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Discussions

# *Interactive comment on* "Lituya Bay 1958 Tsunami – detailed pre-event bathymetry reconstruction and 3D-numerical modelling utilizing the CFD software Flow-3D" *by* Andrea Franco et al.

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The present authors' comment, referring to the discussion paper titled "Lituya Bay 1958 Tsunami – detailed pre-event bathymetry reconstruction and 3D-numerical modelling utilizing the CFD software Flow-3D", is aimed at the comment of anonymous referee #2, published on 20 November 2019.

Dear referee, thank you very much for the time spent in reviewing our work and for the very good advices to further improve the manuscript. We will take care of your comments and provide improvements as suggested. The authors comment on the referee advices as follows:



(1) Referee: 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.

Authors: In the first pages of the manuscript the idea was to summarize the work that has been done by now on the Lituya Bay tsunami event to give a general overview (without going too much into details) and to give the possibility to the reader to refer directly to the specific previous works on this topic. During manuscript revision, we will cite better this first part of the manuscript in order to provide a shorter introduction to the reader without loss of the most relevant information.

(2) Referee: 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?

Authors: In this work the focus is set mainly on the wave dynamics (generation, propagation and inundation processes). Concerning the sliding fluid (the "denser fluid") approximating the rockslide at the bay head, the intention is to apply a simplified modelling concept which initiates the wave process in the way it was observed during the event. The simplified concept has to be applied here since there are modelling limitations of multi-hazards (hydraulic processes and gravitational hazards) within one software application. The reproduction of the physics of the rockslide (rheology) is not the focus of this work. To be consistent with the terminology and the adopted (simplified) model we will refer to the "sliding fluid" (as a general concept) and not to the "rockslide". In the case of the Lituya Bay, we can confirm that our concept worked well in initiating and reproducing the wave dynamics. We will clarify this aspect of our work to make sure that the purpose is clearly understandable and to avoid other expectation from the reader. Concerning the terminology in the manuscript, reference is made also to the authors comment on the review of referee#1 (issue (7)).

(3) Referee: 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 m 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.

Authors: We show the results of simulations with different grid resolutions to highlight the difference in results and to investigate how the hydraulics are affected by the adopted mesh size. We state that the resolution of 15x15x10 m is the one that best (not perfectly) reproduces the observed trimline within the computational limitations on a standard work station. With regard to the general complexity of the modelled processes and the involved uncertainties this quality of reproducing the observed processes is sufficient in our opinion, also in comparison to available previous works. At the time it has not been possible to simulate a model with a finer mesh due to the computational limitations, so it has not been possible to provide a more substantial analysis of convergence related to the size of the mesh cells. To verify improvements in results in function of the grid resolution, we will consider running further simulations with a finer resolution with the support of a more powerful machine. In this way we try to better quantify the convergence rate in the revised manuscript. For this purpose, we will focus not only on the run-up values or in the resulting trimline where 3D topographic effect can be very influential but additionally also on the wave features (it has been already noticed closer values in flow height for the resolution of 20x20x10 m and 15x15x10 m, but we will check better and provide better explanations). Any new findings we will considered in the revised manuscript, hopefully giving them more value and reliability.

(4) Referee: Some phrases are repeated several times (as e.g. the 524 m), possi-



bly indicating that the structure of the paper is not optimal. . . . The linguistics of the manuscript should be improved (not further detailed below).

Authors: Thank you for this notice. During revision, we will carefully check the manuscript for repeated information and adapt accordingly. Further we will improve language accuracy. Concerning the structure of the manuscript and suggestions for improvement reference is made to issue (8).

(5) Referee: 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 definitions of terms like rock fall, rock avalanche, rockslide etc.

Authors: Thank you for this advice, we will check and provide changes in the terminology. Concerning the terminology in the manuscript reference is made also to the authors comment on the review of referee#1 (issue (7)).

(6) Referee: p2, l20: 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?

