Review

On the manuscript **"Contribution of solitons to enhanced rogue wave occurrence in shallow water: a case study in the southern North Sea"** by Ina Teutsch, Markus Brühl, Ralf Weisse and Sander Wahls.

Overview: Conditional Acceptance Upon Major Revision

The paper presents a possible explanation of rogue wave statistics collected over six years in the coast of Germany, consisted of intermediate and shallow water regimes. The authors argue that given the failure of second-order models to correctly describe rogue wave statistics in their previous article (Teutsch et al., 2020), the interaction between solitons and the linear oscillatory wave components of entire time series might be a viable alternative theory/process, whose methodology relies on the analysis of the soliton spectrum. Clearly, the topic is of the highest relevance and of cutting-edge nature in ocean sciences, while also being within the scope of the journal. Provided major amendments are implemented, I believe the revised version of this paper would be an essential reading for everyone studying extreme waves in the ocean.

I would like to provide specific and general comments on three different types:

1. General comments that address references, explanation of scientific terms and further clarifications that are essential for the general reader to follow the scientific reasoning behind this work.
2. Suggestions that would improve the scientific quality of the manuscript.
3. Strong scientific issues in the manuscript that must be revised before I can recommend acceptance.

1. General Comments

I believe the reading quality of the paper is fair but can be significantly improved. Unfortunately, the organization, literature coverage and introduction of scientific terms could have been better implemented. Hence, the suggestions and requests follow:

1A. There is no introduction to what are rogue waves. **Please, introduce rogue waves and their relevance as a natural hazard properly.** For instance, design waves are necessary for the construction of the state-of-the-art vessels and offshore structures, and rogue wave likelihoods are essential for classification societies of ocean engineering (Bitner-Gregersen and Gramstad, 2015).

1B. In the beginning of the first paragraph it is stated:

"There has been a lively discussion on whether the occurrence frequency of rogue waves in the open ocean is well described by second-order models. Both Rayleigh (Longuet-Higgins, 1952) and Weibull distributions (Forristall, 1978) have been used to describe the distributions of wave and crest heights."

The referenced distributions are not of second-order[1]. It may be the case that this is simply a jump in the story being told in the introduction, and readers may believe these distributions are indeed of second-order. **Please add the references for the discussion about second-order models range of validity.** Given that these distributions are actually of first-order[2], **I suggest you clearly state so and preferably write this before you discuss second-order models.**

[1] Whenever second-order models are mentioned they mean second-order in steepness (Tayfun and Fedele, 2007).

[2] That is, for the superposition of linear waves (see section 2 of Longuet-Higgins (1952)).

1C. A few lines down the first paragraph, I find the statement:

"Independent of the measurement device, some authors found measured wave heights to agree well with the established distributions, while others found the frequency of rogue wave occurrences over- or underestimated."

Please be specific and add the references for these distributions and studies. While this is a correct statement, not every reader knows which studies you refer to. However, after this statement the authors provide a few examples. Unfortunately, these are very few examples among several dozen works. **It would be better to cluster all studies[3] known to the authors as agreeing (references), overpredicting (references), underpredicting (references).** Such a suggestion would avoid the following issues found in nearby statements as:

"While Olagnon and van Iseghem (2000) found rogue wave occurrences to be overpredicted by the classical distributions."

What are classical distributions? This is quite confusing for both experts and first readers of rogue wave research.

"Rogue wave occurrences in buoy data from the US coast, recorded in shallow, intermediate and deep water, were found to be strongly overestimated by a Rayleigh distribution."

Again, where is the reference? I assume this is Cattrell et al. (2018). Or is it the Baschek and Imai (2011) study cited one sentence later? **The discussion between lines 25 and 30 must be entirely rewritten. May I suggest that you explain when first-order distributions have different outcomes (agreement, overprediction, underprediction), then move on to second-order models, and always include the references for the models as well as for the studies assessing their predictions.**

1D. Lines 30-31 have a confusing statement from the grammar point of view:

"Furthermore, the respective authors describe local differences in rogue wave occurrence frequency between their measurement stations (Baschek and Imai, 2011), depending on the wave climate and especially in coastal waters, where waves interact with the seabed (Cattrell et al., 2018; Orzech and Wang, 2020)."

