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Review and rebuttal of the paper

Validation of an efficient two-layer non-hydrostatic wave model on a sloping foreshore

Den Bieman et al.

Editor handling the paper: Miguel Esteban

The authors wish to thank the reviewers for their time and effort in the review of the manuscript. The authors are confident that the efforts of the reviewers improved the quality of the manuscript. Note that both the replies from the authors to the reviewers and the changes to the manuscript have been indicated in blue text, both in this document and in the manuscript itself.

Review round 1 – 2024-08-28

Reviewer A

The submitted manuscript shows that XBeach-NH+ is an effective method for the preliminary calculations of the production of experimental tank slopes. The two main contributions of this study are as follows. First, a sensitivity analysis revealed valid parameter settings for XBeach-NH+. Second, the authors confirmed that the method is more accurate than existing methods based on Battjes and Groenendijk (2000). This research topic is valuable for physical modelling research and is suitable for publication in JCHS. However, some revisions are required before the manuscript can be accepted for publication because it is insufficient for publication because of the lack of a detailed description of the experiments and numerical calculations, the fact that the calculation results are only shown after sorting, and the lack of a literature review. The authors are kindly asked to revise the manuscript based on the comments given below.

Major comments:

A01. - Line 19: The authors compared the OpenFOAM, SWAN, and Battjes and Groenendijk (2000) equations, but can't the authors use SWASH?

Reply: Text adjusted. SWASH is indeed quite comparable to XBeach-NH+ and could be used in similar vain, which is now mentioned in the introduction.

A02. - In Table 2, Is it unnecessary to check if maxbrsteep or reformsteep is reduced below the recommended value?

Reply: Suggestion adopted, text, tables, and figures changed. New calculations have been added for a maxbrsteep of 0.35, since the maxbrsteep is clearly the the variable that affects the reproduction of the measured data. Compared to a maxbrsteep value of 0.40, this new value of 0.5 performs slightly better for Hm0, but significantly worse for H10% and H2%. Hence, the recommendation is still to use a value of 0.40, only now supported by stronger arguments. Related to comment B29.

A03. - In Table 2, Why do the authors suddenly show the MAE of the calculation results? Because the reviewer does not know the raw values of each wave height, the reviewer cannot evaluate whether the numerical model is accurate.

Reply: Text clarified, table with bias added (also see reply to comment B30). Given the large number of results from 18 tests with 15 wave gauges on the slope (=270), we feel some kind of aggregation of results is necessary to keep it comprehensible for the reader. To show a bit more information, a table with the bias has been added. Now the tables show aggregate error metrics. Fig 4 shows the relative errors of all wave gauges in all tests, and Fig 3 shows gives insight into the spatial aspect for one of the tests. These different presentations together hopefully give the reader a good insight into the model performance.

A04. - Line 110: It is better to discuss the causes of overestimation and underestimation with their raw values and show a figure.

Reply: Text clarified and expanded, reference to Fig 3 made more explicit and table with bias now also present. Also see reply to comment A03.

A05. - Line 120: The reviewer wonders whether the validity of the parameters examined in the sensitivity analysis depends on experimental conditions. For example, can they be applied under conditions where the slope is 1:10? Please supplement the theoretical basis or the limit of applicability.

Reply: From our results and experience, the main hurdle in the reproduction of physical model results in XBeach-NH+ is whether the parameterized wave breaking in the model is good enough in the situation at hand. We see it performing gradually worse in (very) shallow water, as shown in the manuscript. In principle a change in slope – if not too extreme, very steep slopes make that different physical processes might become dominant – should be handled reasonably well by the model.

A06. - Line 123: Please carefully explain the process of calculating the MAE by citing the equation in Battjes and Groenendijk (2000).

Reply: Explanation added to text. The empirical wave height distribution by Battjes and Groenendijk (2000) is used to predict the H10% and H2% values for WHM04-WHM18 for all tests (using the measured Hm0 as input). An MAE is calculated for the Battjes & Groenendijk prediction w.r.t. the measured H10% and H2% values. This MAE is then compared to the MAE of the XBeach-NH+ model.

A07. - Line 123: There may be other equations for determining the wave height distributions on the slopes. Please add more references throughout.

Reply: Text added. Although Battjes & Groenendijk (2000) is commonly used, there are indeed different distributions known in literature, some recent examples of which (Wu et al., 2016; Karmpadakis et al., 2022) have been added to the manuscript.

A08. - Line 127: Is there any known cause for the larger MAE in the Batjess and Groenendijk (2000) equation than in the numerical model?

