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

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Overview

The paper describes the tests of the CFD code Flow-3D for rockslide tsunamis, applied for the 1958 Lituya Bay event. Both a simplified geometry with a "denser fluid" rockslide and a "real" geometry are applied. Efforts are made to reproduce the pre-event bathymetry.

General comments

The manuscript would benefit from being shortened, citing existing literature rather than repeating. This is especially relevant for the first two sections (first 6 pages), that do not bring much new knowledge.

Some physical explanations are hard to follow (examples presented below).

Why do you say that a denser fluid is a suitable concept for the 1958 Lituya Bay rockslide. This must be substantiated from a discussion of rockslide rheology, which is presently completely left out. The slide is modelled as a Newtonian fluid (Navier-Stokes equations) and I would not call that a suitable concept for a rockslide. What is the "viscosity" of the rockslide?

Sensitivity to (spatial) grid resolution is mentioned in several places. It is not a new thing that results depend on the resolution. And it is not sufficient to conclude that a resolution of 15x15x10 m3 best reproduces the trimline. What if the resolution is even finer? Will the results be further improved (or will spatial refinement even cause instability)? I am missing a regular convergence test quantifying the convergence rate, or (in 3D) at least a conformation that the differences are reduced between each refinement.

Some phrases are repeated several times (as e.g. the 524 m), possibly indicating that the structure of the paper is not optimal.

The linguistics of the manuscript should be improved (not further detailed below).

Detailed comments

Be careful with terms like 'wave height' (crest-to-trough) and 'amplitude' (above equilibrium level for a harmonic wave). Better use e.g. 'surface elevation'.

Be careful with the use and definitons of terms like rock fall, rock avalanche, rockslide etc.

P. 2, L 20: Studies of rockslide tsunamis started long before Fritz et al. (2001), but the references listed here are perhaps meant to be relevant for the 1958 Lituya Bay event only?
P. 2, L30: I do not agree that the questions listed here are all "open questions". Much work has already been done to answer them.

Would it be better to switch Sections 2.1 and 2.2?

P. 4, L. 38: Better use 'head of the bay' rather than 'end of the bay'? At least be consistent throughout the text.
P. 5, L12: What is the difference between physical scale tests and empirical studies? It seems like the terms are mixed further down (e.g. in p. 5 L34 and P. 6 L1 are mentioned experiments under the heading 'Empirical studies')

Section 2.3.3: Several previous studies are mentioned. However, for most of them it is not mentioned what equations are used, rendering the descriptions less useful. The importance of nonlinearity and dispersion should be elaborated.

Section 3.1 might represent a valuable contribution, but is hard to follow.
P. 7, L 33: Volume is $3x10^8$ m^3. P. 4, L 13 and P 7, L 40 say $30x10^6$ m^3. Please comment on this.

Section 3.2.1: I would prefer to see what equations are solved. Also, first and the second order approach for the rockslide must be elaborated further already here (is this the order of the scheme for the phase/density transport equation?). The explanations that follows on p. 12 do not suffice either. P 9: Much of the discussion is on turbulence and density, while slide-rheology is not mentioned at all. See also General Comments above.

P. 9, L. 16: "These models (first or second order) compute a separate transport equation for the density and simulate the movement of two different fluids (of different densities) in the domain." This is basically VOF methodology that is already mentioned in L. 3.

P. 10, L. 2: Cell size (relative to wave length and relative to temporal grid increment) is more important than number of cells.

P. 10, L. 35: Why outflow boundary conditions here? Why not accept reflections (from steep/closed boundaries)?

P. 11, L. 6: Why does the "computational surface" have a sort of a roughness? This should be explained. A numerical "staircase slope" in a vertical transect will not pose the same kind of reflections as a "staircase no-flux boundary" in a horizontal projection.

P. 11, L 23: The slide is slower for a steeper angle? This is counter intuitive and deserves some discussion. A longer travel distance does not "allow the slide more time to get higher speed". Or: what if the slope is zero? Without friction, both slides should have the same velocity at the end of the slope (v ~ sqrt(2*g*H)). Including friction, gentle (and longer) slopes means more energy lost to friction (Energy = Force x distance). This is especially the case for real cases, where friction is of Coulomb type and thus higher for more gentle slopes.

P. 12, L. 7: How can you compare your 3D results with the 2D experimental studies? See also statement P. 17, L. 14.

P. 13. Time intervals refer to time from release, while text all the time describes seconds after impact. This is confusing. What do the x_0 values refer to? (also on p. 14)

P. 13, L. 17: Is the velocity the same as wave celerity or speed of wave propagation? And if so, how is that quantified?

P. 13, L. 24: Is flow height relative to terrain? If so, normally referred to as flow depth.

P. 13, L. 29: 54 seconds = (34+12+8) seconds from release (not from impact)?

P. 14, L. 28: How can the wave slow down due to constriction/narrowing? And why is the wave slower in deeper water? Wave celerity should increase with water depth.

P. 15, L. 23: Is the rockslide considered to be a turbidity current?

P. 15, L. 26: Why is a high-quality reconstruction of the bathymetry more important where the wave characteristics (more used than 'features') change rapidly? L. 32: And how do you know that a reliable bay configuration has a high influence on model performance and outputs?

P. 15, L. 36: The results will of course vary with resolution and are normally better with higher resolution (but too high resolution can sometimes also cause instabilities). Again, this is about convergence. See also General Comments above.

P. 15 – P. 16: Can some of the results deviating from the general trend be explained by numerical instabilities? E.g. violating the CFL criterion?

P. 16, L. 25: A smooth surface? But P. 11, L. 6 mentions a sort of a numerical roughness (see comment above).

P. 16, L. 35: The influence of rockslide characteristics on tsunami genesis is discussed in several papers.

P. 18. L. 38: How well suited in hazard analysis is a model that is so computationally costly? Uncertainties are normally treated by running a large number of scenarios.

Figure 14: Wave run-up seems to be diverging with mesh refinement. This deserves some discussion.

Overall Recommendation

My recommendation is **Rejection**.

References

I don't think Braathen et al. (2004) is the best single reference for the 1934 Tafjord event.

FLOW-3D User manual: Link goes not to the manual.