

Interactive comment on “Preliminary investigation on the coastal rogue waves of Jiangu, China” by Y. Wang et al.

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Comment 1-1: I have a strong belief that the observation of the lower probability of the registered rogue waves is due to the relatively small water depth. Some other observations of coastal rogue waves confirm this expectation, including publications by Yasuda & Mori (1997) and Mori et al (2002) cited in the manuscript. Therefore the measuring conditions related to the local water depth should be discussed in more details. On the basis of Fig. 5 I could estimate the wave period as 4 s; for water depth 9 m the linear dispersion relation gives wavenumber $k \approx 0.257$ rad/m, thus $kh \approx 2.3$. This is the intermediate depth situation, and the modulation instability conditions are strongly affected. In particular, the BFI number is effectively reduced about twice. The

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water becomes even shallower during the low tide ($kh \approx 1.5$). These details seem to be very important; the typical wave periods and dimensionless depth parameters for registered wave sequences, kh , should be given in the manuscript. The method how the BFI number is computed on the basis of the time series should be described. The definition of H_s used in the study should be formulated (is it $H_{1/3}$ or 4, etc.).

Response: The reviewer is appreciated for his professional and nice suggestions. The water depth is important as the reviewer said. We tried our best to collect the varying depth value and provide a new figure 7. It shown that just like the reviewer estimate, most of the rogue waves are present in the intermediate water depth. The corresponding explanation is also provided related to the Benjamin-Feir instability.

Comment 1-2: I have strong doubts about the wave shown in Fig. 10. One may notice that the record gets much rougher just after the rogue wave event. The significant wave height of 32 cm corresponds to a calm sea condition, and the maximum wave height just slightly exceeds 1 m. I have a strong suspicion that some boat could hit the buoy or even drift with it for some time. The boat attachment and then beating between the boat and the buoy could explain this extraordinary record. This time series with the record amplification $H/H_s = 3.14$ results in the spiky data in Fig. 8. Without this point in Fig. 8 the second proportion for larger H/H_s becomes groundless. The record in Fig. 10, i.e., the qualitative difference in the appearance of the record before and after the large wave must be discussed. In this connection I suggest to present plots of few other time series containing rogue waves with $H/H_s > 2.5$.

Response: After a systematical checking for the time series shown in Fig. 10, we agree with the reviewer's opinion. We have deleted this value in the Fig. 8, while we still find there exist a new trend as shown in new Fig.9. We know that it is just a rough idea and we will do more research on this topic.

Comment 1-3: Section 4 “The paradox of nonlinearity characterization” in my opinion is a result of inadequate understanding. The authors claim that the appearance of

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the rogue wave in Fig. 5 contradicts with the statistical analysis, which exhibits the presence of strongly nonlinear waves. Firstly, I estimate the wave steepness in Fig. 5 as $ka \approx 0.26$, which is steep but far from the breaking onset. Therefore the wave asymmetry may be hardly seen by an eye. The relatively low resolution of the time series complicates the observation as well. Secondly, the authors do not specify the peak values of the statistical moments in numbers, these values should be provided.

Response: Based on the reasonable suggestion by reviewer, we make a new picture Fig.5. As the reviewer mentioned, the rogue wave steepness is far from the breaking point. We also provide a detailed numbers and discussion in the related section 4.

Comment 1-4: The last paragraph in Concluding remarks contains the discussion about the mechanisms leading to rogue waves of two suggested kinds. I do not see any arguments given in the manuscript which could help to attribute the waves of the first kind to the linear superposition, and the waves of the second kind to some other mechanism. These statements should be justified somehow or cancelled.

Response: This paragraph is rewritten. Some parts are cancelled as suggested.

Comment 1-5: The selection of references is not always perfect. In particular, on page 6594: Onorato et al (2006a) contains a laboratory work, but not the theory. Onorato et al (2006b) is dedicated to a specific mechanism which is to act in crested seas only and cannot be compared with the presented data. Osborne (2010) is a monograph aiming at a specific perspective of application of the Inverse Scattering Technique to oceanic problems. Besides two publications of Toffoli et al there are a lot of other researches performed in the realm of physical experiments.

Response: This paragraph is re-organized as suggested.

Comment 1-6: The first sentence in Sec. 5 needs a bibliographic reference support.

Response: This sentence and related part are all re-organized.

Comment 1-7: Figures and Typos

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Response: All the details are checked and modified as suggested.

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