The use of "the respective" is in contradiction with the citep format. **I suggest you simply remove these two words.**

1E. You should probably add three further comments on large data sets and their conclusions:

Firstly, **that Karmpadakis et al. (2020) showed that distributions can perform well in a narrow range of sea parameters (steepness, bandwidth and so on) but no available model performs well in a wide range of sea parameters. In my opinion, this is the single most important finding in recent years.**

Secondly, **you should cite Häfner et al. (2021)[4] as another crucial real ocean study that provided unfavorable evidence for established models, including modulational instability.**

Thirdly, to address Karmpadakis et al. (2020), **Mendes and Scotti (2021) showed that a theoretical superposition of established complementary models that individually depend on steepness and water depth can describe the unexplained uneven distribution of rogue waves in deep and intermediate waters reported by Stansell (2004), that is, can describe why some sea states (steepness, depth, etc) have higher rogue wave frequency than others.**

[3] There is no need for an exhaustive list.

[4] They have cited this article, although at the discussion section and never mentioned its main results as described here.

1F. You should probably add some references after line 42 stating that **there is no continuous probability model that describes both deep water and shallow water at a wide range of sea states**[5] **(Massel, 2017; Karmpadakis et al., 2020).**

1G. You should probably add some references after citing Benjamin and Feir (1967) on line 51 stating that **the BFI/Modulational instability do not perform well in real ocean time series (Fedele et al., 2016; Häfner et al., 2021).**

1H. You should be clear with the term "nonlinearity" on line 65 onwards. In many articles, nonlinearity is a code-name for either significant steepness or the Ursell number. **I suggest that when you speak of nonlinearity in general, you rewrite it as nonlinear processes**.

1I. You should add somewhere near the citation of Slunyaev et al. (2016) **that half a century earlier through depth-dependent breaking criteria Glukhovskii (1966) already predicted that rogue waves becomes less frequent than expected by Longuet-Higgins (1952)**, see Wu et al. (2016) for a review.

1J. Lines 78-79 have inconsistent comments:

"However, so far only few studies have addressed the impact of bathymetry on rogue wave generation"

This statement is in clear contradiction with the brief description of bathymetry effect on rogue waves at the discussion section of this very preprint, citing not less than a dozen articles since 2012. **I suggest rephrasing this comment in accordance with the original intent but having in mind the discussion section and the recommended further literature below.**

1K. Between lines 376-384 you should add several crucial references/comments:

Numerical studies like Gramstad et al. (2013); Fu et al. (2021) showed that the slope of the bathymetry change plays a major role in rogue wave amplification. Moreover, Zheng et al. (2020); Lawrence et al. (2021) showed that increasing mild slopes increases the probability of amplification, whereas when slopes are too steep this effect is no longer noticed. Doleman (2021) recently further showed that the slope effect in shallow water is negligible, as opposed to intermediate water. Trulsen et al. (2020); Zhang and Benoit (2021) discussed that rogue wave amplification due to bathymetry only happens in a narrow range in water depth $0.5 < kh < 1.3$**, whose theoretical explanation is provided by Mendes et al. (2022). Further evidence that when the water depth becomes too shallow throughout the shoaling process the amplification dies out is provided by Xu et al. (2021). You should also add other important experiments, such as: Raustøl (2014); Jorde (2018); Bolles et al. (2019); Zou et al. (2019).**

1L. The Bruhl (2022) reference simply could not be found. It appears to lead to a course page at TU Braunschweig.

Scientific Improvement

2A. Lines 32-38 seemingly contradict the main results of Teutsch et al. (2020):

"In a previous study, we have analyzed measurement data from various stations in the southern North

[5] See in particular the second paragraph of section 4.4.5 of Massel (2017).

Sea (Teutsch et al., 2020) and found rogue wave frequencies[6] *to vary spatially and by measurement device. For data obtained from wave buoy measurements, we generally found rogue wave frequencies slightly overestimated by the Forristall distribution,(...)"*

I am intrigued by this comment, because figures 7 and 9 (which show the exceedance probability for all data sets combined) clearly do not support that Forristall distribution is overestimating the observed statistics. In fact, the first paragraph of section 3.3 of Teutsch et al. (2020) states the opposite:

"For wave heights up to twice the significant wave height, which corresponds to the threshold used to identify rogue waves, the measurement data are well described by the Forristall distribution. At a height of $H = 2H_s$, the data begin to deviate from the Forristall distribution. Both distributions increasingly diverge for larger relative wave heights, HH_s^{-1} . This suggests that in our data, rogue waves occurred more frequently than could be expected from the Forristall distribution. The frequency of rogue waves much larger than twice the significant wave height also exceeded expectations given by the Rayleigh distribution."

