Replies to comments of Reviewer #1:

We thank the reviewer, Stephan Grilli, for the very detailed and thorough review and the effort taken over the comments. We are pleased to have found their suggestions exceptionally insightful, and our responses are as follows (in black), in order of the written comments (in blue):

Note: Some responses have been grouped to address similar comments together.

Intro: Mention other NH multi-layer models applied to dispersive and nonlinear tsunamis such as NHWAVE (e.g., Ma et al., 2012, 2013; Grilli et al., 2015, 2019, 2021; Schambach et al., 2019, 2020, 2021).

Reply 1:

Thank you for this suggestion; we shall include a brief on alternative multilayer models at the end of the paragraph at line 61 to contrast with the previous sentence (use of SWE equations for problem in focus) and for seamless tie-in with the following paragraph.

The present model is mentioned to have a good shoreline algorithm, but this is known to be a difficult problem for multilayer models unless the number of layers is gradually reduced towards shore. How is this done here ? How well can shallow water/nearshore results be trusted when many layers are used ? Please discuss and provide additional information.

The breaking criterion/dissipation of breaking wave energy represented by Eq (16) needs some support and/or physical justification. This is an important assumption that will affect the height of propagating tsunamis and bores.

Reply 2:

The present work focuses on initialisation of wavefields that exhibit varying degrees of non-linear properties and frequency dispersion and their propagation. Of the three cases, only the Taupō example quantifiably investigates shoreline effects (maximum crest heights at incidence and arrival times) and none investigate post-shoreline phenomena such as inundation. This itself is a focus of a further work in review external to this paper.

Details of the methods used for shoreline interaction and breaking with justification can be found in works by the scheme's primary author:

Sections 3.6.4 & 3.6.5 (Multilayer scheme):

Stéphane Popinet. A vertically-Lagrangian, non-hydrostatic, multilayer model for multiscale free-surface flows. *Journal of Computational Physics*, 418:109609, May 2020.

Section 3.4 (Methods used in SGN scheme):

Stéphane Popinet. A quadtree-adaptive multigrid solver for the Serre–Green–Naghdi equations. Journal of Computational Physics, 302:336–358, December 2015.

We appreciate that the description of the breaking algorithm is brief and, in addition to the description in Reply 5, propose adding the following referring text from Line 170:

"*…are sign and minimum functions respectively. The breaking algorithm used throughout the multilayer scheme is described in greater depth in the model's defining paper by Popinet (2020)*".

L173: Please indicate papers in text where the scheme has been tested and validated.

Reply 3:

The reference paper is indicated at the conclusion of the paragraph; this can be altered to in-text citation instead at Line 174:

"*…and breaking Stokes waves (Popinet, 2020). Source code of these examples can be found at http://basilisk.fr/src/layered/nh.h#usage.*".

Furthermore, it is important to provide suitable access to source code, as the typed hyperlink at line 174 provides permanent access. Many of the tests of the multilayer scheme were used previously for other schemes and, while often described well at source, are also described in more detail in these prior works and will also be referenced at this point:

S. Popinet. Quadtree-adaptive tsunami modelling. *Ocean Dynamics*, 61(9):1261–1285, 2011.
S. Popinet. Adaptive modelling of long-distance wave propagation and fine-scale flooding during the tohoku tsunami. *Natural Hazards and Earth System Sciences*, 12(4):1213–1227, 2012.
S. Popinet. A quadtree-adaptive multigrid solver for the Serre–Green–Naghdi equations. *Journal of Computational Physics*, 302:336–358, 2015.
S. Popinet. A vertically-Lagrangian, non-hydrostatic, multilayer model for multiscale free-surface flows. *Journal of Computational Physics*, 418:109609, 2020.

L190: Prins' (1958) case appears to be relevant to the problem of concern here, although these are fairly small case experiments in which dissipation in breaking waves and through bottom friction may not be realistic or commensurate with field cases that are much larger cases (with much more turbulent flows). It would have been of interest to estimate the value of experimental Reynolds number and assess whether these were turbulent enough.

