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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Numerical investigation on spectral geometries and their relation to non-Gaussianity in sea states with occurrence of rogue waves: wind-sea dominated events" *by* Xingjie Jiang et al.

Anonymous Referee #1

Received and published: 10 November 2020

(Since I mistakenly submitted this comment before fixing formatting error, I submit this comment again)

I cannot support this manuscript for publication in the present form. Here is comments and major concern of this manuscript.

1. General comments

1.1. Scientific significance

I think that this manuscript should clarify its novelty in comparison with previous studies.

This manuscript investigated the relationship between the non-Gaussianity of the sea states and the three spectral parameters: SP, BW, and DS. On the other hand, some previous studies showed theories to predict kurtosis as the indicator of the non-Gaussianity from spectral parameters, as mentioned in line 51-53 of this manuscript.

In line 53-55, the authors say that "it remains difficult to assess directly the severity of the deviation from Gaussianity for a given sea state, or to infer the nature of the dominant nonlinearities." I didn't figure out why the authors argued "it remains difficult". I believe that the existing theories mentioned in line 51-53 can be applied to predict the non-Gaussianity for operational wave forecasts. Therefore, I cannot understand why HOSM was required to predict the kurtosis for the rogue wave events mentioned in the manuscript. Since the line 53-55 corresponds to the key issue of this manuscript, I think these lines should be written in more detail.

Additional comments on the novelty are the following:

- Even if HOSM is necessary, Xiao et al. (2013) conducted similar computation of HOSM to relate kurtosis to spectral parameters. Is not the result of Xiao et al. (2013) insufficient?

- In order to clarify the novelty and deepen the discussion, I recommend the authors to compare their results in Figure 9 with the existing theories.

- The conclusion of this manuscript is fairly similar to that of Fedele et al. (2016).

1.2. Scientific quality

There are a lack of evidence and a leap in logic in this manuscript. Please see 2. Specific comments.

1.3. Presentation quality

This manuscript is well organized. The figures and tables are easy to see. I found no problem regarding usage of English language.

2. Specific comments

Line 58-60: With regard to “the magnitude of the kurtosis or the narrowness of the DS required for triggering the MI remains unclear. Moreover, the question of whether there are any other thresholds for the SP and BW geometries remains to be resolved”, Ribal et al. (2013) derived criterion of the modulational instability for JONSWAP spectra.

Line 71: The authors should explain the meaning of “involve additional complicated factors might influence estimation of the non-Gaussianity . . .” more clearly. What are “additional complicated factors”?

Line 359-360: With regard to “For dominant wind-sea wave fields, it is known that the spectral geometry of DS is generally wide, although the range of DS might become narrower as the waves become more developed”, please cite some references or show some evidences.

Line 338-341: The authors argued that predictions of the kurtosis based on the spectral parameters (the blue line in Figure 9) are comparable to kurtoses of additional HOSM simulation based on the modeled spectra of the WAVEWATCHIII (the red line in Figure 9). However, the latter is several times larger than the former in all cases of Alwyn, actually. Some explanation on this discrepancy are needed.

Line 125, Table 2 and Table 3: Please add information about water depth of Draupner, Andrea, and Alwyn site because the water depth strongly affects the modulational instability (Janssen & Onorato, 2007). The infinite water depth was adopted for HOSM simulation (line 125) in this study, but was this assumption valid for these sites?

3. Technical comments

Title: The title is a little bit long.

In “Abstract” and “Conclusion and Discussion”: Two different fonts are mixed in these sections.

Line 234, 263, and 290: Line spacings of headings of sections 3.1, 3.2, and 3.3 are different from that of previous sections. The former is narrower than the latter.

References

Fedele, F., Brennan, J., Ponce de León, S., Dudley, J. M., Dias, F., De León, S. P., Dudley, J. M., & Dias, F. (2016). Real world ocean rogue waves explained without the modulational instability. Scientific Reports, 6(27715), 1–11. https://doi.org/10.1038/srep27715

Janssen, P. A. E. M., & Onorato, M. (2007). The Intermediate Water Depth Limit of the Zakharov Equation and Consequences for Wave Prediction. Journal of Physical Oceanography, 37(10), 2389–2400. https://doi.org/10.1175/JPO3128.1

Ribal, A., Babanin, A. V, Young, I. R., Toffoli, A., & Stiassnie, M. (2013). Recurrent solutions of the Alber equation initialized by Joint North Sea Wave Project spectra. Journal of Fluid Mechanics, 719, 314–344. https://doi.org/10.1017/jfm.2013.7

Xiao, W., Liu, Y., Wu, G., & Yue, D. K. P. (2013). Rogue wave occurrence and dynamics by direct simulations of nonlinear wave-field evolution. Journal of Fluid Mechanics, 720, 357–392. https://doi.org/10.1017/jfm.2013.37

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