I can only believe this is a typo and the authors meant **that Forristall agreed with the statistics of large ordinary waves ($H < 2H_s$) but underestimated statistics of rogue waves ($H \geqslant 2H_s$). Please fix this contradiction.** Moreover:

"An exception was one measurement buoy, which was located in the shallow waters off the coast of the island Norderney, Germany (Fig. 1). For this buoy, enhanced rogue wave occurrence, which could not be explained by the Forristall distribution, was observed."

It is in contradiction with the previous text from the author's last paper based on the same data set.**The whole text has to be changed, as not just Norderney buoy showed much higher statistics than expected by Forristall (1978)**[7]**. This previous statement from the paper is problematic because it suggests that only shallow water statistics were not explained by Forristall and needs a new model, while Teutsch et al. (2020) showed that most stations (buoy or radar) feature the same underprediction by Forristall. I recommend the authors to analyze all stations where Forristall and Rayleigh underestimated rogue wave statistics, not only Norderney.** However, the Norderney sample contains enough waves to provide a statistically significant study of rogue waves in shallow water[8], so it is acceptable for the authors to focus their efforts on this data set alone, **provided they explain why this data set is representative of all others where the Forristall model underestimated rogue waves.**

2B. Page 20 introduces the Ursell number written in a very strange way, where it denotes the wave celerity by c while a capital counterpart C as wave crest. **I suggest you add a footnote explaining that many authors define Ursell as $Ur = HL^2/h^3$ and has a different scale for wave theories: for instance Dean and Dalrymple (1984) says Stokes wave must obey $Ur \leqslant 8\pi^2/3$. Your definition would be better written as $3/32\pi^2(HL^2/h^3)$ in which Stokes waves would find $Ur \approx 1/4$, in agreement with your discussion.**

2C. Again as in 2A, the comments on lines 368-370 find no agreement with the results published in Teutsch et al. (2020):

"The impulse for investigating the data at this specific site by using nonlinear methods was given by a previous study (Teutsch et al., 2020). There, it was found that while second-order distributions were sufficient to describe rogue wave occurrences at nearby stations in somewhat deeper water, the Norderney buoy

[6] Frequency is understood as the likelihood of appearance, as described in the caption of figure 7 onwards of Teutsch et al. (2020).

[7] Note that when distributions were separated by location in figure 8 of Teutsch et al. (2020), the buoy data of station WHS is in agreement with Forristall, while station LTH is underestimated by the latter.

[8] Rogue wave return periods according to Longuet-Higgins (1952) are of 1 RW per 2,980 waves, hence a data set (Norderney) with 15,000 samples of 30 minutes with mean period of $T_z \sim 5$s means a total of 5.4 million waves.

recorded a larger number of rogue waves than expected according to second-order theory"

Where in Teutsch et al. (2020) good agreement with Forristall (1978) in deeper waters is shown? It is also not of second-order. **On the contrary, table 1 of Teutsch et al. (2020) clearly shows that the stations named AWG and Clipper ranged from intermediate to deep waters, while WES and LTH remained mostly in intermediate waters[9]. Then in figure 9 Teutsch et al. (2020) delineates exceedance probabilities, of which AWG and Clipper in deeper waters are by no means in agreement with Forristall, and are actually more underpredicted by this distribution than the stations in shallow water.**

2D. The following statement on lines 374-376 is very misleading:

"Non-Gaussian wave characteristics as a result of decreasing water depth have already been described e.g. by Huntley et al. (1977) and gained increased attention in the context of rogue wave occurrence."

I have no idea why this reference appeared here. This text discusses no wave statistics whatsoever (I downloaded and read it), the only non-Gaussian characteristics is the run-up (important for some hazards but not for rogue waves) and the distribution is of run-up velocities, not of wave height measurements. They show the spectrum of run-up instead of a spectrum of wave amplitudes. In fact, the references of this article mentions papers related to swash, set-down, set-up and run-up over sloping beaches. Yet it does not mention to any previous wave statistics article such as Rice (1945); Longuet-Higgins (1952); Cartwright and Longuet-Higgins (1956); Longuet-Higgins (1963); Draper (1964); Glukhovskii (1966); Draper (1971); Jahns and Wheeler (1973); Mallory (1974); Earle (1975); Longuet-Higgins (1975); Haring et al. (1976) among others, and not cited by any relevant rogue wave review paper, which proves it has no connection with a rogue wave context. **I recommend the authors to remove this reference.**

