Nonlin. Processes Geophys. Discuss.,
doi:10.5194/npg-2016-31-RC2, 2016
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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Localized Coherence of Freak Waves" *by* A. L. Latifah and E. van Groesen

Anonymous Referee #2

Received and published: 17 June 2016

The paper presents an analysis of records of water surface elevation over extended periods of time in order to determine the likelihood of appearance of extremely steep (rogue) waves at certain later instant. To this end, both Fourier and wavelet analysis are used extensively, and quantitative criteria are suggested to select segments of the records that contain locally energetic and coherent waves with a significant potential for evolving into a rogue wave phenomenon. The criteria are based on the complex Morlet wavelet spectra that take into account wave characteristic in the neighborhood of each instant. For reader's convenience, the paper contains a separate section with the basics of the wavelet transform. The approach offered by the authors to study rogue waves is interesting and novel so that the paper can be accepted for publication. I have, however, some comments that should be addressed prior to final acceptance. The general idea of the adopted approach is based on dispersive wave focusing, where relatively slow shorter waves precede faster longer waves, and thus may arrive at some later instant and location instantaneously with the same or similar phase. The validity

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of this approach is confirmed by analysis of several signals, part of them recorded in wave tanks experiments, while the others were generated by numerical simulations. The common feature of all those examples is that only unidirectional waves were considered. Prediction of dispersive focusing in the presence of directional spreading that it is always present in real ocean waves, may be much more complicated. I believe the manuscript can be improved notably if an example that contains directional spreading is considered. At the very least, the possible effects of angular spreading on the performance of the suggested algorithm should be discussed. Throughout the manuscript, there is no clear distinction between dimensional and dimensionless values. For example, wavelet theory is apparently presented in the dimensionless form, whereas the experimental results are probably dimensional (judging from the reference to time in seconds in the text); the temporal axis in most graphs is titled as 't' or 'time', no units are given. Similarly, it remains unclear what exactly is meant by 'Time signal' (is it elevation in meters?) or Local energy'. In some figures, there are no axis titles at all (e.g. Fig. 9). In all examples, the wave field characteristics such as peak frequency, spectral shape and significant wave height should be given. In Section 4.3, experiments in MARIN are studied. For some reason, scaling (1:50) is mentioned. The scaling is irrelevant to the goals of the present study, however, the reader becomes confused whether the parameters (at least those mentioned in the manuscript) represent the actual measured values or they are scaled. I fully agree with the 1st reviewer, some clarifications regarding the AB model should be provided. The name of Pelinovsky is misspelled on p. 15, line32.

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