Reply 4:

We thank the reviewer for this suggestion. We calculate the experimental Reynolds number across the whole parameter space for the initially generated wave to be in the range 7.2×10^3 - 3.2×10^5 when considering the depth averaged velocity and water height, with those towards the 'bore' regime demonstrating the highest as would be expected. It is worth reiterating that we are only using this validation case 'as is' for investigation across models for generating varying regime wave trains and do not end up scaling it up itself. Indeed, the Mono Lake example is another validation case at a larger scale.

L205: for instance the breaking parameter is set to b=0.38 without justification. Is this to ensure a good agreement of model results with experiments ? Is this parameter general ? Would it be the same for breaking waves that are 100 m tall ? Is b dependent on ka and kh ? More support from earlier papers or justifications would be desirable here.

Reply 5:

The 'breaking parameter' b is general and was not chosen to ensure good agreement of the model with experiments. Its value was instead used from the previous uses of the multilayer scheme in Popinet (2020). In Boussinesq-type models, such a parameter usually sets the steepness threshold whereafter the equations in use 'switch' to SWE to handle the breaking shock, for instance in works using Basilisk e.g. Beetham et al. (2016, 2018) and investigated well by Orszaghova et al. (2012). In this multilayer scheme, such an approach does not generalise easily and is provided by limiting the maximum vertical velocity based on the characteristic horizontal velocity scale, together with slope-limiting to ensure stability during stronger breaking.

This approach we accept is a relatively simple parameterisation, and is validated with good success in Section 4.6 by Popinet (2020). In deep water, it is the same for breaking waves of varying height and while it may reflect ka, it does not depend on either ka or kh.

We propose that additional text and the following references will be added at Line 206 to justify the value used from prior work:

Beetham, E., Kench, P.S., O'Callaghan, J. and Popinet, S., 2016. Wave transformation and shoreline water level on Funafuti Atoll, Tuvalu. *Journal of Geophysical Research: Oceans*, *121*(1), pp.311-326.
Beetham, E., Kench, P.S. and Popinet, S., 2018. Model skill and sensitivity for simulating wave processes on coral reefs using a shock-capturing Green-Naghdi solver. *Journal of Coastal Research*, *34*(5), pp.1087-1099.
Orszaghova, J., Borthwick, A.G. and Taylor, P.H., 2012. From the paddle to the beach–A Boussinesq shallow water numerical wave tank based on Madsen and Sørensen's equations. *Journal of Computational Physics*, *231*(2), pp.328-344.
S. Popinet. A vertically-Lagrangian, non-hydrostatic, multilayer model for multiscale free-surface flows. *Journal of Computational Physics*, 418:109609, 2020.

L229-230 Please indicate there are many phenomena neglected in the single phase multi-layer model used, and these might affect the level of dissipation. This also relates to the fact that in the explosion tests (in California), the model would overestimate generated waves if not for decreasing the explosion energy by 25% without a lot of justification for this value, except for a statement that energy released may have been smaller than nominal. Please discuss.

Reply 6:

Thank you for this relevant comment; we will amend the sentence from line 231 to read:

"*…demonstrating the restriction of a single value for function on the 1D multilayer scheme not present on a 2D multiphase VOF solver, meaning that there are many neglected phenomena in the multilayer scheme such as bubbles and plunging breaks that, while this may be negligible in this lab-scale experiment, could be more significant at larger scale.*"

Further discussion about the Mono Lake energy disputation is included in Reply 14.

L226: One explanation for the strange results of the SGN model in the very near-field could be effects of very large vertical accelerations (ie non-hydrostatic pressure/dispersion) in the vertical column that are far outside the range of this model. Whereas in the far-field both ka and kh (not calculated by the way) would be back into acceptable ranges.

Reply 7:

We appreciate that little discussion was made on this observation and accept that a similar sentence can be added in explanation such as at line 228 (ka and kh are discussed in Reply 15):

"*…where, intriguingly, the water height temporarily increases above Q. This can be explained when considering the validity of these models does not extend to the very large vertical accelerations experienced near the shock, but instead out towards the resulting waves in the far-field.*"

The results discussed line 255-266 for positive or negative column, are closely similar to those obtained and discussed for positive or negative vertical bottom motions in experiments and model simulations (KdV), in their seminal papers, by Hammack (1973) and Hammack and Segur (1974, 1978a,b). Mention of this work and the similarity of physics and features in resulting wave trains would be interesting with a brief discussion.