Reply: Explanation added. One of the contributing factors might be the shape of the BG2000 distribution, with the sudden change in gradient (discontinuity) at the transition wave height, where in physical models we measure a gentler continuous change towards smaller gradients (as can be seen in Fig 2).

A09. - In Figure 4, The experimental results for all cases of all wave gauges were included, making it impossible to distinguish the influence of wave steepness from the influence of the location of the wave gauges.

Reply: In Fig 4, the errors are displayed against the relative water depth (d/Hm0,In,deep) at the wave gauge. In that sense, the relative water depth does give some information w.r.t. the location of the wave gauge, with larger relative water depths representing wave gauges that are closer to the wave board and vice versa. I currently do not see a way to present this information (and the trends visible from Fig 4) in a better way without either making the figure very hard to interpret or to include a lot more different figures, which also is not desirable.

Minor comments:

A10. - Line 24: When citing SWAN models, the JGR literature is generally cited. In addition, please cite any examples of SWAN use for the purposes like this study.

Booji, N., Ris, R.C. and Holthujisen, L.H. (1999). A third-generation wave model for coastal regions: 1. Model description and validation. Journal of Geophysical Research, 104(C4), 7649-7666. ISSN 0148-0227. DOI:https://doi.org/10.1029/98JC02622.

Reply: Suggestion adopted and reference changed to the more often used one. There are – to my knowledge – no examples of the use of SWAN to calculate wave height distributions, since that is not possible with a spectral wave model.

A11. - Line 34: XBnh2l is not a common abbreviation; it is better to abbreviate it as XBeach-NH+, following the XBeach manual and van Ormondt et al. (2021), or to include the parameter keyword nonhq3d, as in Oliveira and Oliveira (2023).

van Ormondt, M., Roelvink, D. and van Dongeren, A. (2021). A Model-Derived Empirical Formulation for Wave Run-Up on Naturally Sloping Beaches. Journal of Marine Science and Engineering, 9(11), 1185. ISSN 2077-1312. DOI:https://doi.org/10.3390/jmse9111185.

Oliveira, F.S.B.F. and Oliveira, J.N.C. (2023). Topo-bathymetric behaviour of a beach controlled by a groyne field and a dune-seawall backshore. Journal of Coastal Conservation, 27, 10. ISSN 1400-0350. DOI:https://doi.org/10.1007/s11852-023-00938-y.

Reply: Abbreviation changed to XBeach-NH+ in the manuscript, to be consistent with existing literature.

A12. - In Figure 1, it is not common to describe the experimental setup in the graph; slope, wave gauge information, water levels, etc., as described in Lines 43-45 should also be included in the figure.

Reply: Suggestion adopted, figure modified. The figure now contains the relevant information as described in the text. Additionally, the wave gauge positions are included in a table (also see reply to comment A13).

A13. - Line 44: Please explain the details of the installation location and the reason for its setup, along with the name of the wave gauge manufacturer.

Reply: Suggestion adopted. Table added with the locations of all wave gauges (which are kept constant for all tests). The wave gauges have been manufactured in-house.

A14. - Subsection 3.1: Please include "two-layer" in the title of this subsection.

Reply: Suggestion adopted in text, title changed.

A15. - Subsection 3.2: Please include the XBeach revision number the authors used and add a list of parameter settings that the authors changed from default.

Reply: Suggestion adopted, XBeach version added and the non-default parameters have been added to the text.

A16. - Line 79: The reviewer confirmed that the default value for maxbrsteep was 0.4. The default value for reformsteep seems to be 0.25 times maxbrsteep, but is the 0.25 in the statement a setting value or a multiplier for maxbrsteep?

Reply: Text clarified in manuscript. Both parameters are defined as a fraction of the wave celerity. Included a reference to Section 3.1 where this is explained.

A17. - In Figure 3, The physical plot is too large. Is this plot for a case in which the effect of the generating boundary is not shown?

Reply: Figure adjusted. Slightly reduced the size so that it is now nicely in line with text. A slight effect of the boundary can be seen on the right side of the upper left panel.

Recommendation: Revisions Required

Reviewer B

B01. - The paper is rather short. However, it treats a relevant topic, which I did not encounter much literature yet. In physical modelling most often the replicated seabed bathymetry is truncated at a somewhat arbitrary depth/distance from the structure, while the validity of this choice is unclear. Moreover, making the foreshore too large can lead to very large and unnecessary costs. So validating a tool with which this can be determined is very useful to modelling practitioners. Hereby the paper is very relevant for the journal. It is written well. Some small ambiguities can be improved. Although on the specific topic I can believe not much specific literature exists, the paper should be embedded more in literature. I give some suggestions of other literature below, but even more background would be welcome. Moreover, the literature (8 Delft vs 1 rest of the world) should be more diverse.

