Nearshore Tsunami amplitudes across the Maldives archipelago due to worst case seismic scenarios in the Indian Ocean
Response to Reviewer Two

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1 Introduction

The authors would like to thank the reviewer for taking the time to provide a comprehensive review of the submitted manuscript. We are glad that the reviewer found that "The work is significant, the presentation clear and well structured and the methods mostly up to date." and that "with relatively little effort, this work could in my view be of even more relevance."

Below, we respond to each of the comments from the reviewer, and provide a summary of the corresponding changes proposed to be made.

2 Response to comments

2.1 Response to Major Comments

2.1.1 Comment Number 01

A short discusion on why not a standard tsunami model was used, but a self built non-validated (at least not with the standard tsunami benchmarks according to Synolakis et al., 2008) based on Firedrake. What are the advantages compared to e.g. COMCOT or TsunaCLAW?

1. *The Thetis coastal ocean model has been benched marked and compared to similar models such as in Pan (2020), where results are presented for some of the standard bench mark cases also outlined in [https://nctr.pmel.noaa.gov/benchmark/]. However, it should be noted that here we used the 2d hydrostatic version of Thetis since we donot focus on inundation and rather on identifying the larger atoll scale patterns of tsunami flow. We appreciate that for more small scale simulations which includes topography and inundation, we would require the use of non-hydrostatic version of Thetis which has been benchmarked in Pan (2020) marked against standard tsunami inundation cases such as the Okushiri tsunami. In line with the proposed comment by the reviewer we will add a section to highlight the choice.*

2.2 Comment Number 02

2. What kind of criteria were used for the diverse decisions made: a. Mesh refinement - is it just proximity to coast? b. removal of islands from the large scale simulation - is it size?

1. *In selecting the mesh resolutions we made use of a sensitivity study carried out earlier, where we found that a mesh resolution of 100m was adequate to capture the bathymetry of the shallow lagoons (Rasheed et al., 2021). However, based on the reviewer feedback we propose to carryout a sensitivity study to understand the impact of varying mesh resolutions on the results.*

2. *The islands were removed from the simulations because inundation modelling was not considered in the simulations. This was mainly due to the fact that topographic data for the islands of the Maldives is not available in the public domain. If we were to model the islands as flat, as in some studies it would not match with field observations, where we find that the relative differences in island topographies produces differences in tsunami inundation heights across the islands. Hence, a pragmatic choice was made to remove the islands and focus on identifying regions where tsunami amplitudes were high which then could be marked as areas for future studies.*

2.3 Comment Number 03

To me the local resolution mesh sizes seem still rather large. A 5000 m mesh size at the Maledives Atoll coast for the large-scale simulation yields an effective wave lenght representation of 30 km or more (given the linear P1 elements of the DG discretization). Is this a reasonable scale? Additionally, the non-uniform mesh would allow for higher local resolution without much additional effort in terms of added unknowns, since the local area of refinement would cover only fractions of the domain. The same applies to the local simulations, where a 50 m mesh size allows to represent wave lengths of approx. 500 m or a little less that that. With island sizes of only meters in size, I doubt if this is high enough a resolution for quantitatively accurate results. Some sensitivity studies would be helpful in this.

1. *As the reviewer has suggested, we agree that these resolutions are large. However, we would like to highlight that these are mesh resolutions of the larger Indian ocean scale simulations carried out. Also, we would like to add that the larger simulation was carried out using GEBCO bathymetry which does not include any complex features of the Maldives archipelago and as thus increasing the mesh resolution with GEBCO bathymetry would not provide any advantages despite the additional computational time. For the nested simulation which covered the Maldives archipelago and utilized a higher resolution dataset which have the complex features present, we used 100m resolution at the lagoon scale. Since the atoll peripheries are also lagoons, this ensures that the atoll boundaries are also in 100m resolution. However, as the reviewer has suggested sensitivity studies will be helpful to highlight these choices we will include a section on the proposed revision to the manuscript.*

2.4 Comment Number 04

Since you indicated in the text that you are only considering wave heights at the coast and no inundation, what are the boundary conditions at the coasts then? In Harig et al. (2008) it was found that inundation BC are necessary even if not used to realistically prepresent coastal reflection of waves.

1. *Here, since we donot take inundation to account we set the coastline boundaries to no normal flow velocity. We have not taken to account the implication of the coastline boundary condition since the island sizes are relatively very small in comparison to even the atoll scale, and we find that the the impact of shallow lagoons on the periphery and the inner basin of the atolls have a much more significant impact on the flow patterns. However, we will carry out further simulations to check if changing the boundary condition at the coastline has any impact on the flow patterns.*

2.5 Comment Number 05

In order to evaluate the wave build-up it would also be valuable to consider the different wave lengths/periods in comparison to the obstacle size (atoll diameter e.g.) to have a conceptual understanding of this phenomenon. I hypothesize that a singular atoll of a size less than - say - half the deep ocean wave length will be passed by the wave without major harm, given the extremely steep bathymetry. But this would be an interesting topic of diagnostics, analysis and discussion for the different locations and angles of attack.

1. *The authors agree that this is a very interesting question, and indeed as we have demonstrated in the paper along with the field observations, due to the steepness of the atolls, tsunami waves passed across the Maldives with relatively low amplitudes. We would like to highlight that as we have mentioned across the manuscript the tsunami build up across some regions of the county occurred mainly due to the geometric shape of the atolls it self and the location, which forced tsunami to propagate at high velocity at these regions.*

2.6 Comment Number 06

You claim that such results are only possible by high resolution bathymetry data and go further to ask for even higher resolution in this respect. But you do not prove that this is really the case. It would be very instructive (and in your case probably easily possible) to actually demonstrate this claim by comparing the effect of diffraction, reflection and deflection in your large-scale and small-scale simulations. For example the results in figure 8, do they differ substantially for your large- and small-scale simulations? If so, I would buy your demand for ever higher resolution ;-) Here I assume that you use the same bathymetry data in your simulations, but that you interpolate to your unstructured mesh and therefore have different discrete bathymetries in your simulations.

1. *We agree with the reviewer that it was an oversight to not have compared the larger scale tsunami simulation carried out with GEBCO bathymetry with the smaller Maldives scale simulation carried out with a much more high resolution bathymetry. We have addressed this issue in past papers such as in Rasheed et al. (2020), where we showed that low*

resolution bathymetry is not adequate to capture the flow patterns across the complex bathymetry of the atolls of the Maldives because these datasets are completely devoid of features which actually give rise to these flow patterns. As suggested by the reviewer we will add this comparison in the revised manuscript.

2.7 Response to Minor Comments

1. *Since these are suggestions by the reviewer to make minor changes to the manuscript to fix figures and typing errors, we will make the changes as suggested in the revised manuscript.*

3 Conclusion

The main issue that the review has raised has to do with the choice of mesh resolutions selected for the simulations. We have attempted to answer the queries of the reviewer with the main justification that we have two simulations of two different scales with two different bathymetry datasets. However, as the reviewer has suggested, it would be helpful to include a sensitivity study we propose that we will add a section with additional simulations corresponding to a sensitivity study. We hope that these responses are to the satisfaction of the reviewver.

References

Pan, W.: Development of a non-hydrostatic coastal ocean model using the discontinuous Galerkin method, 2020.

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