Dear Editor,

Please find below our item-by-item response to the referee's comments on the manuscript by A. Sergeeva, A. Slunyaev, E. Pelinovsky, T. Talipova, and D.-J. Doong "Numerical modeling of rogue waves in coastal waters"

We would like to thank reviewers for useful comments, and submit the modified version of the manuscript. Significant modifications are marked with red colour.

Sincerely yours,
The authors

## 1. The First Referee's Report

**1.1. The authors re-use the term "abnormality index" to select rogue wave events from the existing wave system. Although used in the literature addressing rogue waves for almost a quarter of century (probably starting from Dean, R. 1990. Freak waves: a possible explanation, in: Water Wave Kinematics, Tørum, A. and Gudmestad, O. T. (Eds), Kluwer, 609–612), this notion is, to my understanding, infelicitous (as it includes also completely "normal" but infrequent waves) and should be replaced by a more appropriate one.**

It is convenient to have a given notation for the parameter $H / H_s$, which has clear meaning. In this and several precedent publications (including dedicated monograph by C. Kharif et al "Rogue waves in the Ocean", Springer-Verlag, 2009) we used abbreviation *AI* (abnormality index) for denoting the ratio $H / H_s$. As the referee has justly observed, this notation was used by other authors earlier as well. We would refrain from introducing a new term, therefore would like to remain this notation unchanged. Taking into account the remark, we added the following explanation:
*"The quantity AI will be referred to as abnormality index, which characterizes the departure of wave height from the significant value".*
We hope that the referee and the editor will find it acceptable.

**1.2. On page 5782, line 15 it is mentioned that 512 s long sections of wave records are used C1948 for further analysis. Their duration (about 8.5 minutes) is a small fraction of the one usually recommended for adequate estimates of ocean wave properties (20 min). The use of short segments that contain a group of high waves may overestimate the significant wave height and in this way may lead to ignoring of some of rogue events. Also, it remains unclear how time series of surface elevations are extracted using wavelet transform; probably it is meant that properties of rogue waves are extracted from the time series of surface elevation using this technique.**

We agree that the time series are indeed shorter than conventional sections of 20 min length. This duration comes from the raw data and cannot be increased. Consequently, the value of $H_s$ may be overestimated in the records containing very high waves, and some rogue waves (according to condition $H/H_s > 2$) may be missed. This point is mentioned in the new version of the manuscript in the sentence just after Eq. (1):

*"Note that we consider short segments of surface elevations (about 8.5 min instead of conventional 20 min), what may result in some overestimation of $H_s$, and hence may lead to ignoring some rogue events."*

The wavelet transform is used in the procedure of conversion of the record of buoy acceleration to the time series of surface elevation. All the analysis reported in the paper concerns the surface elevation time series. The corresponding sentence is corrected as:
*"The 512 second long sections of time series of surface elevations are produced from the raw data of buoy accelerations with use of wavelet transform (see details in Lee et al., 2011)."*

**1.3. The description of the location of the measurement buoy on page 5784, line 19 is not consistent with, e.g., a similar description on page 5783, line 7.**

The referee is mistaken. The initial locations, $x = x_0$, are negative values, whereas the distances to shore, $-x_0$, are positive.

**1.4. The description of how different classes of rogue waves are shown in Fig. 5 (page 5787, line 23) is not consistent with the relevant figure caption.**

The description and caption are reformulated.

**1.5. It is not clear what is meant by "fleeting events" on page 5788, line 7.**

'*Fleeting*' changed to '*transient*'.

**1.6. … the reader might benefit from a certain extension of the reference list. For example, a preliminary version of the classification of rogue waves ... has been provided in Didenkulova (2011). ... the reader might benefit from mentioning that in even shallower water other equations ... and other basically weakly nonlinear mechanisms ... may produce similar long-living rogue waves and thus the threats associated with freak waves may easily extend to the immediate nearshore as highlighted in Nikolkina and Didenkulova (2012).**

The suggested references with corresponding discussions are added to the text.

