anonymous Referee #1

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The paper "Numerical Investigation of Typhoon Waves Generated by Three Typhoons in the China Sea" touches an interesting topic such as wave generation under the simultaneous action of 3 typhoons. However, in its present form it is too descriptive and with limited critical assessment. Moreover, the presented analysis is only statistical with very limited mention to the underlying physics. Because of that I would recommend performing a more in depth physical analysis, combining physics with statistics to advance the state of the art on wave generation from winds with different directions acting on pre-existing swell also with different directions. That complexity, associated to the presence of 3 typhoons, would really advance the state of the art and provide for an interesting paper.

More specifically, I would suggest:

C1

- Defining some of the concepts presented (for instance, a "complex mixed wave" on line 31) or the "Holland model" on line 50 (in this case defined much later in the paper)
- Emphasizing the applicability of such a complex wind and wave pattern from the China Sea to other seas in the planet.
- I would recommend reducing greatly the description of the SWAN model. However presenting in more depth the sink and source terms and the employed numerical discretization and the diffusivity that results would provide an interesting piece of research work. Particularly when assessing how it performs under various cyclones.
- The same regarding the selection for the wind energy input term (the same would apply to the sink-term) where the A and B coefficients have been selected from the state of the art without any mention to the presence of the cyclones.
- The performed comparison emphasizes correlation coefficients, bias, root mean square error, etc. A similar emphasis should be given to the underlying physics. Also, duly acknowledging the state of the art, where the fact that errors get larger when getting closer to the coast is already well known.
- On line 206 it is mentioned that the trend of the wave height is similar to that of the wind speed. Here no mention is made of the pre-existing swell from more distant cyclones and that would be an essential element for the analysis.
- In the comparison only wave height and wind speed are mentioned. Wave direction should here play a critical role. The same regarding wave period or wave age.
- When presenting the superposition formula (for equations 19 and following) the physics behind the proposed formulation and a comparison with other alternatives should be included.
- The same regarding the improvement of the blended wind field when compared to others (e.g. CCMP). The role of fit parameters should also be mentioned in here.

C2

- When presenting the accuracy of the simulated wave field due attention should be paid to the underlying physics since otherwise the statement in line 311 and following that the simulated heights from the blended wind fields are much more consistent with Jason-2 than those from CCMP lacks meaning.

- The same regarding the conclusion (line 363) that the wave results driven by CCMP are more accurate for small waves and the opposite for CFSv2.

In summary the paper should emphasize more the physics behind the analysis and be more critical with respect to the obtained results. With that I think it could provide an interesting contribution to the state of the art.

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