Reply 8:

Thank you for this helpful observation, we will add the following paragraph after the one ending Line 258:

“*These results bear similarity to those found by Hammack and Segur (1974, 1978a,b) in experiments involving a piston producing vertical bottom motions described by Hammack (1973) and modelling using the Kortewig-de Vries equation, also with an initialised rectangular wave source. Notable similitudes include the generation of potentially multiple solitons of decreasing amplitude for shallow water positive initialisations (as seen in orange region of Fig. 5a), followed by a train of dispersive oscillatory waves and that no solitons are generated from negative vertical motions, instating producing a wave train of the type illustrated in Fig. 5b of an initial ‘triangular’ wave of greater speed than the trailing modulated oscillatory waves.*”

and adding the following to the reference list:

Hammack, J.L. and Segur, H., 1974. The Korteweg-de Vries equation and water waves. Part 2. Comparison with experiments. *Journal of Fluid mechanics*, *65*(2), pp.289-314.
Hammack, J.L. and Segur, H., 1978. The Korteweg-de Vries equation and water waves. Part 3. Oscillatory waves. *Journal of Fluid Mechanics*, *84*(2), pp.337-358.
Hammack, J.L. and Segur, H., 1978. Modelling criteria for long water waves. *Journal of Fluid Mechanics*, *84*(2), pp.359-373.

Eqs. 1 and 2 and related parameters lack justification and appear to be simply stated here. Eq. (2) is used in the lake Taupo case but similarly without a justification. More explanations should be introduced at this stage in support of these equations.

Reply 9:

These equations are introduced in Section 2.1 and are accompanied by a description of their derivation and referenced source, alongside the parameter definitions in Section 2.1.1 and a statement of the data sources used for calibrating the model (Le Méhauté and Wang, 1996). The equations themselves have been used in prior works of volcanic explosion context (Torsvik et al. 2010, Ulvrová et al. 2014, Paris & Ulvrová 2019), and this is mentioned in the opening paragraph of Section 2.1. We do agree with the reviewer that this justification is not clearly stated, and will add some discussion when introducing the equations.

While only one is used in this work, it is important to describe both to reflect that these are empirically calibrated models and alternatives that may possess variable validity were also produced by their authors. The choice of these used here reflects their prior use in previous work for similar contexts and its superior performance in the original deriving work; we understand that this is not sufficiently stated later in description of the field case methods so this can be primarily added into Section 4 and Line 275, and Section 5 will be amended to refer to the same methods used in Section 4.

L270-273: The text is not clear and somewhat misleading. One would understand depth to be 1946 m but then Fig 6 and earlier text mention 39 m and up to 45 m ? Is 1946 m the lake MWL altitude ? If so, this really does not matter. And the sentence “… which left 2 m of shore topography..” should be clarified.

Reply 10:

Thank you for this observation, we will clarify the text starting at line 272 to the following:

"*…where the lake water level was set at elevation above mean sea level 1945.7 m. This left approximately 2 m vertically of the bathymetric model dry to act as the shore surrounding the lake.*"

In this first field case as well as in the second one (and some earlier simulations) there is no mention of the horizontal grid range in the automatic refinement and the number of vertical layers, nor is there a convergence study justifying that the vertical discretization is sufficient. The model has automatic refinement but still some information on the numerical parameters used would be important to provide.

Reply 11:

While the parameter specifying maximum (horizontal) refinement level (plus resolution) and number of layers are specified in Section 3 (at end of 3.0 and in Table 1) and Section 5 (sentence beginning in Line 328), we acknowledge this information is absent in Section 4 and shall be included at a position such as at the end of the paragraph at Line 724.

L276: The initial profile of the free surface is modelled with Eq. (2). This is stated without explanations or justification. Why not Eq (1). Were there field measurements indicating Eq. (2) was a good approximation?

Reply 12:

As discussed in Reply 9, this will be amended.

The Figures shown in Fig. 7 and 8 appear to have been inverted and do not correspond to the caption. Please correct.