**1.7. The weakest element of the manuscript is the use of English...**

The language and the entire text are improved with the obliging help of Prof. Tarmo Soomere. We do hope that in the present form it is acceptable for publication.

## <u>2. The Second Referee's Report</u>

**2.1. It would be more informative to show the spectra of the rogue wave measurements, since the validity of the NLS choice depends on the narrowness of the spectrum. This brings us to the Benjamin-Feir index, as mentioned in the manuscript a variable which depends on the bandwidth of the corresponding spectrum. There are several possibilities to define the spectral bandwidth (half the width of the half energy, using the Goda-peakdness-parameter etc.). The chosen method should be mentioned in the manuscript.**

The method how the spectrum width is determined is described in the new version of the text (via second moment of the energetic spectrum for free waves).
The spectrum width is indeed a very important parameter relevant for applicability of the NLS model. On the one hand, the NLS equation is derived for the limit of a narrow spectrum. Strictly speaking, the typical spectrum of wind waves is not narrow, and the accuracy of the results provided by the NLS framework is questionable.
On the other hand, sometimes this framework captures reasonably well even wave groups with wide spectrum (see for example A. Chabchoub, N. Hoffmann, M. Onorato, A. Slunyaev, A. Sergeeva, E. Pelinovsky, and N. Akhmediev, Observation of a hierarchy of up to fifth-order rogue waves in a water tank. *Phys. Rev. E* 86, 056601-1–6 (2012)). Irregular wave dynamics also may be rather well described by the NLS framework (e.g., L. Shemer, A. Sergeeva, A. Slunyaev, Applicability of envelope model equations for simulation of narrow-spectrum unidirectional random field evolution: experimental validation. *Phys. Fluids* 22, 016601-1–9 (2010)). The Benjamin - Feir Index is probably the best example of efficiency of the NLS theory. This parameter, formulated on the basis of the NLS theory, is now being applied to situations with truly wide spectrum.
Therefore we do have some confidence that the NLS framework employed in this research provide with realistic description of the general evolution (or trends of nonlinear wave evolution), although cannot guarantee an accurate description of the particular wave dynamics.

The following remark is added after Eq. (5):
*"When computing BFI, the spectrum width is determined through the second spectral moment within the range 0.4…1.2 rad $s^{-1}$ (it corresponds approximately to the free wave spectral domain). The in-situ wave spectra have different shapes, and generally are not narrow; hence generalized equations for wave modulations should be more accurate."*

**2.2. The authors present the variable bathymetry, related to the exact locations. This is of important relevance for the modeling of course, but it is also crucial to know, if the considered waves were effectively propagating in the direction of the presented uneven bathymetries. This information is missing in the current version.**

Indeed, this is our assumption which was not stated explicitly. We do this in the new version of the text (after Eq. (3)):
*"Here and after we assume that waves propagate onshore strictly in the direction of the presented bathymetry."*

**2.3. Furthermore, there are more accurate weakly nonlinear evolution equations, describing the nonlinear wave motion in finite depth, see Slunyaev (2005). The numerical and analytical difficulties dealing with these extended models should be explicitly discussed in the text. Finally, a short discussion on wave breaking, not taken into account in the NLS model, and the accuracy of the results should be added.**

It is rather straightforward (though technically tediously) to extend the model equation taking into account higher order terms (hence considering analogues of the Dysthe equations or higher order extensions). By this way we could improve the description of waves with not very narrow spectrum (see our reply 2.1 above). The envisaged difficulties may be of two sorts: [i)] much more complicated expressions for coefficients, and [ii)] possible problems with stability of the code. However, we are sure that other uncertainties which we cannot control or even estimate (directional effects, accuracy of reconstruction of the fluid velocity, etc.) make the further improvement of the model equation superfluous, and do not consider this possibility at this point.