Review

on the manuscript "*Contribution of solitons to enhanced rogue wave occurrence in shallow water: a case study in the southern North Sea*" by I. Teutsch, M. Bruhl, R. Weisse, S. Wahls submitted for publication in the **NHESS** journal

In this paper the in-situ data are analyzed using the Inverse Scattering Transform (IST) under the assumption that the waves may be approximated by the Korteweg – de Vries equation. An abnormal statistics of waves measured in a particular site in shallow waters off the coast of the island Norderney, motivated the research. In contrast to measurements at other locations, analyzed in the preceding paper by Teutsch et al. (2020), measurement by a buoy at Norderney demonstrated an enhanced rogue wave occurrence probability, which could not be explained by the Forristall distribution. To the best of my knowledge this is the first time when in-situ measurements are analyzed systematically with respect to the soliton content. The authors show a clear correlation between rogue wave occurrences and the presence of 'outstanding' solitons in the wave fields, and do their best to prove reliability of this result.

It is generally accepted that the 'nonlinear spectrum' provided by the IST should better characterize the dynamical properties of waves with prominent nonlinear coherence. Therefore I appreciate the approach and the work in general. However, the manuscript in its present form has two serious drawbacks. First, the introductory part is written unacceptably badly. Its author is obviously not a specialist in the topic of integrable equations and related issues. The introduction contains statements which are wrong, illogical or can be easily misinterpreted; the references are not always appropriate. There are too many issues to be listed all in my report. I assume that this problem may be solved with the help of co-authors of the work, some of whom seem to be highly qualified in the concerned field of science.

The other problem is that the analysis is based on the assumption that waves may be approximated by the KdV equation, and the revealed by the IST discrete spectrum indeed corresponds to physical solitons which are long-lived wave structures which may be measured upstream and downstream the location of measurements. This point cannot be justified on the basis of single-point measurements. From the general viewpoint, fields of KdV solitons are typically characterized by very asymmetric (cnoidal-type) waves, so that solitons could be recognized by eye. Nothing of this kind can be seen in the presented waveforms. Besides, the estimated soliton amplitudes are not very large, the solitons do not dominate. My personal opinion is that the chance, that the conclusion of the work is wrong, is 80%. The revealed solitons most likely characterize the wave shapes, which do not correspond to physical long-lived KdV-like solitons. I express my alternative vision below. I assume that the manuscript may be published only with a clear discussion that the employed assumption is crucial, but cannot be firmly justified within the frames of the available data. This should be mentioned in the abstract and the conclusion as well.

I have two alternative ideas why the enhanced probability of large waves is observed at this site. First, the location of measurements corresponds to a strongly varying bathymetry (line 138). The authors mention the recent progress in understanding the underlying mechanisms of rogue waves occurring under the depth change conditions (line 376). I wish to draw the attention to another recent work [Ducrozet et al, 2021], where envelope solitons are shown responsible for the wave amplification when the depth increases.

The second idea is based on the discussion in lines 108 and further on. There the theoretical result, which describes four-time wave amplification due to the oblique interaction of KdV solitons, is mentioned. It is an essentially directional effect, which indeed may explain peculiarities of the statistics for crossed shallow water waves. This is somehow confirmed in lines 407-408. However, this effect cannot be considered within the KdV theory, and hence it cannot be revealed using the KdV-NLFT. If this effect, however, takes place indeed, the amplified waves may be interpreted by the KdV-NLFT as ones containing intense solitons, while this will not correspond to the physical essence of the phenomenon.

See also my comment to lines 276-277.

I wish to draw the authors' attention to the work [Slunyaev et al, 2006] where NLS solitons revealed in the instrumental records of deep-water waves using the IST were related to rogue wave events. Accuracy of this procedure for strongly nonlinear waves was estimated in [Slunyaev, 2018]. Observation of a long-lived NLS soliton in the field of strongly nonlinear waves was made in the numerical simulations by Slunyaev (2021) and in-situ in [Onorato et al, 2021].

The KdV-soliton content in sinusoids was discussed in the work by Giovanangeli et al (2018); it may be useful.