Authors: Yes, here only works related to the Lituya Bay case study are discussed. During manuscript revision, we will carefully check the literature again and consider adding some more references in this context.

(7) Referee: p2, l30: I do not agree that the questions listed here are all "open questions". Much work has already been done to answer them.

Authors: We fully agree with this consideration. We will rephrase this sentence specifying that we want to give a further contribution to these research questions which have been raised and discussed within previous studies and which are relevant both for basic research and practice in multi hazards risk assessment.

(8) Referee: Would it be better to switch sections 2.1 and 2.2?

Authors: Yes, for the reader it is probably clearer to get information on the case study characteristics first and a summary of the hazard event subsequently. We will consider this suggestion.

(9) Referee: p4, l38: Better use 'head of the bay' rather than 'end of the bay'? At least be consistent throughout the text.

Authors: In order to be consistent in terminology in the whole manuscript (compare issue (5)), we will always use "head of the bay" in this context as suggested.

(10) Referee: p5, l12: What is the difference between physical scale tests and empirical studies? It seems like the terms are mixed further down (e.g. in p5, l34 and p6, l1 are mentioned experiments under the heading 'Empirical studies')

Authors: Since we are going to summarize the first part of the manuscript and make it shorter, we will adapt the section on the referred past works and thereby consider this issue. It is not always clear how to classify previous works in this context since empirical equations are often a result of experiments and related analyses. During revision and shorting of the discussion of previous works we will merge the sections 2.3.1 and 2.3.2.

(11) Referee: 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.

Authors: As we mention before, we want to give a general overview of the previous work on Lituya bay without entering too much in the details. To be consistent with the request to summarize the first 6 pages (see issue (1)) we suggest no to provide further details on previous studies but we will consider better discussing the importance of numerical set-up and methods used (e.g. nonlinearity and dispersion).

(12) Referee: Section 3.1 might represent a valuable contribution, but is hard to follow.

Authors: In recreating the topography and the pre-event bathymetry we want to sum-



marize the descriptions and available information provided in previous works and, based on that, describe the processing of the pre-event terrain in our study. We will restructure and rephrase this section to make it clearer.

(13) Referee: p7, l33: Volume is 3x108 m3. p4, l13 and p7, l40 say 30x106 m3. Please comment on this.

Authors: As described in Ward and Day (2010), 3x108 m3 is the total infill of the bay after the tsunami event, that included the rockslide material plus additional material coming from other sources (soil, subsoil from the inland, deltas, under glacier sediments etc.). The volume of 30x106 m3 is the one that has been estimated for the rockslide only (Miller 1960). Even this information is already provided in the manuscript, we will take into consideration to make some improvements to avoid misunderstandings.

(14) Referee: 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 p12 do not suffice either. p9: Much of the discussion is on turbulence and density, while slide-rheology is not mentioned at all. See also General Comments above.

Authors: We will consider adding the basic equations adopted in Flow-3D (e.g. RANS and turbulence model) for a better understanding of the computational process. Additionally, we add more details on the density evaluation model. As mentioned previously, since it is not in our interest to recreate the rockslide physics, the rockslide rheology is thus not discussed.

(15) Referee: p9, l16: "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 l3.

Authors: Thank you for this note, we will consider to skip this sentence.

(16) Referee: p10, l2: Cell size (relative to wave length and relative to temporal grid increment) is more important than number of cells.

Authors: To show that we set our preliminary models on the base of the work of Basu et al (2010), we provide the same kind of information they provide (the number of cells for each axis) rather than the cell size. Anyway, it is of course more informative here to specify cell size in the three directions of the orthogonal mesh. We will add this accordingly.

(17) Referee: p10, l35: Why outflow boundary conditions here? Why not accept reflections (from steep/closed boundaries)?