Scientific Issues

The manuscript seems to contradict itself several times, often arguing that data suggests that large outstanding solitons are associated with rogue waves, only as a few lines below (found in several pages) discredit this suggestion. The conclusions do not reflect the discussion within the bulk of the text where it is argued that the soliton spectrum clearly can not account for the rogue wave statistics by itself. The reader would understand from the introduction and conclusions that the soliton spectrum is enough to predict rogue waves. The authors do not discuss or compute exceedance probabilities in the context of solitons, which diminishes the impact of the manuscript from an engineering and prediction perspective. Below I give more concrete constructive criticism:

3A. Despite discussing waves in shallow water and in a bathymetry with steep slope (line 138 and figure 2), the authors simply did not show values of water depth kh nor the slopes ∇h. **Please, provide this essential information. I should remind the authors that the definition of shallow water reads $h < L/20$ (Dingemans, 1997), or alternatively $kh < \pi/10$. I am not sure that all these data sets are actually within its mathematical definition of shallow water[10]. Moreover, KdV is restricted to slightly higher dimensionless depths of $kh \lesssim 1.4$ or $h/L < 0.22$ as the authors wrote it[11], but it is not the KdV that defines what the shallow water regime is. The authors should add a reference. Furthermore, the title should be rewritten as to convey either "Shallow Depths" or "Coastal Areas" that are consistent with the kh range.**

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[9] As defined by the shallow water limit $kh < \pi/10$.

[10] Actually, from table 1 of Teutsch et al. (2020) they are technically not in shallow water, and some stations have a lower range close to but not yet in shallow water.

[11] This can be found on page 559 of Dingemans (1997).

3B. On line 156 the authors refer to the wave celerity in shallow water as $c = \sqrt{gh}$, but as stated above, this formula is valid only for $kh < \pi/10$, whilst the authors use $kh < 1.38$ as the definition. **These two definitions are incompatible, which would affect the threshold for the peak period on line 159**. Although I understand that the 98% percentage of cases would drop to probably 90%, from the physical point of view you must be careful with definitions.

3C. Sample definitions ranging from "normal" to "extreme" on lines 166-186 are very problematic. **First and foremost, for engineering purposes a wave that exceeds a significant wave height of 15m (typical storm in the North Sea near Norway) by a 1.9 factor (28.5 m) is as dangerous as a rogue wave that exceeds it by a factor of 2 (30.0 m)! Secondly, as you may see on figure 14 of Zhang and Benoit (2021), nonlinear processes such as the shoaling of second-order waves will increase the likelihood of all large waves that are not strictly defined as rogue waves ($1.25 < H/H_s < 2$), so that it is highly biased to consider wave samples with maximum height $H/H_s = 1.8$ to be normal. Nonlinear processes are not restricted to waves with $H/H_s > 2$. Please change your text and definition accordingly.**

3D. Statistics based on samples can be misleading. **I suggest the statistical analysis to also include the total wave count/exceedance distribution as a complement. In this particular case, I suggest adding a new line in table 1 for the total count of waves and rogue waves according to each definition on page 11.**

3E. The article clearly draws the theories/methods developed by soliton interaction with linear waves in all the literature cited. Since the manuscript itself never conclusively claims what surface elevation they are actually covering, I would highlight the most important sources as I read in the manuscript: Osborne et al. (1991) and Bruhl and Oumeraci (2016). The results of Osborne et al. (1991) seem to be the core of the meaning of this paper: Solitons are hidden in the time series signal and can be detected with the NLFT. Additionally, Bruhl and Oumeraci (2016) argues that in depths not exceeding $h/L < 0.22$ cosine waves (or any oscillatory wave) are actually a transient wave composed by multiple solitons that mutually interact and at some point may disintegrate into separate solitons. Then, I can not understand why very little of this theory in these two articles are discussed in the introduction, without further expansion in the methods section. It took me quite a while to understand the relevance of the remainder of the manuscript without reading these two sources. Hence, the manuscript is not self-contained. **I believe the authors have to state the theory in these two articles very clearly and not briefly. Foremost, please make similar figures as 1a and 1d of Osborne et al. (1991), to show the non-expert what you actually mean by soliton spectrum (side-by-side with the linear spectrum like 1d) and how solitons are "associated" with rogue waves (like figure 1a). Second of all, please state mathematically how you handle/describe the sea surface elevation components as in equation 6 of Bruhl and Oumeraci (2016)[12] with a similarly robust explanation of its meaning.**

