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Review and rebuttal of the paper

Structural Optimisation and Behaviour of the Breakwater Integrated Oscillating Water Column Device

A combined 3D CFD and Structural Analysis

Goeijenbier et al.

Editor handling the paper: Eva Loukogeorgaki

Review Round 1

Reviewer A (Recommendation: Revisions required)

This paper investigates the (structural) optimisation potential for Oscillating Water Column (OWC) devices integrated in breakwaters. CFD simulations are used to calculate the hydrodynamic forcing on the basis of unidirectional regular waves. The outputs are then used to calculate stresses and moments on the structure. Starting from the configuration of an existing OWC at Civitavecchia, the authors investigate the effect of geometrical changes in the total material used and discuss the impact on the performance of the OWC. A simplified model is, finally, proposed as an alternative design method.

I have found the topic of the paper very interesting. It spans multiple disciplines (structural engineering, fluid mechanics and renewable energy) in the context of structural optimisation and performance. The approach taken is certainly interesting and results show potential. However, I have found some parts of the analysis relatively shallow. I have two main concerns and several minor considerations regarding this work. These are indicated in the comments below.

Major comments

1. *The authors have discarded dynamic effects on the structural modelling. The justification for this is confined to a single sentence (Lines 210-212). It seems that some further elaboration is required here. Have the authors simulated the dynamic effects and found little significance? Recent research on a very similar configuration (e.g. Dermentzoglou et al., 2020) might indicate otherwise. Additionally, apart from mesh sizes there is little information on the characteristics of the structural modelling. The paper would benefit from a more explicit description of the structural model.*

A: We thank the reviewer for pointing this out. We did not consider the dynamic response of the structure in the manuscript because of the more stocky nature of the REWEC. In the structure investigated in this manuscript, as opposed to Dermentzoglou et al., 2021, there is no real threat of structural failure due to the Confined Crest Impact (C-CI, Castellino et al., 2018 and 2021) thanks to the different configuration of the structure on the rear side of the recurved crownwall. Due to the presence of the rear side chamber, in this case, the structure is less slender than the one presented in Dermentzoglou et al., 2021; it has a different reinforcement system that connects the elements of the chamber and therefore induces a box effect and thus increases the resistance of the overall structure (Note that in Dermentzoglou et al., 2021 the steel reinforcement was clearly lacking and there was only a single concrete wall to support the recurve crownwall, see Figure from Castellino et al., 2018). Overall, it is true that the recurved crownwalls presented in this manuscript and in Dermentzoglou et al., 2021 are similar, the location of the two structures is the same port, but the structures are different.

At the end of Section 2.2.4, a description of these differences between the presented case and the one investigated in Dermentzoglou et al., 2021 has been added in the revised manuscript.

2. *Regarding wave modelling, the agreement observed between experiments and numerical simulations (Figure 4) appears to be quite poor. Given that the simulations relate to regular waves a much closer agreement would have been expected. Have the authors thoroughly validated their wave model? There is extensive literature that demonstrates a very high degree of agreement between VOF methods and*

experiments. Is the reason for these discrepancies related to potentially poor experimental data? Have other validation cases been considered (perhaps with a simpler setup)? Note that several wave modelling choices have been based on Peric (2017) which is not available in the literature.

A: We agree with the reviewer about the non-ideal comparison between the experimental and numerical water surface elevation. Due to the agreement of the wave pressure on a vertical wall presented in Figure 5, we, indeed, believe that the disagreement between the numerical and physical water surface elevation is mainly related with the laboratory data. We have used previous experimental data that was not directly collected by the Authors, and some details of the experiments were not available. Additional validation tests were carried out in an attempt to obtain better results. These include using the results for different wave periods and testing several other turbulence models. Different settings for mesh size and time step were used, as well as testing influence of potential errors in modelled geometries. Similar results were obtained in all cases.

