Review

on manuscript by Y. Wang et al "Preliminary investigation on the coastal rogue waves of Jiangsu, China" submitted for publication in NHESS.

The paper presents the investigation of long-time in-situ surface wave measurements near the coast of China. I am not aware of any reports on rogue waves measured near the mainland Chinese coast (at least, published in English), and therefore the disclosed information is of significant interest. The reported results are incomplete; on the other hand, the paper is suitably short, and the preliminary nature of it is stated in the title. Therefore I find the manuscript interesting and suitable for publication in NHESS after it is modified taking into account the criticism given below. I am generally satisfied with the descriptive part of the paper, though cannot agree with some analysis given in the paper. This issue needs revision.

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 modulational instability conditions are strongly affected. In particular, the BFI number is effectively reduced about twice. The 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 4σ, etc.).

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 $H/H_s > 2.5$.

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

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.

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.

6) The first sentence in Sec. 5 needs a bibliographic reference support.

Figures:
Fig. 2: It is difficult to see the location of measurements. I suggest to prepare it with more contrast and to provide a scale rule.
Fig. 3 in its present form is useless, the difference between the raw data and the processed data cannot be seen.
Fig. 4: The plots should be stretched out in the horizontal direction.

Typos:
Page 6596, line 1: Correct to: Didenkulova **et al** (2006)
Page 6597, line 2: Words ‘wave buoy’ repeat twice
Page 6597, line 17: What is the dimension of the expression $\pm(0.1 + 5\%H)$? Is it meter? Explanation is required.
Page 6599, lines 8, 9: Correct to: Kimura **& Ohta** (1994), Liu **et al** (2004).
Page 6600, line 22, page 6601, line 3: Correct rouge to rogue.
Page 6601, line 23: Correct to Fig. **9**.
Page 6605, line 29: Correct to Bitner**-**Gregersen