3F. Let us assume that the soliton spectrum can single out rogue waves, and describe its formation through soliton interaction. The way I see it, the authors have overlooked the prediction/warning aspect of rogue wave research, which is intrinsic to all natural hazards. The eventual inability of the soliton spectrum method to become predictive does not diminish the importance of the manuscript, but it has to be stated. **I therefore suggest that the authors should outline how this process could become actually predictive for engineering purposes, in line with the journal scope. It is not at all clear to me how extracting the soliton spectrum of a time series (as times go) can predict rogue waves, that is, this seems to be a trace method. Can we infer the soliton amplitudes before the actual time series is recorded, for instance, from sea state variables? Regardless of the author's answers to these questions, this discussion must be included at the end of the manuscript.**

3G. Lines 217-223 on page 13 and all discussion following the soliton spectrum are all problematic:

[12] I wonder if we also could write the interaction as $\zeta(x,t) = \zeta_{linear} + \zeta_{second-order} + \zeta_{soliton}$, with ζ being the sea surface elevation, because at $0.05 < h/L < 0.22$ second-order waves may still exist.

"Solitons were found in all samples, with and without rogue waves".

Here the reader may become confused in view of the title. The text lacks a deep description of the theory and the reader will likely be induced to believe having solitons will lead to rogue waves. Reading this statement, it induces a conclusion that solitons don't affect rogue waves. **If you add the main theory/results of Bruhl and Oumeraci (2016) a line before, all will be clear. Solitons are the "fabric" of the irregular wave train and will be there regardless of the tallest waves according to Bruhl and Oumeraci (2016).**

"The aim of the study was to explore the role of the determined solitons for the generation of rogue waves. In the first part of the study, it was investigated whether specific solitons in the NLFT spectrum could be associated with the recorded rogue waves."

Here the authors use a very vague set of words. I suspect they meant "individual solitons". **They must be more precise**.

"For this purpose, all free-surface elevations between the two zero-crossings of a rogue wave (or largest wave, for normal samples) were scaled down to 80 % (Fig. 6)."

This methodology has no physical, mathematical or statistical reasoning in the manuscript. The authors fail to give a reference. It suggests they implement an arbitrary method to check soliton "associability" with each individual rogue wave. In figure 6c the largest soliton "associated" with the rogue wave did not decrease its amplitude to 80%, rather to $\sim$60%. **Unless the authors provide a strong argument with a solid base for this approach, I recommend to completely remove this artificial change that by no means reflect reality (i.e. the observation). Alternatively, having in hands more than 5 million waves from Norderney, the authors could compare different and yet identical time series but with different wave heights of the rogue waves, and check whether they find the associated soliton. As a second alternative, the authors could run a simulation that creates identical time series with distinct heights for rogue waves**[13]. To highlight the arbitrary nature of this procedure, the authors claim on lines 220-223:

"The KdV-NLFT was then repeated for the modified time series, which resulted in a new soliton spectrum. It was monitored which of the solitons had changed in amplitude A (and, therefore, in frequency F), due to the change in wave height. These solitons were assumed to have the same position as the rogue/ maximum wave."

It is not clear why they can assume it is the same position. Although Bruhl and Oumeraci (2016) resonate deterministic reasoning, this above statement shows full arbitrariness. Likewise, the start of section 3.1 discusses how they "associated" solitons with rogue waves through this artificial method:

"The solitons with constant amplitudes can be regarded not to be associated with the rogue wave."

Which again demonstrates its lack of analytical and deterministic approach. It is quite surprising, since formation mechanisms dealing with spectral analysis are supposed to tackle deterministic approaches. A further criticism deals with the fact that we have no idea of the depth kh in the discussed examples. **Unless the authors rewrite the text as "(...) amplitudes ARE associated with the rogue wave because (...)", I do not see the relevance of this approach.** In addition, on line 235:

"Although often the case, the largest soliton attributed to the rogue wave was not necessarily the largest soliton in the spectrum (Fig. 7)."

