JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Review and rebuttal of the paper

Rock armour slope stability under wave attack in shallow water

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Editor handling the paper: Nils Goseberg

Reviewer #1

This manuscript is a review paper that discusses the quality of stability prediction of rock slopes with permeable and impermeable cores at very shallow water depths. The focus is on the identification of deviations in the stability prediction and the attempt to assess in which physical constraints these deviations can be justified. The authors deal intensively with the quality assessment and processing of data sets published in the literature on this topic. A very high level of expertise and care is applied here. The authors clearly describe constraints between individual wave parameters and provide helpful engineering suggestions to calculate these parameters in a robust way. The authors come to the conclusion that there is no reliable method for describing the stability of rock slopes for very shallow water depths (h/Hm0deep<1.5) based on the currently available data. Furthermore, a table with standard deviations to be taken into account is included for the interim period.

To the authors.

Following editorial and content comments are suggested by the reviewer:

Page	Line	Comment
1	40	“space” is missing after 1.5
2	25	Can the authors provide a threshold for the so-called “small” wave steepness? Is it in the range of 0.02 or lower?
3	11	For clarity, it would help to introduce “P” here, as the reader directly went to the figure to follow the argumentation, and P is introduced 2 sentences later in the actual version: … with increasing permeability (permeability factor P) of the structure.

4	4	Since none of the curves became truly horizontal, the authors should qualify "almost horizontal".
5	20	To avoid an overlapping of the given ranges, the authors can provide h/Hm0deep>5-6 instead of 4-6.
6	6	The reviewer agrees with the authors, that the ratio of wave height is not influenced by the wave steepness in the depth-limited area (h/H_m0,deep<1.5). Based on the provided data by Eldrup, I think it is not correct to state that “the wave steepness does not show any influence” in the region of h/H_m0,deep>3. I suggest to relativise the wording. I agree that there should be no influence of the wave steepness for non-breaking waves and no significant shoaling.
6	22	The chapter discusses data from 1:30 and 1:50 foreshores which can be included in the heading. Or the author could use terms like “very gentle foreshores” (1:100) and “gentle foreshores” (1:30 and 1:50) in the headings 2.2 and 2.3.
6	23ff.	The discussion with respect to the maybe wrong water level in the van Gent spreadsheet is very interesting to read and is a nice example how to analyse data and detect causes of differences in data. However, I would suggest do move the whole section until page 8, line 17 to an appendix or the supplementary data. The author wants to discuss wave breaking over a 1:30 foreshore and not a maybe biased data set.
7	12	The authors could rename the heading in the column “Difference” to “Difference (P=0.1 to P=0.5)”. It took the reviewer a moment to bring the explanation in line 13ff. in context with the data.
8	37	This comment leads to Figure 6. The authors discuss the Eldrup data “green triangles”. The dark green color however is not easy to differ from the dark black circs and diamonds. The reviewer suggests to choose a more contrasting color (blue?) to highlight this specific data points.
11	23	The reviewer agrees with the authors, that data for a 1:30 or a 1:35 slope should not differ too much. To be scientifically accurate, however, the legend in Figure 8 should state m=35 for Hofland (2017).
12	34	As the authors use ≤ and ≥ it is not clear which equation to use in case of $\xi_{m-1,0} = \xi_{m-1,0c}$.
13	20	It may help the reader to quantify the term “very small wave steepness” with s<0.01.
14	33	The expression "… more a nice summer condition at a beach were the temperature is above 25° C." is illustrative but quite colloquial. Maybe the authors can reformulate.
15	24	A reference to Table 2 is given first on page 16, line 10. To shorten the manuscript the authors can think about providing this table in an appendix or in the supplementary material.
16	39	Suggestion to clarify “breaking” with “wave breaking.
18	32	The figure caption or the figure legend should include information, that the sloid black line gives Eq. 1-3. This should be taken into account for the figures 12 to 17.
22	28	Do the authors really see a “reasonable fit of the data” in the left graph of Figure 18? It should be clearly stated for $\xi_{m-1,0}$<3.5, although the “plunging side” is mentioned.

Reviewer #2

Review of Rock Armour Slope Stability under Wave Attack in Shallow Water

1.1 General comments & appraisal

The paper presents a reanalysis of the stability of rock slopes with shallow foreshores. It is a review and reanalysis of several existing datasets. The paper is written in good English, and reasonably well structured (somewhat too long). The first part describes the change of waves and wave parameters over a (linear) shallow to very shallow foreshores, and is a nice addition to the analysis of the damage numbers. Next, the damage of rock slopes is treated. Novel items are a further update of the 'rewritten VdM equation', to include Hm0 instead of Hs, and an extension of the range to shallow foreshores. It is nice to see the Van Gent et al. (2003) data plotted in the classic plots with ksi vs Ns $(S/\mathrm{sqrt}(N))^{0.2}$. It seems to be a useful aim to see to which end the range of applicability of a much used formula (without changing the coefficients) can be extended. For very shallow foreshores the formula is invalidated (not new) and some constant stability number values for the tested cases are given.

