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Review and rebuttal of the paper

Validation of nonlinear wave decomposition method including shoaling for irregular waves

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Editor handling the paper: Miguel Esteban

1 Response to reviewer comments round 1:

The authors wish to thank the reviewers for their constructive criticism and suggestions. The authors are confident that the efforts of the reviewers improved the quality of the manuscript. The replies from the authors to the reviewers have been indicated in green text. The changes in the manuscript are visible in the document with track changes.

1.1 Reviewer A:

Overview:

The manuscript presents a new methodology for wave decomposition incorporating the effects of shoaling. The manuscript is an important extension of already existing methodologies and provides a clear and concise portrait of the new approach. I also appreciated the clear indication of the limitations of the approach. In general, the manuscript is well written with only a few minor errors (mostly related to the reference style).

Please review the comments (grouped into Major and Minor).

Major:

1) Figure 3: I may have missed it, but what are the target values here?

The caption of Figure 3 is adjusted to clarify the definitions in the figure.

2) Figure 4: I am not sure if the text matches with the figure? The mean in the Figure seems to show a 4 % error, but the text shows 1 %.

The mean relative error is 1% and the maximum of all the relative errors is 4 %. The result section is rewritten to clarify this difference.

3) Page 8 Line 5: I am not sure this first line represents well what was presented in the results. I think it is an important discussion point, but not representative of what is in the manuscript.

The discussion is rewritten. We kept the part about wave breaking because it is important limitation of the method and therefore important to discuss in our opinion. However, the order of the topics in the discussion is changed to have a better link with the results in the manuscript.

Minor:

- Page 3 Line 2 "… because the bound waves do not shoal linearly."

Sentence rewritten.

- Figure 1: Move (or remove) legend in bottom figure from blocking data.

Legend is removed in the lower panel

- Page 5 Line 8: Space between comma and 2019.

Space is added.

- Page 5 Line 11: Space between 100 and m.

Space added.

- Page 6 Line 17: Error in reference method.

Reference fixed

- Page 7 Line 13: I am not sure “distinguish behavior” is the correct phrase here maybe “marked”

or “pronounced”.

Sentence rewritten.

1.2 Reviewer B

Dear author, thank you for providing the manuscript. Generally, I see the additional value of this work, which extends the nonlinear decomposition methods for a sloping bed. However, there are several issues that require further elaboration before I can suggest the manuscript for publication. Please see my comments below.

General Comment

In general, a clear description of the research gaps and questions that this manuscript addresses is missing. For example, the introduction is too vague, as it does not specifically mention the need for better decomposition methods. After all, there are already methods to separate wave signals into incoming and reflected components—so why exactly do we need these "new variants"?

The research gaps are added and the objective of this paper is added in the introduction. It is not our intention to derive a new decomposition method, but to show the important aspects when accounting for a sloping bed in a nonlinear decomposition method.

Section 1 & 2: The introduction and literature review are very brief and include only a few references, although they are relevant to the manuscript. Specific findings from the references are not mentioned, and an adequate narrative for the manuscript is lacking. Furthermore, there is only a reference to de Ridder et al. (2023), where additional information can be found. The literature review needs to be thoroughly revised.

The introduction is revised with a more thorough literature review.

Section 2: The methodology section (2-Theory) is also very qualitative and lacks specificity. The handling of noise, the respective regions of the spectrum, and the decisions on when to use the respective components of the matrix are described too vaguely.

The method section is revised. Initially, we only wanted to focus on the part of the method related to a sloping bed. We have added a 1) more detailed description of the existing decomposition method (reference method), 2) the handling of noise and 3) a description of the reduction of the system of equations.

Section 2: The calculation and application of the shoaling coefficient for bound waves according to Padilla and Alsina (2020) is overly simplified and not solved iteratively. This should be explained accordingly, and the simplifications should be justified.

The assumptions made with the proposed shoaling coefficients for bound waves are added in the method section. In addition, the reason why this approach with the oversimplified approach is applied is added. In the discussion, we come back to these assumptions.

Section 2, Fig.2: It is unclear which test was used for Fig. 2.

The wave conditions for Fig 2 are added in the caption and in the text.

Section 2: It is unclear how the theoretical signals of the JONSWAP spectrum were generated and why these boundary conditions were selected.

The generation of the synthetic time series is added and an explanation of why these wave conditions are chosen is described.

Section 3.2: A sketch would be helpful for the numerical description.

A figure with an overview of the numerical domain is added.

Section 3.2: How was the numerical model calibrated and validated?

This study only focuses on applying the model, but a validation is given in de Ridder et al., 2019 for several test cases. This description is added to the manuscript.

Section 3: It is already known from other publications that shoaling coefficients must be determined differently for individual bound waves, and this could have been implemented here. This should at least be included in the discussion.