Reply: Suggestion adopted and more relevant (diverse) literature addressed in the manuscript.

B02. - Unfortunately the physical modelling application that is given in the first sentence of abstract and main text, determining whether a truncated foreshore bathymetry resembles the full bathymetry (or even a method for optimizing the truncated bathymetry), is not discussed any further in the remainder of the paper. It should discussed how the validated model could be used to this end. From the presented calculations and tests, combined with some reasonable assumptions, some good pointers can be extracted. Like: At which location/depth(s) should the target conditions be compared between full and truncated bathymetries (probably not too shallow). What parameter(s) should be compared, and what accuracy would indicate a suitable truncated foreshore? Some sentences should be spent on this in the discussion.

Reply: Suggestion adopted. The discussion has been expanded to give a bit more practical guidance to the potential use of XBeach-NH+ (or numerical models in general) in the design of foreshores.

Next, more detailed remarks (ranging from typos to more fundamental suggestions) are given in chronological order:

B03. - Abstract. The abstract can be somewhat more clear for a new reader. Indicate why especially wave height distribution is important to get correct for a physical model. Some steepnesses are discussed, but maybe first introduce that wave fields with 1, 2.5 and 5% steepness were tested. It is somewhat confusing to mention first a 5% steepness, followed by an argumentation using kph values. Please note where the mentioned steepness applies (offshore), and its definition, s_op.

Reply: Text clarified. We've tried to clarify the abstract as much as possible while adhering to the 300 word limit stated by the journal.

B04. - Introduction. More background references should be discussed. The IAHR manual on physical modelling for instance gives some guidance on the required length of a foreshore. Discuss the (lack of) research that this is based on.

Reply: Suggestion adopted. In the introduction some discussion on existing guidelines and heuristics on foreshore and transition slope design.

B05. - It would also be nice in a JCHS paper to refer to another JCHS paper that is relevant to this case, as it also treats breaking the breaker type of (tsunami) waves, using the SWASH model which is very similar to XBeach. It shows that the general breaking behaviour is modelled well: Roubos et al (2021). Formulation of a Surf-Similarity Parameter to Predict Tsunami Characteristics at the Coast. Journal of Coastal and Hydraulic Structures, 1. https://doi.org/10.48438/jchs.2021.0009

Reply: Suggestion adopted and reference included in the introduction.

B06. - l12 Consider using H0.1% instead of Hmax, as it Is a better defined quantity.

Reply: Certainly an argument can be made for using H0.1%, as it is a more specific quantity than Hmax. In our experience, however, Hmax is a more commonly used variable and thus recognizable variable for the reader. Furthermore, since the decision for which parameters to recommend is not based on Hmax (or would be based on H0.1% for that matter), we think it is clearer for the reader to stick to the a more familiar variable in the Hmax.

B07. - l14 Consider adding: "… as here the response is governed more by extreme waves instead of a mean value."

Reply: Text clarified in manuscript, suggestion adopted.

B08. - l14 Add some references where the importance of the extreme wave heights to overtopping and forces is shown.

Reply: Suggestion adopted and reference added.

B09. - l20 Add reference other examples of CFD models, e.g. Higuera et al 2013 in Coastal Engineering (was also significantly earlier than the now mentioned studies).

Reply: Suggested reference added to manuscript.

B10. - l22 Add '…for one-dimensional layouts' and maybe indicate that for a two-dimensional case the computational effort becomes practically impossible to achieve.

Reply: Text clarified. Here, I'm assuming one-dimensional layouts refers to 2DV (two-dimensional with one horizontal and the vertical dimension) calculations of a transect; and two-dimensional refers to both horizontal dimensions being relevant so a full 3D calculation.

B11. - l30 Do note that Boussinesq models could also be useful, but that this model is chosen because of … <add reason>.

Reply: Text expanded. Included the mention of Boussinesq models as a (in our experience generally more computationally expensive) alternative numerical model type.

B12. - fig1 Option: add a dotted line indicating a possible prototype bathymetry that is truncated by the transition slope in the physical model. -> can aid the explanation of the foreshore truncation approach.

Reply: Fig 1 has been clarified (also see reply to comment A12) by added more relevant information w.r.t. the setup of the physical model. I do get the idea behind added the dotted line, but I would interpret it as a variation on the layout (a second setup that was tested). To prevent that confusion, the dotted line was omitted.

B13. - l47 s_op where?, "…at the offshore location"?