Reply 13:

Apologies for this oversight and thank you for the keen observation - this shall be corrected.

In Fig. 8, the match of model and experiments requires a 25% reduction of the explosion energy. Besides the charges this could also reflect an inaccurate level of dissipation of breaking wave energy in the model in the near-field, also there could have been in the field some energy transferred to the bed as elastic deformation and elastic waves. Supporting insufficient breaking wave dissipation would be also the fact that later in the time series the model with reduced energy underpredicts experiments. Please discuss.

Reply 14:

Thank you for these good points and we agree that they should be included in discussion in this section, for example as follows:

At Line 293:

"*The latter part of the initial wave group maintains higher amplitudes in the experimental trace for both records, whereas the envelope decay is sooner in the numerical model. Shot 3 also seems to exhibit a positive amplitude shift in the early part of the experimental envelope. These could be* *genuine underestimation of wave amplitudes towards the end of the initial group which could be due to variations in dissipation of the initially generated breaking waves.*"

At Line 303:

"*…in addition to the resultant early wave group and individual phases. Additionally, additional energy dissipation not accounted for in the physical model may be responsible for the greater fit of the reduced yield simulation, such as loses from a higher amount of dissipation from breaking of the initial waves from the explosion or some of the energy transferred to the nearby bed as elastic deformation. While the initialisation model is calibrated to charge depths relative to water depth, bed characteristics were not strongly considered.*"

Fig. 5 is very interesting and important to understand the salient physics of this case. However, one is disappointed that kh and ka, the measures of dispersion and nonlinearity are not calculated nor discussed in the 2 field cases, with the former, if indeed very large (say beyond 3) justifying the need for a multi-layer NH model, rather than eg a SGN model.

L311: Please replace or complement relatively deep nearfield by actual values of kh. There should be a discussion of nonlinearity and dispersion in the results shown here and in the next application. Also relate these values to those in Fig. 5 and hence the kind of wavetrain obtained.

L335: Like in earlier field case, some mention of the kh and ka values of computed wavetrains at gauges would be useful. It is pretty clear that a SV model will fail in this dispersive case but why not running the SGN model ?
In fact if one assumes depths of 20 or 50 m and periods of 15 or 65 s, one gets kh = 0.22 to 1.11 and for a = 3 m, ka = 0.01-0.15. So for the wavetrains at gauges, waves are moderately nonlinear and intermediate water so a SGN model should work well. Here as well no mention of the number of layers used in the NH model is made.

L359-362: As before, no information is provided on nb of layers required and since SGN was not tested one does not know if a NH multilayer model was really needed her, particularly in view of the large uncertainty on the initial empirical source shape and level of energy. Please discuss.

L364: A SGN model such as e.g. FUNWAVE (Wei et al., 1995; Shi et al., 2012), which has extensively been applied and validated against tsunami benchmarks and case study (e.g., Watts et al., 2003; Day et al., 2005; Ioualalen et al., 2007; Abadie et al., 2012; Kirby et al., 2013; Grilli et al., 2015, 2019, 2021; Schambach et al., 2019, 2020, 2021; Tappin et al., 2008, 2014), particularly landslide tsunamis, also has NH pressure terms and breaking algorithm that have proved accurate in shallow water/nearshore. The multi-layer scheme may be needed for deep water explosions with very large kh values but not so much for the nearshore. This justifies many investigations of landslide tsunamis or tsunami from volcanic collapse, where a NH multilayer model was used in the near-field of the tsunami source and coupled to a SGN model for the far- field and runup/ inundation where such models usually perform better than multi-layer ones. See, e.g., NHWAVE-FUNWAVE applications (e.g., Grilli et al., 2015, 2019, 2021; Schambach et al., 2019, 2020, 2021; Tappin et al. 2014). A brief mention of this would be of interest at least in conclusions/discussions.