The simplified shoaling model for bound waves is discussed in the discussion.

Table 2 – a description of the parameters is missing. Were all tests used for validation, and could the respective breaker parameters also be added here?

The description of the parameters is added in the caption. Also, the measured breaker parameter is added.

Section 4.2: I do not see this as a concrete justification for why only the first and last wave gauges were used. Can this be described more clearly?

The reason why the first and last wave gauge were selected is to show the effect of the method for both wave gauges. We could have shown the results for all wave gauges, but the differences are most pronounced for the first and last wave gauges because the mean water depth is used for the reference method. In addition in practical application, one is most likely interested in the last wave gauge, but that is the closest to the structure. This explanation is added to the manuscript.

Section 4: There is generally almost no information provided on the measurement techniques, setup, etc. However, these should be part of the manuscript to assess the significance of the experiments. Please add this information.

The method section is extended. The model setup is described in more detail.

Section 4, Fig. 4: As I see it, only the result of the model including shoaling is shown, and not compared to the model without shoaling as stated in the text. Please add this comparison.

The figure shows the difference between both methods (reference method and method with slope effects). The description of Fig 4 is changed.

Section 4: Additional figures are necessary to evaluate the physical experiments. Please add meaningful figures for this (e.g., energy density spectrum, time series, etc.).

Both a time series and a spectrum is shown for one of the test conditions. In addition, we added the spectrum of the error for the first and last wave gauge to show the effect of the decomposition on the error term.

Section 5 / Discussion

What is meant by "applicability range"? Please introduce this term previously and discuss only the results and findings, which are also part of the numerical and physical results of your manuscript.

The discussion is written to describe only the current results. The part about the applicability range is changed to clarify this definition.

The first paragraph of the discussion is not very convincing or specific; please revise and clarify.

The discussion is rewritten.

The discussion should fundamentally place the work in the broader context of comparable studies and the original objectives. How can the work be usefully further applied? This is missing from my perspective or could be more clearly elaborated.

Examples of applications of this method are added to the discussion.

In the last paragraph, two discussion points are mixed. First, there is the question of why analytical methods are sometimes needed in comparison to numerical methods, and what benefits are gained from them. This is not clearly explained and discussed. The second point refers to the phase transformation of bound waves, which were not adjusted as in Padilla and Alsina (2020). This is also inadequately explained, as it is unclear why this was not used and which aspects are still insufficiently known. Please discuss this further.

The paragraphs is rewritten to better distinguish between the two points.

Section 6 / Conclusions

Please refer to the original objectives here and explain to what extent they have been met. The deviations are described too vaguely, and the reasons for the larger deviation need to be explained and justified in the results section.

The conclusions are rewritten to make the conclusion more to the point. An explanation for the deviations is added in the results section.

2 Response to reviewer comments round 2

2.1 Reviewer A

The author's have sufficiently responded to all of my comments.

Recommendation: Accept Submission

2.2 Reviewer B

The authors wish to thank the reviewer for the review. The replies from the authors to the reviewers have been indicated in green text. The changes in the manuscript are visible in the document with track changes.

First of all, thank you for your work on the manuscript and incorporating my suggestions and your reply. The manuscript is overall improved and I have only some minor remarks and comments.

General comments

Please review the references since they are given in a variety of formats.

References are fixed.

Figures: It would be helpful if you add identifiers for each of the subplots you are generating. E.g. you could add (a) – (d) to Figure 2 and incorporate them into your figure captions and text to be more consistent and help the reader to follow you.

Identifiers are added to all the figures with multiple panels.

Specific comments

p.2, Line 9: *Medina (2001) used a different approach for regular* ***waves*** *[…]*

Fixed

p.2, Line 22: *In addition, Lykke Andersen and Eldrup (2021) presented a method for nonlinear* **waves** […]

Fixed

p.4, Line 7-11: Please reread the sentences and improve the readability and correctness.

The sentence is rewritten

p.4, Line 34-35: […] *but the method will add energy*. Please rephrase.

Fixed

p.5, Line 20: Reference in upper- or lower case? Please decide and check the manuscript.

Fixed

p.8, Line 5: Figure 43 -> Figure 4?

Fixed

p.8, Line 5ff.: Please be aware that you are showing the relative significant wave height *Hm0*, which is indeed closely correlated with the energy content of the wave but it is not the same. Therefore, please adapt your wording.

The sentence is rewritten

p.8, Line 12: Table 11 -> Table 1 – please check all cross-references

Fixed, all references are checked

p.9, Figure 4: Please change the legend entry “target” as it is also used on the y-axis and the reader might think, that they should be the same.

The text in the ylabel is changed to $H_{m0,target,last}$ to distinguish between the target and the target value for the last wave gauge.