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

resubmitted for publication in the **NHESS** journal

The revised version seems to be noticeably improved compared to the original one. However, I still have a significant list of critical remarks. Though I appreciate the work in the whole, sections 1 and 2 are generally poorly written – in the sense that they are obviously authored by a beginner who is not a strong specialist in the theory of nonlinear waves, and in particular the KdV equation. They are barely acceptable. I do not understand this situation, as there are experienced people among the authors.

The introductory part still looks like a collection of unsorted pieces of information which do not form a complete picture. The introduction would benefit if it is shorter but contains clear messages. The selection of references sometimes looks random. For example, I do not understand why the references to Peregrine (1983) are given in lines 120 and 147. It is a good paper, but **it is not at all** about the KdV equation and corresponding IST method. The statement that "*For vanishing boundary conditions, the soliton spectrum completely describes the behaviour of the wave train in the far field*" (Line 264) may be found in any classic book on the IST, not in the recent paper by Prins & Wahls (2019). Eq. (10) is a basic consequence from this statement, therefore the given list of 4 references with equation numbers is absolutely unreasonable. There are other similar examples.

I urge the authors to revise the introductory part of the work, as well as to take into account my comments below, before the article can be recommended for publication.

Line 21. The authors cannot insist that the revealed waves are indeed solitons (i.e., travel preserving their individuality), therefore it seems reasonable to slightly weaken the sentence in the abstract as follows: "*These results suggest that soliton**-like** and nonlinear processes…*"

Line 35. Please expand the abbreviation "ADCP".

Line 60. The words "*including both nonlinearity and dispersion*" are superfluous and should be removed. This is already said by "*weakly nonlinear narrow-banded approximation*".

Line 75. "*…in terms of linear waves, Stokes waves and breathers*". I believe, this list is not correct. A breather is a coherent wave structure, whereas a Stokes wave is in this sense a free non-linear wave. Therefore I suggest writing as "*in terms of quasi-linear waves and breathers*" or "*in terms of Stokes waves and breathers*".

Line 89. The sentence "*The NLS equation was used as an approximate model of the wave dynamics*" is actually repeats the content of the previous sentence and should be deleted.

Line 99-100. The condition $kh < 1.36$ makes unidirectional waves modulationally stable, what is not sufficient for applicability of the KdV equation. Here and after the authors refer to Osborne & Petti (1994), but these authors discussed the 'cutoff period' for KdV as $kh = 1$ (see their Fig. 3). I did not find a condition of this sort in the second reference Osborne (1995). Bearing in mind that the KdV theory takes into account the two first terms of the Taylor expansion for the dispersion relation ($\tanh(kh) \approx kh -$

1/3 $(kh)^3$), it is obvious that the request of applicability should be at least $(kh)^2 \ll 1$ (assuming that the waves are small enough in amplitude). This comment is also valid for Eq. (1), line 250.

Line 121. It is better to say "*The* ***regular wave*** *solutions of the KdV are stable…*"

Line 122. It is better to delete the strange sentence "*This is the mathematical explanation of why rogue waves in shallow water cannot be a result of the modulational instability.*" The modulational instability is absent under the discussed conditions. Therefore the modulational instability cannot be a reason of anything, and a 'mathematical explanation' is not needed.

Line 137. I assume that the sentence "*Costa et al. (2014) found a method to filter soliton trains from measurement data by a linear Fourier transform for the KdV equation with periodic boundary conditions and associating them with wave packets*" is not sufficiently accurate. In Costa et al. (2014) they use the linear Fourier transform to estimate the power law spectrum, which is then shown (using the **nonlinear** method) to be related to solitons. Thus, they use the linear method to see some evidence of hidden solution but not "*to filter soliton trains*".

Fig. 2 caption. What is "NN+m" ?

Line 245. It is not clear from the description, if there were rogue waves satisfying the criteria (5) and (6) simultaneously?

Eqs. (13) and (14). As discussed in the text, these 'different' definitions of Ursell parameter lead to the values which are proportional. Therefore these definitions are essentially the same. The only difference is in the reference (the threshold between soliton- and non-soliton solutions). Besides, what is the need to introduce a new quantity m, which is identical to k^2?.. These are unnecessary details.

Lines 394-395. The subscripts of amplitudes A_1, A_2 are given by the regular font, not lower case.

Line 455. The sentence "*Another indication that the soliton spectrum alone is not sufficient to explain the presence of rogue waves is given in Fig. 10, which shows that the shapes of most rogue wave crests are not soliton-like*" is incorrect. It may be concluded from the examples and the discussion provided in the paper, that the revealed solitons have very little in common with the observed extreme wave shapes (see e.g., Fig. 5, top). Therefore they provide no information about rogue wave crests themselves.