

Reply to the author's comments:

I still cannot support this manuscript for the publication. The author's comments are laid out in *italicized* font. My response is given in normal font.

Reviewer's Response:

Overall, there are still expressions (**bold** fonts) that I disagree with in the author's comment and the manuscript.

Comment 1: *This manuscript aims at providing a systematic approach that allows a **convenient** and quantitative comparison of non-Gaussianity of real-world wave fields through the corresponding wave spectra. ... Apparently, the newly proposed approach is **operational** and can be used in more studies related to rogue wave sea states.*

For whom is your method convenient? Is your method reusable for readers? The computational burden of HOSM is not small, and it is difficult for some readers to perform HOSM simulation similar to yours. If you argue that your method is convenient and operational, the way readers can reuse your results should be provided. For example, your HOSM results corresponding to Fig.3 could be released as a public database or some regression formula relating the kurtosis and skewness to spectral geometries. As a reference, Annenkov & Shrira (2014) showed formula predicting kurtosis from JONSWAP's peakedness and steepness.

Comment 2: *The operational non-Gaussianity indicators, especially dynamic kurtosis, were first obtained from theoretical models derived under the narrowband assumption in an environment with near unidirectional wave propagation. The original expression of those indicators **cannot be applied** to the real sea conditions with broad bandwidth and directional spreading. ... However, calibrations were conducted based on the final forecast results, ... the number of waves is a quantity that cannot be accurately estimated in real wave fields. (And that is the "additional complicated factors")*

I did not see a significant advantage of your method compared to the previous theories. I thought that your statement could be summarized as "better model, better results", but your method using HOSM is still not convincing if comparison to the observation is not provided. Simple methods (like models based on the narrow-band assumption) calibrated well often outperforms complex methods (like HOSM) without calibration. Since ECMWF calibrated their theory with the observation, if anything, their method is more convincing. I admit that the number of waves is a quantity that

cannot be accurately estimated in real wave fields, but it is practically sufficient if the final forecast results can predict the maximum wave/crest height well.

The “additional complicated factors” are associated with statistical calibration. It is possible for me to interpret that your statement denies statistical calibration. However, since the nature of wave in the ocean is very complex and there is no perfect model that treats all physical phenomena, I believe that the calibration is inevitable to treat factors neglected in models statistically. For example, physical factors such as ocean currents, wave breaking neglected in your HOSM might affect the kurtosis. Even more, although the red lines and the blue lines in Fig. 9 are comparable, there are some errors. The error of your method should be statistically calibrated for operational prediction.

The error itself is of interests of readers who have willing to apply your results. Some kinds of error index like RMSE, SI, correlation coefficient and so on should be shown.

In addition, I think the sentence “*The original expression of those indicators **cannot be applied***” in the author’s comment is too strong. I agree with the revised sentence “*However, the usage of these criterions in real cases was not specified in their research. There is still a lack of a way to introduce arbitrary 2D wave spectra into the criterions (line 450-451 in the revised manuscript)*”, but the previous theories (e.g. Annenkov & Shrira, 2014; Ribal et al., 2013) can be applied if JONSWAP spectra is fitted to spectra output by wave models.

Comment 3-7: I understand your comments. I revised my opinion, and now I think the title is not too long.

In conclusion, if you focus on practical (operational) advantage of your method in comparison with the conventional methods such as that of ECMWF, comparison between your method and the observation should be provided. If the comparison is difficult to do, this study should focus on how to reuse your results for interests of readers as mentioned above.

If you focus on theoretical aspects of your method, and if you would show how to reuse your results, it would be convenient for theorists. Theorists can compare your results to previous theories (e.g. Annenkov & Shrira, 2014; Ribal et al., 2013). Since HOSM  $M=2$  includes some part of four-wave interactions and HOSM  $M=3$  exhibits Class II instability associated with five-wave interactions (Fujimoto et al., 2019; Fujimoto & Waseda, 2016), HOSM  $M=3$  might not necessarily show results same as the previous theories. The relationship between HOSM and the Zakharov equation is not trivial, and difference between kurtosis calculated by HOSM and the Zakharov equation might be of interest for theorists.