Replies to comments of Reviewer #2:

We thank the reviewer, Simon Barker, for their helpful and insightful review and generous comments on the work under review. We address the points raised (in blue) in order starting with the major points as written, followed by the minor comments (which were originally annotated on manuscript). Our responses are in black with suggested text revisions in red:

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 Taupō scenario used here, the authors have chosen eruptive volumes of 0.04 and 0.4 km3 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.

Reply 1:

Thank you for this important point. While we emphasise that the inclusion of this volcanic example is mainly for a demonstrative purpose and not to act as a full hazard assessment, we agree that more care needs to be taken in the treatment of the source and its justification. We propose changing the modelling scenarios in this last section as follows:

- Removing the two source sizes (0.04 & 0.4 km^3) investigated presently.
- Adding modelling of one source size derived from an approximation of mass eruption rate (MER) from the literature for a moderate size eruption at Taupō, for example the following parameters which is then input into the existing explosion model:

MER ($kg\ s^{-1}$)	Simulation Time (s)	Ejecta Volume DRE (km)
1.2E+07	1000	4E-03

This involves re-running of the model with these parameters, its insertion into the section and adjustment of the figures accompanying it, which is relatively straightforward and preliminary results from this in the form of an equivalent Figure 10 are presented below. The results obtained reflect that of a much smaller source in that wave amplitudes are significantly smaller, especially near-source, and the speed of propagation of these waves across the lake is slower. Discussion of the results of this revised example will involve many of the same components as originally done, with addition of additional discussion of the wavefield as suggested by Reviewer #1, by complementing the early work in the manuscript with highlighting ka and kh in this example and relating it to results in Sections 3 and 4.

