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Review and rebuttal of the paper

Re-analysis of the deformation of rock-based scour protections around monopiles exposed to waves and current

Nielsen

Editor handling the paper: Miguel Esteban

The reviewers remain anonymous.

1 Reviewer's comments:

First of all, I would thank the author for the changes made to the paper that have significantly increased its quality and practical application. I very much appreciate the inclusion of a new prediction model and the in-depth comparison with other prediction models.

I think we can agree to disagree on the choice of primary calculation parameters (i.e., Shields vs Mobility, KC vs Ucw). From my perspective, there is more than sufficient evidence that the KC-number or total KC-number, in fact, does correlate quite well with scour protection deformation whereas I find limited evidence to suggest the same for Ucw. When shifting through the data (much appreciated that this is made available with the submission!) I think you could easily find a couple of cases with similar Mobility numbers, KC-numbers and different Ucw numbers that show different deformation depths. However, the same is true for the other way around: there are plenty of cases with similar Mobility numbers and Ucw values but with different KC- numbers that support the KC trend. I think this is a testament to everything that is not yet known about scour protection dynamics.

Having said that, the additional analyses and figures definitely add to the paper, although they do make it a bit lengthy and it is sometimes difficult to get the right amount of focus. I find especially the newly introduced Section 4 quite hard to follow.

2 Author's Response

I fully agree that the update of the manuscript with the prediction model have improved it significantly. I will also like to thank the reviewer for the thorough review that pushed me to add this part.

I agree that the KC-number will have an impact under certain conditions: wave dominated flow and relatively high KC-numbers. However, it has not been possible for me to find any trend between KC-number and deformation in the present dataset, just like in the case of initiation of motion of rocks around monopiles as reported by Nielsen and Petersen (2019).

However, I must admit that data for waves (ucw<0.2) and current (ucw>0.8) dominated conditions are very limited. I do agree that this limited data result in uncertainties regarding the most wave and current dominated conditions. Uncertainties that cannot be eliminated or reduced until additional data in the relevant ranges of ucw become available. Nevertheless, it is noted that the design formular by Broekema et al. (2024) (eq. 22 in the manuscript) predicts very small deformations for current dominated conditions when compared to the results for steady current presented in the manuscript and also the threshold for initiation of the horseshoe vortex in steady current as presented by Nielsen and Petersen (2019).

I agree with the reviewer that a full overview of the effect of KC and – for that matter – ucw cannot be given on basis of the present data. So, until additional data become available that can clarify this issue, I agree to disagree. That said, I then hope that this manuscript and already published articles will inspire to further research in the community on this topic.

I agree that the manuscript has become long. I have tried to reduce it, but I must admit, with little success. I also agree that Section 4 was hard to follow and I have added explanations to

the examples and changed the generic examples to specific numerical examples supported by figures. I find it easier to follow and hope the reviewer does as well. Finally, I have added a "reader's guide" to the introduction which I hope will give the reader a better overview of the article from the beginning.

My comments for this revised version of the paper are as follows:

Comments 1) A final remark on the discussion on KCtot vs Ucw: I do agree with the Author's Response 5), where it is remarked that there is similar input which does not equal similar output. I think this all boils down to which parameter we think represents the physics as appropriately as possible. In other words: is the flow field change from approach conditions to near-structure conditions best described by the ratio of the current velocity and wave orbital velocity, or is it best described by the summation of both components? I would argue the latter, since it is not unreasonably imagined that a wave/current velocity ratio can be similar for a combination of conditions with a short-period wave and a long-period wave, whereas the bed shear amplification in case of both is completely different. In the end, physically that is what drives scour protection deformation, and I think it is still premature to assume that in case of combined wave-current conditions there is no effect observed for KC < 5.5. Also for these small KC-numbers, vortical structures may start to develop, which will lead to a different amplification pattern compared to just streaming around the pile.

Response 1: It will probable not come as a big surprise that I disagree in this. The fact that the same KCtot-number can represent radically different flows (see Fig. 20) and, hence, requirements to the scour protection is, in my opinion, a problem. At least until an additional parameter to account for this is introduced.