Moreover, due to the additional validation steps that were performed, we believe that the disagreement between the numerical and physical results does not compromise the overall goal of the paper and that is mainly due to the lack of control we had on the physical results. Finally, the same numerical set-up has been applied throughout all the simulations and therefore also the relative results of the proposed methodology can be considered valid.

Minor Comments

1. *The literature review on the subject could be enriched. See Vicinanza et al. (2019) for a review.*

A: The literature review was extended on various aspects.

2. *The formatting of some citations in the text is not consistent/correct. For example, see Lines 27-28. I suggest the authors review their citations and correct discrepancies.*

A: We thank the reviewer for pointing this out.

The citations were checked and where needed corrected.

3. *Line 106: Sentence regarding the turbulence model should be rewritten with more appropriate grammatical structure.*

A: We tank the reviewer for noticing this.

The sentence has been adjusted

4. *Lines 118,136: The paper would greatly benefit from tables with boundary conditions and simulated conditions (and potentially other summary parameters). Generally, the description of parameters is*

lengthy and removes focus from the actual work. I suggest the authors considerably reduce the text and introduce schematics and tables to convey the same information.

A: We appreciate the reviewers comment.

Tables have been added to obtain a clearer view of the input data. Tables were added for boundary conditions, simulated conditions and mesh refinement in the wave zone.

5. *Lines 138,179: The reader will most likely not be aware of the remesher, trimmer etc utilities of the software. Some indication as to what they actually do would be beneficial. Also, no mesh convergence study is presented in the paper. Have the authors considered alternative mesh sizes before making their selection?*

A: Thank you for pointing this out. The surface remesher improves the quality of the model surface and optimises it is for volume meshing. The trimmed mesher generates the volume mesh with hexahedral elements. The tetrahedral mesher generates the volume mesh with tetrahedral elements. Different meshes sizes were tested during the validation and again with the full-scale tests. The testing consisted of multiple sets of mesh sizes and corresponding time steps. Refinement was mainly applied in the wave zone and in the vicinity of the OWC structure. The cells around the OWC make up approximately 80% of the total number of cells across the three domains. A trade-off had to be made between numerical accuracy and computation time. All tests showed similar results in terms of wave propagation and surface elevation in the main chamber, with a spread of around 5%. This was deemed as acceptable. In the final simulation, we opted for a (within reason) coarser mesh here, size due to the already long computation time. The adaptive time step model ensures the free surface in the OWC doesn't pass through a cell within a time step.

6. *Line 197: Is there a reference to justify the approach taken regarding the damping on the porous domain and halving it during design conditions?*

A: We understand that this raises a question. No reference was used for halving the values. It was known however, emergency valves are present in the turbines and that they open once certain limit pressures are reached in the chamber.

Simulating the action of a relief valve operating in conjunction with the PTO in this numerical model is quite complex. For this reason, we circumvented the problem by modifying directly the damping. We added a reference in the article (Scialò et al., 2021), where the PTO and the relief valves are working in parallel and the effects mentioned in the article are used in operational conditions for PTO control purposes. The response that we get, in terms of water column oscillations is quite similar, albeit only qualitatively (anyway, this limitation was already emphasized in the manuscript).

7. *Line 235: "The OWC …" sentence is not grammatically correct.*

A: Thank you for noticing this. The sentence has been adjusted.

8. Section 2.3: A schematic with the geometrical changes would be beneficial. Text is not always easy to follow. Perhaps the dimensions changed could be referenced back to variables using Figure 1.

A: We agree with the reviewer that an image would make things clearer.

This has been added to the text.

9. Line 291: Is there a reference to add re Equations 4-6?

A: The equations for the bending moment capacity follow from basic engineering practice in concrete design and are derived from Eurocode 1992-1-1. The equation for shear capacity is formulated in this as well.

10. The SLS criterion should be defined.

A: Arena et al. (2013) states an exposure class of XS3 for the Civitavecchia OWC. According to Eurocode 1992-1-1, a maximum crack width of 0.3 mm is permitted. This has been added to the text.

