Reviewer's comments:

Major comments

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).

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.

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

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.

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.

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.

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.

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.

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.

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.

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.

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.

L276: The initial profile of the free surface is modeled 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 ?

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

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.

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.

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.

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.

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 modeled here. Please be more nuanced in this statement.

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

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: relace 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

References

Abadie, S., J.C. Harris, S.T. Grilli and R. Fabre 2012. Numerical modeling of tsunami waves generated by the flank collapse of the Cumbre Vieja Volcano (La Palma, Canary Islands) : tsunami source and near field effects. *J. Geophys. Res.*, 117, C05030

Day, S. J., P. Watts, S. T. Grilli and Kirby, J.T. 2005. Mechanical Models of the 1975 Kalapana, Hawaii Earthquake and Tsunami. *Marine Geology,* 215(1-2), 59-92, doi:10.1016/j.margeo.2004.11.008.

Grilli S.T., O'Reilly C., Harris J.C., Tajali-Bakhsh T., Tehranirad B., Banihashemi S., Kirby J.T., Baxter C.D.P., Eggeling T., Ma G. and F. Shi 2015. Modeling of SMF tsunami hazard along the upper US East Coast: Detailed impact around Ocean City, MD. *Natural Hazards*, 76(2), 705-746.

Grilli, S.T., Zhang, C., Kirby, J.T., Grilli, A.R., Tappin, D.R., Watt, S.F.L., Hunt, J.E., Novellino, A., Engwell, S.L., Nurshal, M.E., Abdurrachman, M., Cassidy, M., Madden-Nadeau A.L. amd S. Day 2021. Modeling of the Dec. 22nd 2018 Anak Krakatau volcano lateral collapse and tsunami based on recent field surveys: comparison with observed tsunami impact. *Marine Geology*, 440, 106566.

Hammack, J.L., 1973. A note on tsunamis: their generation and propagation in an ocean of uniform depth. *Journal of Fluid Mechanics*, 60(4), pp.769-799.

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., 1978a. 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., 1978b. Modelling criteria for long water waves. *Journal of Fluid Mechanics*, 84(2), pp.359-373.

Ioualalen, M., Asavanant, J., Kaewbanjak, N., Grilli, S.T., Kirby, J.T. and P. Watts 2007. Modeling the 26th December 2004 Indian Ocean tsunami: Case study of impact in Thailand. *Journal of Geophysical Research*, 112, C07024

Kirby, J.T., Shi, F., Tehranirad, B., Harris, J.C. and Grilli, S.T. 2013. Dispersive tsunami waves in the ocean: Model equations and sensitivity to dispersion and Coriolis effects. *Ocean Modeling*, 62, 39-55.

Ma, G., Shi, F. and Kirby, J.T., 2012. Shock-capturing non-hydrostatic model for fully dispersive surface wave processes. *Ocean Modelling*, 43, 22-35.

Ma, G., Kirby, J.T. and Shi, F., 2013. Numerical simulation of tsunami waves generated by deformable submarine landslides. *Ocean Modelling*, 69, 146-165.

Schambach, L., Grilli, S.T., Kirby, J.T. and F. Shi 2019. Landslide tsunami hazard along the upper US East Coast: effects of slide rheology, bottom friction, and frequency dispersion. *Pure and Applied Geophys.*, 176(7), 3,059-3,098

Schambach L., Grilli S.T., Tappin D.R., Gangemi M.D., and G. Barbaro 2020. New simulations and understanding of the 1908 Messina tsunami for a dual seismic and deep submarine mass failure source, *Marine Geology*, 421, 106093

Schambach L., Grilli S.T. and D.R. Tappin 2021. New high-resolution modeling of the 2018 Palu tsunami, based on supershear earthquake mechanisms and mapped coastal landslides, supports a dual source. *Frontiers in Earth Sciences*, 8, 627

Shi, F., J.T. Kirby, J.C. Harris, J.D. Geiman and S.T. Grilli 2012.A High-Order Adaptive Time-Stepping TVD Solver for Boussinesq Modeling of Breaking Waves and Coastal Inundation. *Ocean Modeling*, 43-44, 36-51

Tappin, D.R., Watts, P., Grilli, S.T. 2008. The Papua New Guinea tsunami of 1998: anatomy of a catastrophic event. *Natural Hazards and Earth System Sciences*, 8, 243-266

Tappin D.R., Grilli S.T., Harris J.C., Geller R.J., Masterlark T., Kirby J.T., F. Shi, G. Ma, K.K.S. Thingbaijamg, and P.M. Maig 2014. Did a submarine landslide contribute to the 2011 Tohoku tsunami ?, *Marine Geology*, 357, 344-361

Watts, S. T. Grilli, J. T. Kirby, G. J. Fryer, and Tappin, D. R. 2003. Landslide tsunami case studies using a Boussinesq model and a fully nonlinear tsunami generation model. *Natural Hazards and Earth System Sciences*, 3, 391-402.

Wei, J., Kirby, J.T, Grilli, S.T. and Subramanya, R. 1995. A Fully Nonlinear Boussinesq Model for Surface Waves. Part1. Highly Nonlinear Unsteady Waves. *Journal of Fluid Mechanics,* 294, 71-92.