Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/nhess-2021-109-RC2, 2021

Comment on nhess-2021-109

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Referee comment on "Multilayer modelling of waves generated by explosive subaqueous volcanism" by Matthew W. Hayward et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-109-RC2, 2021

Hayward et al. present a multilayer model to simulate waves generated from underwater explosions, modified to represent subaqueous volcanic eruptions. This is an interesting and important research avenue in volcanology as tsunamis from volcanic eruptions can be very deadly, but their conditions of formation are difficult to constrain and model. The multilayer numerical scheme is tested against other numerical models, previous lab-scale experiments and also field-scale observations from military tests, with convincing results. The model is then demonstrated for a single mostly subaqueous volcanic system, TaupÅ□ caldera, to investigate tsunami hazards from two different sized eruptions. The presentation of the paper is logical, first presenting the details of the physics of explosions and wave formation, then comparisons are made to experimental data and then finally the paper finishes with application to TaupÅ□ volcano. With my background, I have focussed my review and detailed comments in the application to TaupÅ□. I have attached a pdf file of the manuscript with comments and text edits.

The authors have done a good job of showing that their results are superior to other numerical solutions, particularly when compared to the results of lab-scale experiments of Prins (1958). I don't see any major issues with the model itself. However, from a volcanology point of view, the paper suffers from a lack of detail and discussion around the limitations of applying this to model to volcanic eruptions in general. The methods briefly mention the way in which crater size can be used as a measure for energy release. This may be the case in very simple one off explosions. However, the physics of magma-water interaction and explosive volcanic eruptions are not this simple. There is a bit of a disconnect between the model and the volcanology, particularly when using a very complex system like TaupÅ□ volcano to demonstrate the results. It appears that there are potentially two separate papers here which have been merged, with one on the details of the model and one on tsunami hazards at TaupÅ□ volcano. Given the nature and aims of this particular journal, the hazard aspect is particularly relevant for the audience. I understand that the authors want to simply demonstrate the application of the model and this manuscript is not meant to be a full tsunami hazard assessment for TaupÅ□. However, the nature of the results and their implications for volcanic hazards at TaupÅ□ means that more consideration should be given to this example. As it stands, the details of the particular scenario, the eruption parameters used and the limited discussion of the volcanology requires further work.

A few major points from a volcanology point of view that require some further thought:

1) **Estimating explosion energy.** One of the hardest parameters to estimate as an input for the model is the energy release from the initial explosion of a volcanic eruption. A common approach is to use the crater size to estimate total energy release. This is a simplification, but has been a useful in some cases (e.g. Taal, where there are explosion craters to measure and historic eruptions for comparison: Paris and Ulvrova, 2019). However, volcanic eruptions are often much more complex. Particularly those from rhyolitic calderas. In the case of the TaupÅ□ scenario used here, the authors have chosen eruptive volumes of 0.04 and 0.4 km^3 and then back-calculated the theoretical crater size and hence energy release from such an explosion. However, these volumes from past eruptions are from whole eruption sequences that may have occurred over hours to days. Using these volumes to represent a single explosion is not a wise approach and probably leads to an overestimation of energy that has little volcanic or geological context. See more detailed comments in the manuscript.

2) **Little discussion over background volcanology**. Scenarios for eruptions from Lake TaupÅ□ have little background other than saying they fall broadly within an area where eruptions have occurred over the past 5000 years. The references cited in this section are very sparse and volumes and eruption ages are often given without correct citation. There is very little discussion around where the most recent vents were, eruption styles inferred from the geological record, or where the current areas of hydrothermal venting are. Surely a phreatic explosion would also be a good scenario to model? The depth may also be highly variable, but little attention is given to why the particular depth used was chosen.

In summary, I think that the manuscript has the potential to be valuable for refining tsunami modelling and for application to subaqueous explosions. However, the illustrated use of the model and the scenario chosen for TaupÅ□ does not seem to be constrained by the appropriate data and therefore likely generates results that have little context for hazard assessment. I would therefore suggest that either the scenario for TaupÅ□ needs to be more carefully refined with justification behind the parameters used, or that it be dropped from the current manuscript to allow the focus of the paper to be solely on the new multilayer scheme for wave generation.

Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2021-109/nhess-2021-109-RC2-supplement.pdf