Reply: Text clarified. It is indeed the steepness on the offshore wave gauge array.

B14. - l48 Hm0 in the fraction is the offshore value? then eg call it Hm0o.

Reply: Text clarified, changed to Hm0,In,deep to indicate the incoming Hm0 on the deep water wave gauge array. Additionally, for the sake of consistency with other literature, the symbol for water depth has been changed from h to d in the whole manuscript.

B15. - l49 Add something like "…from the offshore three-gauge array…" after determined.

Reply: Text clarified.

B16. - l52 Remove most sentences about the data access. Open access data is the standard these days, so will be self evident when the paper is read in the future. It should described in the data access statement at the end of the paper (see the latest template on the website). Here a short reference to the data access statement will suffice.

Reply: Text removed and data access statement added.

B17. - table 1 caption: Indicate that values are given for the offshore location.

Reply: Text in caption clarified.

B18. - l73 Explain what the nonh1d boundary condition entails

Reply: Text clarified, explanation of the nonh1d boundary condition added. See also reply to comment B27.

B19. - l73 Omit WHM01, as it is not used anywhere else (or better: indicate the WHM names in fig1).

Reply: Mention of WHM01 removed to prevent confusion.

B20. - l73 Why force the waves at the gauge location?, and not somewhat offshore? Now the offshore wave height cannot be checked in the numerical model. -> ah, I later see that this was now done with gauge three.

Reply: Exactly, wave gauge three is used indeed.

B21. - l79 Give references to the literature where these values are obtained from.

Reply: Text clarified. These values are from Roelvink et al. 2018 and de Ridder et al. 2021. Moved up the references in the text for clarification.

B22. - fig2 Indicate the Hs value on the exceedance curves to show its value is rather constant.

Reply: Figure modified. The value for Hm0 is now indicated in the wave height exceedance distributions.

B23. - fig2 caption. Where was this measured? Better: indicate whm15 in fig1.

Reply: Figure clarified. In Fig 1 now more essential info is displayed w.r.t. the setup details (also see reply to comment A12). Some of the wave gauge names have been indicated as well (all names would become unreadable), so that it is easy for the reader to see where WHM15 is. Additionally, the location is now also indicated in Table 1.

B24. - fig2 Comment in text on the deviation that can be seen in Hmax in the measured curve (due to normal sampling error).

Reply: Suggestion adopted. Added clarification in text in Section 4.1 to existing remarks on Hmax.

B25. - fig 2 Please give another exceedance curve plot for another relative depth to show the influence.

Reply: Suggestion adopted, figure added. Besides the existing figure for WHM15, one for WHM08 (in shallower water) has been added to show the influence of depth.

B26. - fig3 Why one plot with a red line, and the rest with markers? use one type, eg line with markers. I could not distinguish the line from the circles on my greyscale printout.

Reply: Figure modified. The markers now are not filled anymore, to make the figure better distinguishable in greyscale. The reason for the difference between the line and the markers is the different outputs from the XBeach-NH+ numerical model. While the Hm0 is calculated for the entire computational grid, the H10%, H2% and Hmax can only be calculated at those locations where a (digital) wave gauge is defined. The positions of these digital wave gauges correspond to the positions of the wave gauges in the physical model (see Table

1). Despite the difference, I think it is still insightful to the reader to show the entire spatial range of the Hm0, to clearly show the breaking point and the small dip near the offshore boundary.

B27. - l88-92 This behaviour also occurs at wavemakers, is related to remark on l73.

Reply: Text clarified. Yes it is likely that this is caused by the assumptions behind the generating boundary do not (fully) hold, which causes the behaviour at the boundary. Explanation and analogy with wave board steering added to text.

B28. - l93 Explain why you (can) calibrate on the total wave height at a single gauge (low reflection for this case?).

Reply: Text clarified. The amount of reflection is relatively small for this layout (long slope up to very small water depth), so the error in calibrating on the total wave height is also small.

B29. - table 2. If the optimal value of maxbrsteep is determined at 0.4, why is not a 0.35 value added in the table? why would that not give even better results?

Reply: Suggestion adopted, text, tables, and figures changed. New calculations have been added for a maxbrsteep of 0.35, since the maxbrsteep is clearly the the variable that affects the reproduction of the measured data. Compared to a maxbrsteep value of 0.40, this new value of 0.5 performs slightly better for Hm0, but significantly worse for H10% and H2%. Hence, the recommendation is still to use a value of 0.40, only now supported by stronger arguments. Related to comment A02.

B30. - table 2. Add another table with the bias.

Reply: Suggestion adopted, an extra table with bias has been added.