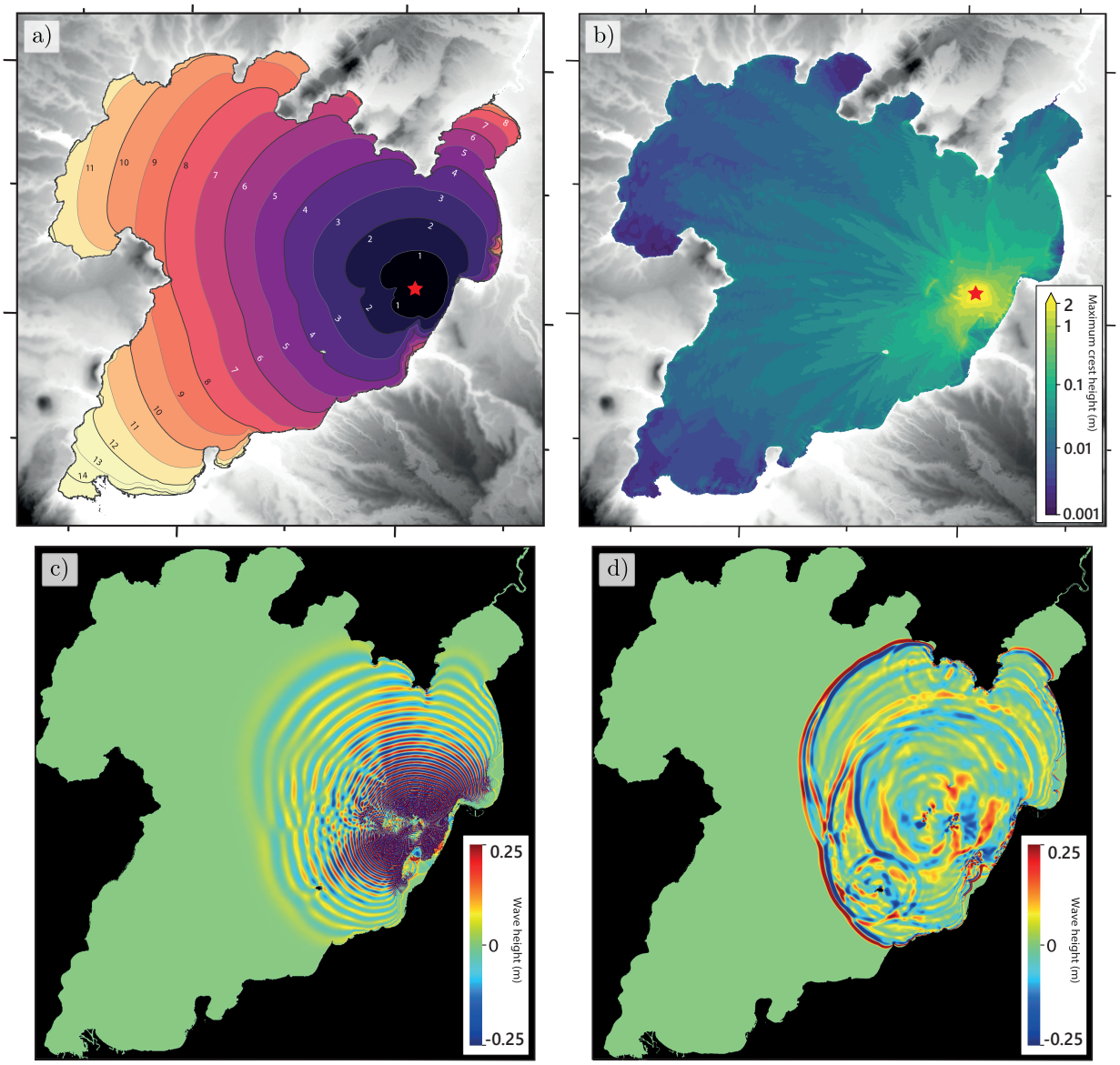

Figure 10. Multilayer simulation of potential volcanic explosion in Lake Taupō. (a) First arrival travel times in minutes. (b) Field of maximum crest amplitudes. (c) Snapshot of wave amplitudes at t = 4.5 minutes. (d) Equivalent of c) but using the Saint Venant scheme.

2) Little discussion over background volcanology. Scenarios for eruptions from Lake Taupō 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 Taupō 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 Taupō 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.

Reply 2:

Thank you again for the detailed review. As stated in Reply 1, we would intend to revise the parameters behind the Taupō example (Section 5) using a slightly refined method enabling additional justification. In addition to this, in order to satisfactorily complement this change and provide additional discussion over background volcanology, we propose to:

- Add introduction paragraph to Section 5 about how we now apply the multilayer model to a volcano as an example. Emphasise it's to demonstrate use, not hazard assessment.

- Include further background volcanology in the paragraph following, including typical eruptive styles and range of sizes, most recent events and general locations, and most recent hydrothermally active areas.

- Provide additional justification towards the chosen source size and location. Size as described in Reply 1, and location complemented by previous bullet describing the most recent events and hydrothermal features.

- Section 5.2 (Hazard implications) will be renamed to 'discussion' and be re-focused away from explicitly discussing impacts on hazard modelling, instead discussing these but primarily with model implications. It will be continually stressed that this is a use-case example of the model focussed on earlier in the manuscript and that many assumptions are required in modelling this scenario, leading to concluding that lots of care would be needed in specifying (and then justifying) a source model and its assumptions if utilising these methods for future work in a more complete hazard study.

- Reword conclusions (from Sect. 5, Line 405) to reflect more that this is an example - will focus on the modelling outcomes rather than hazard outcomes.

Reply 3:

Most comments as annotated on the manuscript are addressed as part of Replies 1 and 2. The remaining are detailed and answered below:

Abstract, Line 9: I think this needs to be reworded given the comments below over relationships between eruption volume and initial explosion. You also don't model a range between these end members. Reword.

Reply 4:

Text within Lines 9-11 shall be reworded as part of the planned revisions detailed in Replies 1 and 2.

Line 20: This sentence is a little awkward as written. Are you trying to say that they kill more people than other volcanic hazards? How does 5% of tsunamis being volcanic relate to the number of fatalities from volcanoes? These seem like 2 different concepts mixed up.

Reply 5:

Will reword as follows:

"*While volcanoes are estimated to be responsible for just 5% of all noted tsunamis since 1600 AD, they can be particularly dangerous in that they account for 20-25% of all recorded fatalities resulting from volcanic activity.*"

Line 67: This would also be a good place to note that you do not do a full probabilistic hazard assessment but that will be the focus of another paper.

Reply 6:

Text will be added at this point to reflect this:

"These tests are to establish fitness of the underlying models, which are then applied to a hypothetical explosive submarine eruption at Lake Taupō, New Zealand as an example use case in hazard applications."

Line 76: here and elsewhere. You switch between subaqueous and submarine several times. Perhaps stick with subaqueous for general application and then only use submarine when talking about examples in the sea?

Reply 7:

In the context of the cases and locations used in this paper, we agree it is more suitable to consistently use 'subaqueous' primarily the work, leaving subaqueous to actual marine cases discussed early on (and additionally 'laucustrine' where necessary in Sections 4 and 5). This will be revised throughout:

- Title
- Abstract Lines 2, 4, 12
- Lines 16, 41, 55, 76, 191, 310, 383
- Fig. 1 caption

Line 299: not clear why this data is not shown? Does it not exist? just need some clarification

Reply 8:

Unlike the series in 1965, the data from the three additional shots in 1966 were only partially included in the technical reports on the series as it seems they were primarily focused on investigating run-up and other phenomena rather than the immediately generated wavefield. We will refine this sentence to read:

“Further similar deep tests at Mono Lake in the following year, which were conducted for other investigations, delivered much greater amplitude waves in line with expectations, leading to the suggestion by Wallace and Baird (1968) that…”

Line 312: and note that these timescales are suitable for very short lived events.

Reply 9:

A small rephrasing will be made at Line 311 to address this:

“…it demonstrates suitability to model such events at these spatial and time scales. However, this test did not contain…"