Reply 15:

Thank you for this very important and constructive comment. Following this and later comments, we will add results and discussion of the kh and ka type parameters to the two field cases. As the Taupō section as a whole is under revision following comments by Reviewer #2, the Mono Lake case additions only are described here:

In a new paragraph following Line 295:

"*In terms of parameters pertaining to dispersion and nonlinearity, kh and ka, waves in the initial group near the source at the nearest gauges on each radial were in the ranges 1.34 < kh < 3.56* *and $3x10^{-3}$ < ka < 1.181 and across the gauges beside the shore were in the ranges 0.105 < kh < 0.235 and $5x10^{-4}$ < ka < $3.6x10^{-3}$. As would be expected, moderately nonlinear waves are generated and kh decreases as the waves approach the shore and become shallow, whereas wave steepness ka decreases on average towards shore.*"

and at Line 311:

"*…accurately propagate such a source in the relatively deep (kh≈3) near-field through to the shore, it demonstrates suitability to model such events at these scales.*"

We acknowledge that while we used the SGN (Boussinesq-type approximation scheme) in the laboratory case, we did not perform the field cases with with it as we were primarily testing the multilayer scheme against a SWE method with the context of comparing against that most used for wave and tsunami modelling. We appreciate that the SGN method may produce results of similar contrast against SWE in this case, however our primary focus is that of demonstrating a usage case where a scheme capable of handling a wide range of dispersion characteristics as well as the initial source. Additionally, we found in initial investigations of both the cases in Section 3 and 4 that the multilayer scheme in Basilisk performed more efficiently than the SGN scheme in the same framework, for example included in this work in Table 1.

We propose adding multiple parts to address and inform the potential audience about the existence and capabilities of SGN-like models as described in the comments. Firstly following the paragraph ending Line 183, and secondly in an additional paragraph at the end of Section 5.1 after Line 369 which will detail alternatives to the multilayer scheme in this case, referencing ka and kh values with examples of SGN schemes like FUNWAVE and other higher schemes such as NHWAVE-FUNWAVE and SWASH.

L343 and L356: the slower waves for the smaller V could be a result of reduced nonlinearity of wavetrains and hence amplitude dispersion effects. Please discuss.

Reply 16:

Thank you for this point, we will include discussion of frequency and amplitude dispersion at these lines and pending the changes and corrections suggested by Reviewer #2 for this section.

L380-381: Were the very large volumes listed here released at once in giant explosions or caldera collapses or were they the total deposits during a particular event. In this case only a small fraction could have been responsible for tsunami generation such as modelled here. Please be more nuanced in this statement.

Reply 17:

If section is to be kept (considering Reviewer #2's suggestions), will consider rephrasing following sentence as:

"*Events of such magnitude, even when considering they may consist of multiple smaller episodes, undoubtedly carry wind ranging…*"

L393-394: the actual nonlinearity and dispersion parameters were not mentioned nor discussed in the 2 field ases. This weakens the conclusions and the support for a multi-layer NH model.

Reply 18:

- As described in previous responses (Reply 15), these will be addressed such that this initial (or a rephrased similar) sentence is supported by the prior work.

Minor comments

L44 remove “is needed”.
L88 replace specialist by specialized ?
L106 change to : where c=.. is an imperial...
L212 lb of TNT ? Be specific
Fig. 6a : Some contour lines of depth and topography would be useful.
L267: replace charge by charge magnitude ? or energy ?
L309: I would replace excellent by good or reasonable in view of the many hypotheses introduced to obtain a reasonable match between model and field data.
Fig. 9: A table with actual location/depth of eruption and all the gauges would be useful. Information of gauge depth is mostly missing.
In caption, replace building by built-up
L341: replace all by the entire
L340-343: text is not clear
Fig. 10 caption. Please indicate this is for the larger V case.
Fig. 11 caption: Make reference to table where gauge locations and depth are listed

Reply 19:

Fig 9: A table with shot and gauge location/depth information, available in the Mono Lake technical reports, will be included in an appendix or supplementary material.

L340-343: Rephrased as: “*The initial phase velocity typically starts from 40-45ms-1 until they heavily interact with the nearby Horomatangi Reefs. Due to the limited area of the lake, all of the shoreline experiences wave phenomena from these sources within 15 minutes, with only a slight increase of arrival times for the weaker run, due to both a slightly shorter period group generated by the smaller source and reduced nonlinearity.*”

All other minor suggestions accepted with thanks.