Review of revised manuscript: Lituya Bay 1958 tsunami –pre-event bathymetry reconstruction and 3D-numerical modelling utilizing the CFD software Flow-3D

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Comments on the revised manuscript

For the 1934 Tafjord event, I suggest you include also:

> Harbitz, C., Pedersen, G., Gjevik, B., 1993. Numerical simulations of large water waves due to landslides. J. Hydraul. Eng. 119, 1325–1342.

that summarizes all previous literature (including Holmsen 1936 and Furseth 1958). Braathen et al 2004 rather discuss the rockslide itself, perhaps less relevant for the present study not focussing on rockslide processes.

For the 1963 vaiont event, perhaps consider:

> Ghirotti, M., 2012. The 1963 Vaiont landslide, Italy. In: Clague, J.J., Stead, D. (Eds.), Landslides: Types, Mechanisms and Modeling. Cambridge University Press, pp. 359–372.

Linguistic improvements to the new "green" text are still recommended in several places. A few examples:

- "The present work also aims to contribute to this. With the focus on the Lituya Bay 1958 tsunami it is addressed to the following research questions"
- "Since reproducing the physics (rheology) of the rockslide is not target of this study, the simplified concept of a “denser fluid” in comparison to the seawater is adopted for simulating the impact from the slope. Additionally, the use of a fluid gives the possibility to adapt the volume shape according to the topographic surface during the collapse process"
- "A number of studieis based on the application of analytical equations, amongst derived from experimental analyses. With it, amplitude of the impulse wave as well as maximum run-up were reconstructed"

- "The Gilbert Glacier body is recreated starting from the descriptions provided by Miller (1960) and the available cartography (see Figure 16 in Miller, 1960), so the shape of the two deltas located in front of the glacier. The same is done for the Crillon Inlet. Miller (1960) describes a scars area located northern of the maximum observed run-up as pre-existent the tsunami event (Fig. 2b)"

It still puzzles me that a discussion on rockslide rheology is more or less omitted. There should at least be a discussion on how well a denser fluid replaces a rockslide (even though the masses are "flowing"). Does it suffice to state that the concept reproduces the wave dynamics – and later say that the numerical model converges because the waves are reproduced? And why does the use of a "denser fluid" give a (better) possibility to adapt the volume shape than another kind of flow rheology (p.3 L19)?

Ad convergence, I still do not follow all details in the reasoning. Firstly, the dense fluid must be "tuned" somehow to achieve good agreement with the observed run-up heights (cf. comment above). Secondly, grid resolution is refined to optimize the results. How can you then know that the model converges? I suggested already to perform a standard convergence test without comparing with observations. If this is too computationally costly, it could perhaps be done for a smaller area? Maybe this is what is shown for the impact area (Figure 13 refers to 5m x 5m x 5m)? Further, I do not understand why it is referred to other available numerical models in previous studies (probably with other geometries, other rockslide volumes and wave lengths, other equations,…).

What is meant by (p. 6) "These studies stated that a straightforward landslide-generated tsunami leads to wave floods. If the slide would lift a volume of water equal to the slide volume upon the sea level, it results in less than one tenth of the observed one"?

The difference between first and second order models must be explained before it is mentioned for the first time (p. 10) in the sentence "Both the first and the second order approaches for the density evaluation implemented in Flow-3D are adopted to simulate the two fluids and their interaction"

Boundary conditions: No reflections considered at the boundary locations of the domain? Are you saying that you use the same boundary condition for the Gilbert and Crillon Inlets, as for the seaside at La Chaussee Spit? Why do you need an open boundary condition at the Inlets where you are also modelling the inundation? Is the computational domain smaller than the full yellow rectangle of Figure 5?

P. 12 L15: Should the text point to Figure 5 (rather than 6). Caption of Figure 5 should explain the two locations of x_0.

P. 12 L 18-29: I understand that surface roughness is divided into two components where the first one describes how well the mesh reproduces the terrain/structure and the second one (named equivalent grain roughness) describes vegetation. However, I get confused when it is subsequently stated that all computations apply an almost smooth topographic surface (which I interpret as terrain regardless of vegetation) "equal to 0 m in additional equivalent grain roughness" (that you just said was related to vegetation), before you finally say that values of 1m and 2 m are applied to analyse the sensitivity to vegetation.
In your reply (#30) you say that equivalent grain roughness is zero, and that roughness is related to processing (no further additional roughness).

P. 13 L 6-17: Center of gravity obviously changes position during movement. What you probably want to say is that the centre-of-mass changes position relative to the front of the denser fluid? I don't understand Figure 6. Should time of impact be indicated?

P. 16: Flow velocity is quantified several places. What is this, and how is it quantified in 3D? Is this the particle speed (or current speed, i.e. the absolute value of the velocity without a direction) extracted from your modelling results?

P. 17 L3: What causes an attenuation process? Is the water depth reduced? You say it is because the bay floor increases. Are you referring to nonlinear waves with a wave celerity that is reduced when the amplitude is reduced? I don't follow.

When you say in your reply that 'on the other side (north to the island) the flow velocity is <reduced?> due to a breaking process because of the lower bay floor depth' – are you saying that the wave is breaking because the water depth decreases? Perhaps ok, amplification and breaking due to shallowing could be an explanation, but in the manuscript p. 18 L 4 breaking is rather stated to result from amplification when the second wave catches up with the first one. Why does this happen? A result of different wave lengths produced at different times with different wave speeds causing wave fusion? Some clarification is needed.

To your reply on instability and CFL-criterion (#29): It may be correct that your model is stable and converging, but again you haven't really justified this by standard test procedures. By the way, the CFL criterion is relevant also in 3D (not only for 1D or 2D shallow water equations):
https://en.wikipedia.org/wiki/Courant–Friedrichs–Lewy_condition

P. 21 L26: The contribution of the material generated from the deltas displacement, the sediment released by the glacier and the eroded soil from the inland haVE to

Your reply on hazard analysis (#32): I assume hazard analysis is always about predictions (as you say forward-oriented; not hindcasts). If uncertainties are treated in a probabilistic manner running thousands of scenarios, a computational time of ~days are difficult to handle. Hence, computationally costly models serve their right when running a limited number of scenarios for introductory analysis and increased physical understanding, but not for hazard analysis based on a larger number of scenarios.

100 Figure 10 Caption: The purple color in the inland representS

Overall recommendation on the revised manuscript

My recommendation is **Major Revision**.