The manuscript seems to aim to validate that a certain formula can be used. This is methodologically problematic, as any line can be said to describe the data 'with some scatter' – if 'some' is not quantified. The goodness of fit of the 'rewritten VdM equation' for shallow foreshores should be carefully quantified, and related to an acceptable value.

The long presentation of the data via many graphs in sections 3.5-3.7 is repetitive long and therefore somewhat unclear. Focussing on the figures with the preferred Hm0, omitting the ones based on H1/3, would already halve the number of graphs. The analysis is very qualitative, with many statements that a scatter is 'large', or data 'follows a trend' without much quantification.

I would advise a major revision for the paper. The lengthy, descriptive and qualitative analysis should be shortened (the paper could be some 4 pages shorter). Moreover, the analysis should be better quantified for the shallow foreshore range to support the conclusions of the quality of the fit.

1.2 Some general remarks on the paper and formatting are:

- I would encourage the authors to place the complete data-set as combined and reanalyzed on a non-commercial, open access repository like Zenodo.
- Complete sources for all data, e.g. Thompson & Shuttler (1975), should be given.
- Many figures are not easily read in black-and-white printed paper. Would be good to choose markers/line styles/colours such that can be distinguished in black-and-white.
- Font in figures should be larger.

1.3 Detailed comments (in the order as read, so content and text combined).

p1. In abstract no references should be given.

p1.l18. 'breaker parameter' is unclear as it is also used for Hs/h. Using Irribaren number or surf-similarity parameter, or simply adding "…, ksi, …" is more clear.

p2. Some general comments on the use of the fitting techniques and AI-type approaches should be made, as many papers on those techniques and rock stability have recently been published, eg:

Gandomi, M et al. 2020 Permeable Breakwaters Performance Modeling: A Comparative Study of Machine Learning Techniques. REMOTE SENSING 12 (11)

Lee, JS and Suh, KD 2020 Development of Stability Formulas for Rock Armor and Tetrapods Using Multigene Genetic Programming. JOURNAL OF WATERWAY PORT COASTAL AND OCEAN ENGINEERING 146 (1)

Wei, XL et al. 2019 Stability Assessment of Rubble Mound Breakwaters Using Extreme Learning Machine Models. JOURNAL OF MARINE SCIENCE AND ENGINEERING 7 (9)

Erdik, T and Pektas, AO 2019 Rock slope damage level prediction by using multivariate adaptive regression splines (MARS) NEURAL COMPUTING & APPLICATIONS 31 (7)

Gedik, N 2018 Least Squares Support Vector Mechanics to Predict the Stability Number of Rubble-Mound Breakwaters. WATER 10 (10)

Lee, A et al. 2016 An easy way to use artificial neural network model for calculating stability number of rock armors. OCEAN ENGINEERING 127

It is maybe good to mention shortly at the end of the paper why simple compact design relations are still useful.

p3. l3-5. "… the work of Stewart […] showed that there was no gradually developing damage curve …". Please add for what type of placement, now it reads as it this holds for all type of rock placements.

p3. Some recent work on rock stability is not mentioned/treated. Especially the first one should be discussed:

- Etemad-Shahidi, A. et al. 2020 On the stability of rock armored rubble mound structures. COASTAL ENGINEERING 158
- Losada, MA 2021 Method to assess the interplay of slope, relative water depth, wave steepness, and sea state persistence in the progression of damage to the rock layer over impermeable dikes. OCEAN ENGINEERING 239
- H.J. Verhagen, M. Mertens (2009) Riprap stability for deep water, shallow water and steep foreshores Proceedings of Coasts, Marine Structures and Breakwaters Conference, Edinburgh (2009).

p3.l19. The symbols in the caption are used for the first time. Should be introduced (or reference made to the symbol list).

p3.l124-27 These remarks do not seem relevant for the paper. Please omit. The reference to the data can simply be added like a normal reference in the reference list.

p4.l18 Remove “are interesting”, Other option is to explain why the data are interesting, but that would take too much space.

p5.l12 “straight” or “linear” in front of foreshore.

p5.l15 Omit “Figures 3 and 5 … . Furthermore, “ The text can be understood without the reference to figures further on in the manuscript, and hinders a smooth read.

p5.l22 h/H_{m0deep} <1.5-2. In many instances further on in the papers both h/ H_{m0deep} <1.5 (eg p5.l9) and h/ H_{m0deep} <2 (eg p5.l25) and h/ H_{m0deep} < 1.5-2 (p9.l27) are used. Please choose one value here, possibly written as: $h/\mathrm{Hm0deep} \lesssim 1.5$, and use consistently.

p5.l27 Figure 1 -> Figure 2?

p5.l28 Maybe add the limit $h/ H_{m0,deep}$ to the graphs?

p6.l1 “1:100 foreshore” (change word order)

p6.l10 Omit “very rough”. A rule of thumb generally ís very rough, but especially this one is rather exact (see figure 3).

p6.l16 Omit “continuous wave breaking and” (One cannot see this from the graph).

p6.l23 – p8.l17 This is a nice improvement of the dataset, but it is (when corrected) a small issue and it distracts from the main line of the story. I propose to move it to an appendix, and simply state in one or two lines in the main text that some data has been corrected, with reference to that appendix.

