Reply to referee#1's comments:

Again, we would like thank you for the comments that we have used to improve our manuscript. The responses to comment 1 and 2 are given below:

Comment1: About the '***convenient***' and '***operational***' of our approach to estimate the non-Gaussianity of given real sea states.
Responses: We think you are right. The approach presented in this manuscript would be neither convenient nor operational if the pre-calculated non-Gaussianity references cannot be accessed. We decided to open the references dataset, which could also be found in the supplement. This manuscript only gives the first attempt to solve the problem, readers who are interested in this subject can reuse the approach with the accessible references, or even improve it.

Comment2: About the statement of '***cannot be applied***' for the operational non-Gaussian indicators.
Response: Actually, our statement of 'cannot be applied' is for the original expressions of the indicators, which were derived based on assumptions of narrowband and unidirectionality, rather than the calibrated ones. And in the revised manuscript, the statement has been revised as: *'However, those operational non-Gaussian indicators adopted in rogue wave forecast systems* ***are not suitable*** *to describe the non-Gaussianity and its relation to the spectral geometries in real sea states….'* (see Line 60-61 in the revised manuscript). We admit that those operational indicators are practically sufficient to predict the maximum wave/crest height. However, since the calibrations were conducted to improve the final forecast results, which also depends on the wave height probability density distribution and the number of waves, the non-Gaussianity and its relation to the spectral geometries expressed by those indicators might be affected by the calibrations.

We should also admit that our method can not be validated by observations of skewness and kurtosis of a given wave filed. In this manuscript, the additional HOSM simulations might be the best way to prove the feasibility of the method proposed. And we think the factors neglected in our simulations should be treated, and calibrations are inevitable if we want to improve our method. Again, the manuscript is a first step, more work could be done in the future.

In addition, fitting the JONSWAP spectrum and the directional distribution might be another way to relate spectrum models to arbitrary 2D spectra, we will consider that in future studies.

Finally, we sincerely thank your comments and suggestions for the improvement of this manuscript.

Yours,
Dr. Jiang Xingjie