I have tried many times to find a relation between the deformation and the KC-number, but without finding any relation. This was also the result found by Nielsen and Petersen (2019) when they studied initiation of motion. I will note that I believe that the KC-number will have an influence when the KC-number become large enough (as shown by Umeda (2011, 2013) and Nielsen et al (2023) for waves alone), eventually the deformation should go towards the deformation by steady current when the oscillatory flow become current-like for very high KC-numbers. However, I have not found any data for combined waves and current and KC larger than 6 (except for the previous mentioned articles for waves alone) so it has not been possible to back this by any data, and hence I have not discussed it in the manuscript as it is in essence speculative.

Finally, regarding KCtot and in particular Eq. 24 in Broekema et al. (2024). Although the formular includes the effect of waves in terms of KCtot, it does not consider effects of large KC-numbers. The effect of KCtot is limited to KCtot smaller than around 15, but for this value of KCtot the deformation predicted by the formular is much smaller than the deformation reported for steady current, see e.g. de Lemos et al. (2023), as presented in Fig. 11 in the manuscript.

Even for waves alone the maximum predicted S/Dp is small, around 0.35 (MOBtop=1 and KCtot>15). This should be compared to a deformation of S/Dp=0.5 for waves alone (KC around 12) as reported by Nielsen et al (2023). It is noted that Broekema et al. (2024) provides no limitations for the use of Eq. 22 which could lead to significant under dimensioning of rocks if users are not aware of this, see also Fig. 20 and line 889 to 893 in the mark-up manuscript.

Comments 2) A section that prescribes the followed calculation methodology of this paper specifically is (still) missing.

Response 2: I have added a step-by-step guide (App. C) with reference to relevant formulars and description of input parameters. I have also added the applied method to calculate bed shear stresses (App. B).

Comment 3) It is not clear what we are looking at in Figure 19 and how this Figure was made.

The lines are a function of ucw and theta, but these parameters are, generally, not independent of one another and should also not be treated as such: scour protection deformation

experiments are usually based on practical applicability ranges of environmental conditions. These are characterized by various (non-dimensional) parameters, but these are never independent of course.

Response 3: All the methods presented in Fig. 19 are controlled by parameters for waves, current, water depth, pile size, and rock properties (and in case of de Vos et al. (2011) some additional). This means that all the methods can be represented as Shields-numbers as function of ucw-number if other parameters are kept constant and this is how the curves are made. An explanation of this have been added to the relevant sections (4.2 and 4.3) and the input parameters for the different cases have been listed in Table 5.

Comment 4) Following comment 3, the example in Lines 989-999 would also not hold: the combination of various values of um and V that add up to the same total velocity in the KC numbers would lead to completely different bed shear stresses near the scour protection. All parameters that are used to describe scour protection performance are, in some way, interconnected, and treating them as completely independent is incorrect.

Response 4: I agree that my attempt to make a generalised example can be very hard to follow and can easily be misleading. I have replaced it with a numerical example, which I find easier to follow, see Fig. 20 and the associated text in Sec. 4.3.

Comment 5) In general, prediction methods have their best perfomance if applied in the way they have been designed for. For example, if you use the method of Den Boon et al. (2004) with a different way of calculation wave-orbital velocity it will give different results. From the current way Section 4 is presented, it is difficult to reverse-engineer how all parameters are calculated. In general, it is not easy to assess if the entire database of deformations is based on the same processing of the bathymetry data, which makes it hard to judge if results are that much

different or intercomparable. The example in Section 4.3.1 very clearly highlights this issue, and I expect it is not unique to only this method and dataset. I think it is good to acknowledge that a uniform (practical) definition of deformation depth is necessary and could be a reason that also the other datasets do not match well.

Response 5: I agree that the calculation of the different input parameters is of great importance. I also agree that the example in Sec. 4.3.1 is very illustrative in this respect, but not a fair representation of the method by Broekema et al. (2024) so I have changed the figure so the definition of um applied by Broekema et al. (2024) is used. However, I have kept a description of the effect of different definitions of um in the text.

Regarding Den Boon et al. (2004). From the article it is not clear how they defined um, so I have decided to keep the definition of um applied in the manuscript, but changing the text to emphasis this and that it may have change the Shields number for the limits relative to what was intended by the authors. Similarly for de Vos et al. (2011), but here the results of the equation is the damage number, S3D, which is not directly comparable to S/Dp, so again the definition of um is less critical as the comparison will be qualitative anyway.