11. Line 316: The authors mention: "The optimal structural design is by definition not equal to the optimal geometrical design aimed to optimise the energy output.". Some elaboration on this statement would be beneficial.

A: We understand this description might be unclear, thank you for noting. The optimal geometrical design, or maybe better phrased as functional design, is in this case the design that yields the largest energy production in the situation of Civitavecchia, e.g. chamber and duct dimensions. From a structural point of view, this may not be the ideal design due to large spans er cantilevers, implying a heavy structure. The structural design from this view would be in terms of this research, least volume of concrete required, i.e. thin walls and short spans, while still maintaining structural integrity. The two designs are therefore by definition not the same and a compromise must be found between the two.

A more elaborate description was added to the manuscript.

12. Line 360: Is the recirculation statement supported by the simulations? Providing model results (e.g. velocity fields) would be a good addition.

A:Thank you for noting this. The phenomenon of recirculation can be observed in figure 13. Here, one can see that during inflow, there is both a flow directed downwards and upwards simultaneously in the front duct. Unfortunately, similar data for the wider duct is not available. One could argue however, that a wider duct reduces the forcing of flow to a singular direction. This could result in a lower amplification in the chamber.

13. Lines 423-425: Have the authors tried to address the corner problems? Is this a discretisation issue? It seems that data have been discarded without sufficient justification.

A: We understand that this point raises questions. This indeed mainly is a discretisation issue, in this case caused by the sharp corners in the model. The sharp corners result in peak values in stresses. Smoothening of these corners, e.g. rounding them, could cause a reduction of this effect. This was however not seen as necessary here. The consequences mainly affect the results in the vertical Z-direction. In all cases, this direction is not governing. (The error only causes stresses to be larger) The effect of the discretisation issue on the final result therefore is negligible.

14. Line 297: Values for f_{ck} and f_{cd} should be explicitly provided.

A: The values are added to the text. f_{ck} follows directly from the concrete class at 35 N/mm^2. The value for f_{cd} is found by dividing f_{ck} by a material factor of 1.5, giving 23,3 N/mm^2.

15. Section 3.23: The simulation time for each experiment is not provided. I suggest that the authors either provide this explicitly or use relative times.

A: At t=0, the wave is generated at the inlet. After approximately 40 seconds, the head wave reaches the OWC. The simulations were ran for a duration of 80 seconds. This was also added to the text.

16. Section 4: I am not convinced by the novelty of this method. The agreement demonstrated in Figure 12 (b and c) is not great and is probably oversimplifying the physics. Similarly, for directional effects, results would be more convincing if corner flows around U-wall and front wall were adequately resolved and presented. Generally, I am fine with presenting a reduced order approach but would refrain from suggesting it as alternative design practice.

A: We understand the reviewers doubt in this. The method obviously includes a simplification of governing physics, but in our opinion a reasonable one. The simplification using beam theory instead of 2D behaviour proves to be successful when observing transient behaviour. Unfortunately, this is hard to show on paper. The method works best for the front wall, mainly due to its relatively large height. The 3D structural behaviour comes into play at the constraint, where a deviation is observed.

A larger discrepancy is visible for the back wall. At this height from the bed, the only pressure acting on the wall is that of the air. Due to simplification of incompressible air, this pressure is constant and thus gives a constant bending moment. What is observed however is that it is in good correspondence with the maximum value found, which is of main interest for design purposes. We agree that it might be too soon to use it as a full design method. But looking at the transient results, the maximum values of bending moments are captured quite well, which makes the method suitable for preliminary calculations.

Regarding the directional effects, only normally incident waves were used within the study. The assumption that the pressure over the transverse width is predominantly constant, is supported by the velocity plots in figure 13. Moreover, the actual pressure values were checked in the simulation, were they indeed were found to be constant.

17. Figure 13: Contour plots would probably be more appropriate here.

A: Thank you for pointing this out. The wireframe was the preferred option for showing the transient flow field animation, as it was more intuitive to us. We agree that for a still image, a contour plot might be more clear. The figure is replaced in the manuscript.