B31. - l125 BG is also an empirical model. I would simply refer to it as the Battjes Groenendijk model.

Reply: Text clarified. Removed the somewhat misleading term 'analytical model'.

B32. - fig4 It would be nice to also split IG and primary waves in the analysis. IG is often prone to more errors (imperfect absorbing boundary condition), and might be less important or response like damage.

Reply: As XBeach-NH+ has been validated for IG waves already by De Ridder et al. (2021), we did not think it would add much new insight to also add it to the this manuscript as well.

B33. - Discussion: In the physical model the setup on the foreshore will probably cause an erroneous setdown in the offshore swl due to volume conservation. An error that typically is not present in the numerical model (see thesis or paper by VIncent Gruwez). Please discuss if that could have influenced some of the errors. (check setup/setdown).

Reply: Suggestion adopted, discussion expanded to also give insight into this aspect.

B34. - l178 Add "(very) shallow" before gentle.

Reply: Text clarified.

B35. - l183 Add “at the offshore boundary” after “wave steepness”

Reply: Text clarified.

B36. - l183 Add the range of offshore kph values that were achieved in the 5% test

Reply: Range of kph values added to text.

B37. - l183 Shows -> showed (not a general truth for 5% steepness, but single observations in the test, as it is related to kh).

Reply: Suggestion adopted, text changed.

B38. - l187 Use past tense, “performed”, as this is a conclusion only for the conducted tests. The general conclusion on applicability would be quantified in terms of kh value, so for the applicability in terms of kh, the present tense can be used.

Reply: Agreed, suggestion adopted.

B39. - Notation. The notation section should be filled in. In the text the most important symbols should be introduced at first use as well.

Reply: Text added: notations and abbreviations sections added with clarifying tables.

B40. - General: The subscript for offshore location is o for offshore, not 0, eg s0p -> sop.

Reply: As far as the authors are aware, the s0p is the deep water water wave steepness based on the L0 (hence the subscript 0) based on the peak wave period (hence the subscript p). See for instance Jacobsen et al. (2018).

Recommendation: Revisions Required

Review round 2 – 2024-11-13

Reviewer A

The authors have improved their writing since the initial submission. The reviewer satisfied the story of this study (validation of XBeach-NH+ using the experimental setup. The reviewer requests that the authors reconsider their recommendation of maxbrsteep prior to publication.

A01. - In response to the reviewers' inquiries, the authors conducted additional calculations (maxbrsteep=0.35). The authors recommended maxbrsteep=0.40 based on the MAE of Hm0, H10%, H2%, and Hmax. In the introduction, the authors focused on the validation of wave height distributions in comparison with the work of de Ridder et al. (2021). Given this focus, maxbrsteep=0.45 or 0.50, might be more appropriate for wave height distributions. The reviewer requests that the authors provide a more detailed explanation of their recommendation.

Reply: Text clarified, explanation added to manuscript. In practice, it is often important to both represent the bulk parameters of the spectrum (such as Hm0) and the wave height distribution well, so then the compromise between the two of a maxbrsteep of 0.4 seems appropriate. For applications where bulk parameters are less important, but the focus is on the right side of the wave height distribution (small exceedance probabilities), better results may be obtained with maxbrsteep values of 0.45 or 0.50.

Reviewer B

Dear author(s) and editor,

I appreciate the thorough adaptation of the manuscript by the author(s). I would recommend to accept the manuscript with a few minor revisions. I have confidence these modifications can be made without a new review:

B01. - l30. often -> hardly ever (or similar). Maybe computational power has grown more than I realize, but I never saw a full 1000 wave 3D computation in a practical project yet.

Reply: Suggestion adopted.

B02. - l35. brackets not around name with inline reference to Caires and Van Gent (2012)

Reply: Reference corrected to inline reference.

B03. - Under table three it may be clear to mention explicitly if/that the MAE includes the bias, as you can also can take the variance with respect to the real mean.

Reply: Suggestion adopted, text clarified.

B04. - Also the (fictitious) offshore wave length Lo should have subscript o for 'offshore', so it really is sop, not s0p. But I realize different people/groups have different opinions. The editor of the rock manual was also very particular on these matters, and also used o. Maybe check the Holthuijsen or other respectable waves book for the final choice.

Reply: After checking it appears the Holthuijsen book (Waves in Oceanic and Coastal Waters) uses L0 for the fictitious offshore wave length (Section 7.3.6, page 224, eq. 7.3.41), so we've decided to conform to that. Although indeed we also recognize that throughout literature different choices are made w.r.t. the subscript (either a 0 or an o).