[13] Removing this method/result does not necessarily impact on the overall results. I deem reasonable that the soliton spectrum effect may actually be translated to the mean normalized soliton amplitude. As a third alternative the authors could compute probability curves for mean normalized soliton amplitudes.

Do the authors have any idea of why that is? Again lacks deterministic understanding. **In that case, I see as the only remedy to resort to the statistical understanding.** Although line 250 asserts that the author's results agree with Osborne et al. (1991)[14], the latter study showed that each soliton was uniquely locked with each wave group, as opposed to the current manuscript. So far, the authors speak of a deterministic process but actually talk of statistical uncertainties, like "may" or "may not" be associated for both ordinary and rogue waves. Hence, that is why I believe the authors have plotted figure 8. Unlike the previous figures, figure 8 does contain significant statistical knowledge. **But given the strong ambiguity in the "association" method, I recommend the authors to change the plots for either mean soliton amplitude vs. maximum height or maximum soliton amplitude vs. maximum height, but without any "association".**

In addition, as summarized at the start of this section, the whole association process between individual solitons and individual rogue waves that cover three pages is simply dismissed as a formation mechanism for rogue wave on lines 254-258:

"The interaction of unidirectional solitons, however, as described by KdV, is known not to result in exceptional increases in wave elevation (Kharif and Pelinovsky, 2003). Hence, the soliton spectrum alone does not yield a satisfactory explanation of the generation mechanism of extreme waves/ crests. One may speculate that the formation of the rogue wave in these cases is a result of the interaction of one or several solitons with the underlying oscillating wave field, a hypothesis which will need further analyses to be validated."

The above statement is quite confusing and adds to the lack of organization of the article. If the authors strongly believe the soliton spectrum to show rogue wave formation, this discussion should be place at the end of the paper. On the other hand, if they believe the soliton spectrum to not be able to understand rogue wave formation, **given all the criticism above, I wonder why spend so many pages with an arbitrary approach that can not explain rogue wave formation. I suggest shrinking the whole soliton spectrum development to less than half a page, and give more attention to the requested revised figure 8 that is statistically significant.**

Lines 259-261 repeat the same contradiction of lines 254-258, this is redundant. But more importantly, authors try to remove the influence of the sea state as follows:

"To remove the influence of the underlying sea state, the soliton amplitudes A_s^1 were normalised by the significant wave height H_s of the corresponding sample."

Except that normalizing the wave height or soliton height does not remove possible nonlinearities due to the underlying sea state. Proof: We may take the probability density of absolute wave heights for a Gaussian sea (Massel, 2017):

$$f(H) = \frac{H}{4m_0} e^{-H^2/8m_0} \quad , \quad m_0 = \int_0^\infty S(\omega)\, d\omega \quad . \tag{1}$$

The underlying "sea state" in this case causes Gaussianity (Rayleigh distribution), however, by normalizing the height into $H^* = H/H_s$ does not change the Gaussianity:

$$f(H)dH = f(H^*)dH^* \quad \therefore \quad f(H^*) = 4H^* e^{-2H^{*\,2}} \quad , \tag{2}$$

and thus not changing the underlying sea state. The sea state is not simply defined by H_s, and the plot of $f(H/H_s)$ may have sea state peculiarities even in deep water (see Karmpadakis et al. (2020)). Indeed, by plotting the exceedance probability of H/H_s the sea state peculiarities were by no means removed in figures 7-9 of Teutsch et al. (2020). **Therefore, I believe the conclusions based on this statement are misleading as to the removal of the effect of sea states. I recommend authors to plot exceedance probabilities of H/H_s for ranges of A_s^1/H_s and varying sea parameters such as steepness, bandwidth, depth kh or Ursell number. I strongly believe the true influence of solitons will abide in these plots.**

[14]Please provide the exact quote from this paper that is assured to be in agreement, as I could not find it.

