Reply to Anonymous Referee #1

We thank Referee #1 for the effort he/she put into revising our manuscript, and the constructive comments that have helped us to a major improvement of the manuscript.

Regarding the additional comment on rogue wave statistics in intermediate water that was suggested by Referee #1:
Following the suggestions of Referee #2, we have strongly revised our Introduction. Its topic is now mainly focused on the analysis of rogue waves using NLFTs. As there was no longer a good location to place the references concerning rogue wave statistics in intermediate water in the Introduction, we have decided to leave the subject of intermediate water rogue wave occurrence frequency in the Discussion, as not to interrupt the new thread of the Introduction.

We also thank the reviewer for pointing out a misleading statement in our previous version. We have adjusted the statement as suggested to “Doeleman (2021) recently showed in tank experiments that the effect of slope is weakened in shallow water.” (see line 525 of the revised manuscript, track-changes version).

Reply to Anonymous Referee #2

We thank Referee #2 for the additional report, which has helped us re-structure and improve the manuscript.

We have especially put an effort into revising the Introduction section. Following the suggestion of the reviewer, we have re-structured the section to improve the cohesion of the text. The parts regarding nonlinear equations and the NLFT have furthermore been rewritten. We also removed some less relevant parts, and added some sentences to better guide the reader. Furthermore, we have revised our references and chosen classic textbooks where possible. We hope that in this shape, our introductory part appears more convincing to the reviewer and the reader. We finally also improved some formulations and references in the Methods section and split the last paragraph between the Methods and the Results section.

In the following, we address the comments of the referee that concerned specific lines in the manuscript. *The original reviewer comments are listed in italic font.* Our answers to the comments are shown in blue color. The revised lines and mentioned page numbers refer to the new *track-changes version*.

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…"*
We have changed the sentence in the abstract accordingly (line 21).

Line 35. *Please expand the abbreviation "ADCP".*
Thank you, we have added the full expression to the text (line 35).

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".*
We agree with the reviewer and have removed the doubling (lines 64/65).

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".*
The sentence has been removed in the new revision, due to re-formulation of the paragraph (lines 102/103).

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.*
The sentence has been removed in the new revision, due to re-formulation of the paragraph (line 114).

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) ≈ kh – 1/3 (kh)3), it is obvious that the request of applicability should be at least (kh)2 ≪ 1 (assuming that the waves are small enough in amplitude). This comment is also valid for Eq. (1), line 250.*
In Osborne & Petti (1994), the KdV cut-off frequency is indeed defined around *kh=1*, but note that this is not considered a strict bound in that paper. They write that "The value f_{KdV} provides a rough definition for a cutoff frequency, far to the right of which, in the spectral domain, KdV evolution cannot occur." (emphasis added for clarity).
In the later paper Osborne (1995), the cut-off frequency $f_{KdV} = 1.36\, c_0/(2\pi h)$ is used in a more strict sense: "Determine if most of the wave energy lies to the left of a KdV 'cutoff frequency,' $f_{KdV} = 1.36\, c_0/(2\pi h)$". Note that this cut-off frequency corresponds to *kh=1.36*, as the following calculation shows. The frequency bound in Osborne (1995) is

$$\frac{1.36\, c_0}{2\pi h} > \frac{1}{T} = f$$

We multiply both sides of the inequality by $\frac{2\pi h}{c_0}$, and arrive at

$$1.36 > \frac{2\pi h}{c_0 T} = \frac{2\pi h}{L} = hk$$

where *L* and *T* are the characteristic spatial and temporal periods and $c_0 = \sqrt{gh}$. The reviewer is correct in that this is the point where the modulational instability disappears.

We updated the description in the paper to reflect that *kh* should be small due to the approximation of the dispersion relation, and that, following Osborne (1995), we used the criterion *kh≤1.36*. The text in the paper has been updated as follows:

“Osborne and Petti (1994) point out that *kh,* with *k* and *h* denoting wave number and water depth, respectively, should not be much larger than one for the KdV equation because of how the dispersion relation is approximated. The threshold kh≤1.36 marks the point at which the modulational instability disappears (ibid.). Following Osborne (1995), we use this threshold to define shallow-water conditions in this work.” (lines 150-153).

Line 121. *It is better to say “The regular wave solutions of the KdV are stable…”*
We have added the insertion as suggested (line 153).

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.*
The reviewer is of course correct in that the modulational instability cannot cause rogue waves if it is absent, but we would like to point this out explicitly for readers that are not familiar with the area. We changed the sentence to “Therefore, the modulational instability cannot contribute to the explanation of rogue-wave occurrence in shallow water.” to avoid the “mathematical explanation” part (lines 155/156).

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”.*
We thank the reviewer for this comment. The sentence has been changed to “Costa et al. (2014) used the periodic KdV-NLFT to confirm the soliton content of low-pass filtered time series measured in the Currituck Sound during a storm.“ (lines 181/182)

Fig. 2 caption. *What is “NN+m” ?*
The height values refer to German “Normalhöhennull”, an official vertical datum used in Germany. In the revised version of the manuscript, we have replaced “NN” by the explanation that this represents the standard elevation zero of the German reference height system.

Line 245. *It is not clear from the description, if there were rogue waves satisfying the criteria (5) and (6) simultaneously?*
We did not introduce a specific category to include rogue waves according to both criteria (5) and (6), as this category would have been too small to be statistically significant. Therefore, rogue waves satisfying criterion (6) **can** also satisfy criterion (5), but then they are not treated separately.

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 k2?.. These are unnecessary details.*
We would like to point out that we have added the second definition and further explanation as a reaction to a comment by Referee #1 in the first iteration. However, as not to interrupt

the reading flow, we have now moved the equation and details to a footnote in the updated version of the manuscript. This is also in agreement with the suggestion of Referee #1. (footnote on page 24)

Lines 394-395. *The subscripts of amplitudes A1, A2 are given by the regular font, not lower case.*
We have adjusted the font accordingly.

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.*
The reviewer is right. We have removed the sentence in question from the Discussion (lines 503/504).

References

Costa, A. et al., 2014. Soliton Turbulence in Shallow Water Ocean Surface Waves. *Phys. Rev. Letters,* 113(108 501).

Doeleman, M. W., 2021. Rogue waves in the Dutch North Sea.

Osborne, A. R., 1995. The inverse scattering transform: Tools for the nonlinear Fourier analysis and filtering of ocean surface waves. *Chaos Solitons Fractals,* 5(12), pp. 2623-2637.

Osborne, A. R. & Petti, M., 1994. Laboratory-generated, shallow-water surface waves: Analysis using the periodic, inverse scattering transform. *Phys. Fluids,* 6(5), pp. 1727-1744.