18. Several typos can be found in the references and some inappropriate citation styles. Perhaps it is worth reviewing all the references and following a single citation style.

A: Thank you for pointing this out. The references were checked and corrected. Indeed there was an inconsistency in the number of authors printed in the reference list.

19. General comment: The study has focused on long-crested regular waves. Similar approaches are routinely followed in literature. However, real waves are random and short-crested. I am not suggesting the extension to investigate these in the present paper. However, a note about it would be a good idea. Particularly how these two effects could yield dynamic effects structurally and directional effects in the OWC. The latter can also be introduced through high Reynolds numbers close to U-wall and front wall. In turn, these effects would have a profound impact in the design cases.

A: We understand this comment. Our expectation is that the structural behaviour in terms of timing of extreme loading remains the same under irregular waves. In the study, it was shown these are related to defined phases in the oscillation cycle of the OWC. Generally, as used here, normally incident waves generate the largest forces, which is of main interest in this research. Although the short crested waves can load the structure at different time signatures and under different angles, their apparent wave length will always be larger than the cell width of the structure, meaning 3D effects on a cell are negligible. Therefore, the directional effects can be left out while determining maximum stresses. Waves with a smaller wave length result in a lower forcing, but impulsive breaking becomes a larger risk.

Reviewer B (Recommendation: Revisions required)

The reviewer would like to thank the authors for their contribution and study of structural optimization of breakwater integrated oscillating water columns (OWC) using static structural analysis methods. The study presents a numerical model divided for fluid domain, solid domain and turbine domain to estimate first the wave pressures on the as build structure in Civitavecchia, translate them into bending moments and normal forces in the different structural elements front-, back and u-wall, in order to finally derive the required thickness of the different elements.

Different ways for optimization of the structure are discussed:

First the geometry of the oscillating water column structure was varied and 7 structure modifications (plus the build situation) studied in terms of reducing the required concrete material. It was concluded that only a reduction in duct width to 1.2 m (W12) will lead to a significant decrease in concrete volume of 35% or 4.6 m^3/m, while keeping the loss in performance of the OWC to a minimum.

Second the bending moment and normal forces in X-, Y-direction were analyzed. The authors conclude that the 8 investigated structure modifications behaves the same in transverse direction (Y-direction) and therefore the structural analysis can be reduced to a 2D situation, decreasing the numerical model requirements.

Third the structural analysis is replaced by simple beam formulae (using wave pressures and structure height) for the structural elements U-, Front and Back wall and the results compared to results of the 3D numerical model. The authors state that the maximum bending moments are in the same order of magnitude and propose this as a simplified design method.

The paper is properly organized and to the point and concise. The illustrations and figures are useful, it could help to add a grid to the figures in order to better judge the difference between the two methods in Figure 12. Also the quality of Figure 1 needs improvement (axes are blurred). The authors identify the research gap that breakwater integrated OWC's have not been yet optimized structurally in order to arrive at a more cost efficient design. The objectives is therefore clear and the optimization of the concrete structure the goal of the proposed research. The topic is of interest to the field and connects the challenging study of wave kinematics with the structural analysis to obtain the structural response. The conclusions are supported by the data, however more explanation is required regarding the limitations of the proposed simplified calculation method (dynamic analysis for impacting waves/impulsive waves, implications of short crested sea states which are not covered by the study of regular waves).

A few remarks and questions remain, which are summarized below.

1. *In the introduction (Line 8) the authors state that the LCOE for wave energy is too high with 0.5-0.6 Euro/kWh (compared to wind and solar with 0.1 Euro/kWh), motivating the proposed research. Is there an estimate available how the reduction in concrete (-35%) will affect the LCOE?*

A: To these authors' knowledge, there is no publicly available article or report providing such an estimate. However, the general estimates mentioned at the beginning of the introduction (with the associated references) in our opinion corroborate the goal of the article. In this regard, it is worth mentioning that we are dealing with a device embedded in a vertical breakwater: a classical marine infrastructure in which the cost of concrete plays indeed a significant role. Thus, it makes sense to play with concrete reduction for reducing the LCOE.