The whole analysis seems to confirm that the wave heights were correct and the water depths were wrong. As the water depth is not included in the stability formula it also does not seem to influence the results, or does it influence the categorization of the data with respect to the relative depth?

p8.l24 Remove ”the rule of … being about”.

p8.27 Interesting.

p9.l11-12 Making no choice between Hm0 and H1/3 makes the paper needlessly long, and I did not discern any new insight in presenting all the plots in a double fashion. The reason given at the end for choosing Hm0 is the same as the reason known at this point in the story, or at most the right graph in figure 12.

p10, figure 8. Plot formula of Battjes and Groenendijk with line without markers.

p10. With the description of H2% on this page I was already wondering how N was determined for H2% (deep water N). This should be shortly stated. It is now mentioned only later in the paper.

p11.l28-l34 Use cot alpha instead of m (and remove from symbol list).

p12.l14-32. Omit. Explain symbols when first used in the paper (most have already been used at this stage). And please update the symbol list that has double and missing entries.

p12.l12 Add ksi<ksic and ksi>ksic behind the two formulae. Consider replacing Hs with H1/3.

p12.l34-35. Please omit: ”For ksi … should be used.”

p12.l35-36 I would omit this sentence, and simply alter formulae (3) to have two options base on cot alpha </> 3 (similar to (1) and (2)).

p14.l5 omit “Sd > 1.5 x “. Unclear what x means. I infer it means something loke a logical ‘and’. I cannot imagine an underlayer to be visible at Sd = 1.5, so seems redundant.

p14.l6-7 Omit “The criterion … for 1:4.” Not relevant here.

p14.l30-43 Omit here. State here shortly that low steepness (<1%) are not seen as relevant and omitted from the analysis. A longer discussion should be given ion a discussion section at the end of the paper. A better substantiation (with references) of the exact value would be better. Now a single location is referred in a colloquial way. The exact value of 1% (why not 1.5%?) should be better corroborated.

p14.l15 Mention if this choice was new to the present paper, or also chosen by previous studies like Van Gent at al, Eldrup&Lykke Anderson, Etemad-Shahidi (I assume the latter).

p14.l25 ‘Steepnesses’ is plural form of steepness.

p14.l30-43 This part is interesting, but should be changed. Firstly, it is a discussion that should be given after the analysis, where at this location simply the limit of 1% steepness can shortly be introduced, in order not to disturb the flow of the text.

Secondly the language is rather colloquial, it bases the number of 1% on one practical case, without reference to that location, any verifiable dataset, or any other literature where this claim is being made. This should be corroborated better.

p15.l23 Add brackets around S_d/sqrt(N), or it is unlear what the exponent pertains to.

p16.l1-9 Difficult to follow. Too lengthy.

p18.l11 Depth induced breaking start around h/Hm0=3, not 1.5-3 (it does increase in severity when h/Hm0 decreases, but starts around 3)

p17, figure 11 Indicate in legend or caption what the arrows indicate.

p17, section 3.5.3. Omit this section here and move to a discussion section at the end, or remove altogether. The part on the measurement accuracy is unclear and incomplete (also imaging techniques are used to measure damage, and IG waves lead to less waves at toe and more scatter). Simply stating that the very shallow data are expected to have somewhat more scatter suffices at this location.

p18.l22-24 No need to list all data in text if legend in figure is correct (this also pertains to description of several following figures). So please omit, as it hinders a smooth read of the text.

P21.l14 Apparently much of the data by Van Gent (2003) has not been used by Eldrup et al (2019) as the quality of the data was not trusted. So why can it be used now? It seems that this must be discussed in the discussion section, maybe also in relation to the larger scatter if these data.

p23.l2-3 Omit '..although in a different … and 19.'

p24.l5 Remove 'important'.

p24.l8 The phrase 'that were kept in range' is unclear.

p24.l14 '…to that the wave […] below…' -> '... to a local wave height larger than 70% of the offshore wave height..'

p24.l18 'In practise there appears to be …' -> This is an unclear statement. It was not described in the paper what in actual consulting/design practise is used. Please state what the conclusion as obtained from the data is, like: 'For practical use, it seems reasonable to use …'.

p24.l20 '…eqs. (7), (8), or (11)…' (no capital , brackets, comma). All equation numbers in the text should have brackets.

p24.l25 Please indicate what reliable means, for instance by indicating the accepted rms value of deviations of predictions from measurements.

P25 Significant wave height is introduced twice in the symbol list. Introducing slope angle as cot(alpha) is wrong (is alpha), and m as its cotangent is redundant.