3H. Yet another statement hard to reconcile with the manuscript main line of thought:

"Brühl (2022)[15] classifies waves with $0.559 \leqslant U$[16] *as solitary-like wave types and waves with* $U < 0.559$ *as Airy-like, Stokes-like or cnoidal-like. For our data, in which the bulk of waves are located below* $U = 0.559$*, this means that most rogue wave crests are not soliton-like."*

While Bruhl and Oumeraci (2016) assured that all periodic oscillatory waves in the range of validity of the KdV equation are composed of solitons interacting with each other, what does this above statement mean? How can these waves be of no soliton-like structure and yet be composed of solitons? **Please reconcile both statements in a clear fashion, if possible.** To add even more confusion, the authors portray no surprise, and claim that it is expected the rogue waves can not be explained by solitons alone on line 294. They also add that maybe solitons have to interact with other wave components, but they have not analyzed it. **If none of the above is reconcilable, I recommend the authors to include the literature discussion that precludes rogue waves from being formed by solitons alone in the first pages of the manuscript, and move on directly to what can be attained. It requires the reader many pages to understand the relevance of a soliton spectrum, only to learn that it alone can not achieve the expected purpose.** Then figure 10 discusses soliton amplitudes versus Ursell number, and authors observe a saturation in the growth of the former until on lines 298-303 admit that there is no explanation for this phenomenon. **Then, I ask: what is the relevance of figure 10? It would be better to plot exceedance probability for varying Ursell and fixed soliton amplitudes.**

3I. After a series of contradictions in 3H, The authors jump to the next section (3.2) seemingly assuring that soliton spectrum can reveal formation of rogue waves, just after the paragraph that said the latter task to be impossible!

"When investigating the attribution of solitons to rogue waves in Sect. 3.1, we found in the majority of cases that the largest soliton in the nonlinear spectrum could be attributed to the rogue wave. In addition, this soliton was often outstanding from the other solitons in the spectrum, with a much larger amplitude than the remaining solitons in the spectrum (see the example in Fig. 6). We were therefore interested in whether the existence of an outstanding soliton in the nonlinear spectrum was typical for rogue wave samples off Norderney. We investigated this question statistically by comparing soliton spectra, calculated from 310 vKdV-NLFT, for normal samples and the four different categories of rogue wave samples".

The comments extracted from lines 294-303 on page 21 of the manuscript clearly dismisses what is written on page 22. **I do not understand why the text insists on soliton spectrum after being dismissed at least three times earlier. If the authors want to discuss the statistical inference from soliton spectrum without damaging the reader's ability to understand and without challenging the line of thought on every page, I recommend that the caveats and issues of the soliton spectrum be either transferred to the introduction or the discussion as a counterpoint.**

3J. On page 23 leading to equation 13 the authors implement yet another arbitrary tool[17] without any solid justification nor references. Nevertheless, even so the results in table 2 demonstrate large percentage of false positives for "normal" samples (which surely include many relevant waves in the range $1.5 < H/H_s < 2$) and for "height" samples. The authors correctly conclude that outstanding solitons (whatever arbitrary notion defined them) can not be a strong predictor[18] for height rogue waves or large ordinary waves near the rogue threshold, and hence, it is a good predictor only for extreme rogue waves. **The problem here is that this statement is not found in the conclusions. Please add this discussion to the Conclusions.** I would expect something of the type:

[15] Attention to this reference that can not be found.

[16] Why don't you write $U \geqslant 0.559$?

[17] I wonder if the variance of the soliton spectrum would not be a better tool.

[18] Here predictor does not mean it has predictive power, as one can not extract the soliton spectrum before the time series materializes.

The literature shows that solitons alone can not form rogue waves, and it is hypothesized that when the former interact with linear waves the latter can be formed. Nevertheless, we carried out a clustering procedure to separate normal and outstanding solitons loosely associated with rogue waves, and found that only for extreme rogue waves the soliton spectrum analysis is a good predictor.

3K. At the start of the conclusions it is stated:

"Rogue wave occurrence recorded off the coast of the island Norderney is not sufficiently explained by second-order theory."

Where in Teutsch et al. (2020) have second-order models in steepness been evaluated? You mean Weibull model, no? **Please be careful with your jargon. You could test an actual second-order model for wave crests, like Forristall (2000).** This text in the conclusions point to the fact that indeed in 1B (see page 1) they were describing Forristall (1978) as a second-order model, a clear mistake.

3L. In the concluding remarks I find:

"Rogue waves at Norderney are likely to be a result of the interaction of solitons with the underlying field of oscillatory waves."

This is a big leap not supported by the manuscript, considering that you did not study this type of interactions (see line 258). **I believe the authors can not discuss likelihood of a process that was not analyzed. I recommend you change "are likely" to "Despite not assessing the interaction of solitons with oscillatory waves, we are confident that rogue waves at Norderney could be a result...".**

Conclusion

The reviewer thanks for the opportunity to read this important work. Overall, I support the publication of this preprint once all these issues have been clarified and/or amended/removed.

References

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