2. *Line 500: Please improve the quality of Figure 1 (axes are blurred)*

A: Thank you for noticing this.

Figure 9 was replaced with a hopefully more clear figure.

3. *Line 501: In this study only regular waves are taken into account and a static analysis is performed. However, at least for the front wall there is the possibility of wave breaking and subsequent impulsive wave impacts/pressure distribution. I assume that in the proposed research no wave breaking took place*

or is assumed for the Civitavecchia OWC location? Could you elaborate how breaking impacts and potential impulsive pressure distribution would affect the simplified prediction method?

A: We thank the reviewer for pointing out this interesting aspect. What the Reviewer mentioned is correct, we did not assume any breaking wave conditions due to the deep water depth at the toe of the structure, i.e. 15 m operation and 15.5 m for the design conditions. We are aware that, in particular for the design condition, the risk of breaking exists, however, it was not considered in this preliminary analysis. The simplified prediction method may not be applied under breaking wave condition. The generated impulsive wave loading would possibly require to take into account the dynamic response of the concrete frontal wall that has been neglected in this manuscript.

This aspect has been mentioned in the text.

4. *Line 594: Constant pressure along transverse width (y-direction) is assumed. The sea-state in reality will be of short-crested and the impact pressures contain a stochastic/random nature to some degree. In Line 597 it is mentioned that the method is only valid if the 3D hydrodynamic behavior (subsequently the wave impact pressures) is simulated accordingly. Could you elaborate please on how the pressure variations, due to short-crested seas and stochastic behavior of the impacts, in transverse direction can practically be taken into account?*

A: We thank the reviewer for this interesting comment. Indeed, we checked this aspect before the development of the numerical model. The main reason why we believe that the assumption of constant pressure along the transverse width of each individual chamber is valid is related to the width of the chamber and the incident wave climate that characterises the area. The width of the chamber is 3.87 m, so also short crested wave approaching the structure under non-perpendicular direction would still have an apparent wave length along the structure that is always several time the chamber width. As an example if we consider the operation condition the wave length on a water depth of 15 m is around 57.5m, the assuming an angle of incidence on the structure equal to 80 degrees the apparent wavelength is 328m (L/cos(80)) that is several times the chamber width. If the angle is smaller the apparent wave length is even larger, therefore we believe that also short crested waves will not have any effect on the spatial pressure distribution that affects the individual chamber. Note that this aspect was also investigated by Malara et al. (2017), who showed that this “directional” effect is negligible (see Fig. 6 -7 in that reference) This aspect has been added to the text.

References:

Dermentzoglou, D., Castellino, M., De Girolamo, P., Partovi, M., Schreppers, G.-J. and Antonini, A., 2021. Crownwall failure analysis through finite element method. 9(1): 35

Castellino, M., Romano, A., Lara, J.L., Losada, I.J. and De Girolamo, P., 2021. Confined-crest impact: Forces dimensional analysis and extension of the goda's formulae to recurved parapets. Coastal Engineering, 163: 103814.

Castellino, M., Sammarco, P., Romano, A., Martinelli, L., Ruol, P., Franco, L. and De Girolamo, P., 2018. Large impulsive forces on recurved parapets under non-breaking waves. A numerical study. Coastal Engineering, 136: 1-15.

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Scialò, A., Henriques, J.C.C., Malara, G., Falcão, A.F.O., Gato, L.M.C. and Arena, F., 2021. Power take-off selection for a fixed u-owc wave power plant in the mediterranean sea: The case of roccella jonica. Energy, 215: 119085.

Review Round 2

Reviewer A (Recommendation: Accept submission)

Reviewer B (Recommendation: Accept submission)