Lines 44, 83. Application of the NLS theory is not limited by the condition $kh > 1.363$. The de-focusing NLSE may be derived for shallow water waves. The focusing (deep water) and defocusing (shallow water) NLSEs are equations on wave modulations, while the KdV equation describes the wave displacement. Therefore the phrase "*The shallow-water equivalent to the NLS equation is the Korteweg–de Vries*" in line 83 is essentially inaccurate.

Line 48. Under the deep-water condition, the most unstable perturbations are directed in the longitudinal direction, not oblique, see [McLean, 1982a,b].

Line 52. I believe that the BFI parameter was introduced for characterization of irregular wave statistics for the first time in [Onorato et al, 2001].

Line 55. I would not say that exact breather solutions explain the physics of the modulational instability. They simply describe the dynamics.

Line 61. The paper by Shrira & Geogjaev was published in 2010, not 2009.

Lines 67-73. It is reasonable to say more precisely, what is called shallow water, in terms of the dimensionless depth. At $kh < 0.5$ the effect of the modulational instability disappears.

Line 78. Below in the text, the authors mention a great number of recent works on the effect of rogue waves under the conditions of variable depth. Hence, the statement "*so far only few studies have addressed the impact of bathymetry on rogue wave generation*" is not consistent. The effect of finite depth on the structure of rogue waves described by the NLSE was directly discussed in [Didenkulova et al, 2013].

Lines 97-99. The works by Zabusky & Kruskal (1965) and Peregrine (1983) are fundamental in this story, but the references to them are inappropriate. In the former paper they consider periodical domain, hence could not observe the wave decay; while the latter paper is dedicated to the NLSE, not KdV. It is better to cite here some classical textbook on the IST in infinite line.

Line 102. The result of the work by Pelinovsky et al. (2000) is inverted. In fact, they show that the wave train contains **at most** one soliton, not at least.

Lines 103, 257. The guess that the interaction between many KdV soliton can lead to a rogue wave formation is not supported by the theoretical findings [Slunyaev, 2019] and direct numerical simulations, e.g. [Dutykh & Pelinovsky, 2014; Pelinovsky & Shurgalina, 2017]. Thus, it is wrong.

Line 117. There are two more recent publications from this series: Costa et al. (2014) and Osborne et al. (2019).

Fig. 2. I suggest to show the scale (meters) in the map.

Eq. 1. Why this threshold for the shallow-water condition is used? It is related to the Benjamin – Feir instability, but not to the applicability of the KdV theory…

Line 155. I believe, the number '1' should be removed from the formula for T_p.

Table 1. The caption is wrong.

Table 1. In the time series the data acquisition frequency is rather low, thus the waveform resolution is poor, what may complicate analysis of the wave shapes. According to the table, the amount of

rogue waves of the types "crest rogue", "double rogue" and "extreme rogue" are at most 1%. How reliable is the casting into these classes?...

Line 211. I am not sure that the words "*the frequency axis has no physical meaning*" are correct. As I understand, the horizontal axis in fact corresponds to the inverse duration of the soliton. Since for the KdV solitons the relation AT^2 = Const holds (where A is the soliton amplitude and T is its duration, see Eq. (12)), then the "frequency" (~1/T) may be a more sensitive parameter than the soliton amplitude. This property is used de-facto by the authors to distinguish the "outstanding" solitons.

Lines 267, 269. The words "right" probably do not correspond to Fig. 9.

Lines 276, 277. I interpret the conclusion in the way that outstanding solitons are not indicators of rogue waves (in terms of the quantity H/H_s or similar), but are indicators of large waves in general. Then, I can assume that they do not correspond to physical solitons, but rather artifacts of application of the weakly nonlinear theory to the analysis of strongly nonlinear waves. Hence, they are fakes.

A similar conclusion seems to follow from lines 428-429, where it is mentioned that "double" and "extreme" rogue samples exhibit the effect most clearly.

Fig. 10. The dimensions of the vertical axes are not given.

Table 3. I suggest to mention in the discussion of this table, that depending on the depth, the amplitudes of the solitons change. The soliton amplitude is controlled by the square root of the Ursell parameter (12), $U \sim h^{-3}$, hence the amplitudes should depend on depth as $h^{-3/2}$. Therefore, a 20% change of the depth will result in roughly 30% change of the amplitudes, if my estimations are correct.

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