Authors: Reflections are not considered to happen at the boundary locations of the domain. The outflow boundary condition is set to allow the wave to exit the domain as it is supposed to be on the floodplain and as well at La Chaussee Spit. Reflections at the steep slopes around the bay are already due to the topographic effect and thus at these locations the mesh block boundaries are not relevant.

(18) Referee: p11, l6: 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.

Authors: The computational surface of the considered solid bodies, which are implemented in Flow-3D as stl-files, are generated by use of the FAVOR-method during preprocessing. Based on the characteristics of the applied orthogonal mesh this computational surface is slightly differing from the smooth surface as it is composed for this work by NURBS surface in Rhinoceros 6. It features a slightly rougher surface (staircase structure) which is treated as one component of the total surface roughness in Flow-3d. Secondly an additional parameter (equivalent roughness height) can be at-



tributed to the surface components in Flow-3D. For further comments on the roughness in Flow-3D reference is made to issue (29) and the authors comment on the review of referee#1 (issue (4)).

(19) Referee: p11, l23: 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 ($v \sim \mathrm{sqrt}(2^*g^*H)$). 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.

Authors: We thank you very much for this important note. It is obvious that, according to basics in mechanics, end velocity of an obstacle sliding on an inclined plane is only related to the difference in height as long as friction is not active. There is no influence of mass or density. For the case that friction is additionally considered this force acts against flow direction along the flow path. However, the difference in length of the slopes with different slope angles and a given difference in height and as well the different processing of the computational surface of these slopes is marginal in our case study and would not explain that end velocities decrease with increasing slope angle. Based on your valuable comment we did further simulations to better analyze the process characteristics we discuss in the manuscript. We could point out that, in addition to the principles in mechanics mentioned above, two basic aspects are relevant for the impact velocities at different slopes: - In all simulations the difference in heights is equal and already discussed in your comment. However, we did the geometrical set-up in the way that this difference is measured from the sea level to the upper edge of the sliding fluid at the initial position. To maintain the same volume and shape it means that the center of gravity and as well the lower edge of the sliding fluid is at different heights for the different slope situations. - The denser fluid does not act as a non-deformable obstacle during the sliding process along the slope. This deformation of the fluid has a substantial influence on the impact velocity. We apologize for this imprecise explanation in the manuscript. In the revised manuscript we will firstly describe the specific geometrical condition of every simulation clearer. Secondly, we will provide a plot showing the temporal distribution of the impact velocity for the sliding fluid for every simulation and compare with assumed values by use of empirical equations and with data from literature.

(20) Referee: p12, l7: How can you compare your 3D results with the 2D experimental studies? See also statement p17, l14.

Authors: We did not use results from previous works to calibrate or rather validate our model output, but just to compare our quality of the results related to the observations with those from other studies, despite the different approach adopted and in order to have a better understating of the applicability of the modelling approach.

(21) Referee: p13: Time intervals refer to time from release, while text all the time describes seconds after impact. This is confusing. What do the x0 values refer to? (also on p14)

Authors: We refer to the simulation time to make the connection with the images easier, and we use in the text the time from the "impact" to better describe the wave process. We will consider improvements to make it more consistent. x0 is a referred origin coordinate to express the position and the distance of the gauges (history probes). In the impact area it refers to the impact point of the sliding body at the shoreline. For the whole bay it is located at the shoreline in front of the Cascade Glacier. We will show the location of x0 in fig. 6.

(22) Referee: p13, l17: Is the velocity the same as wave celerity or speed of wave propagation? And if so, how is that quantified?

Authors: In this work we do not refer to the wave celerity, but to the fluid velocity, so it is not quantified. We will add more information about the wave propagation starting from



the data recorded by the gauges.

(23) Referee: p13, l24: Is flow height relative to terrain? If so, normally referred to as flow depth.

Authors: Thank you for this note. We will consider this during revising the used terminology in the whole manuscript (see also issues (5) and (9)).

(24) Referee: p13, l29: 54 seconds = (34+12+8) seconds from release (not from impact)?

Authors: Thank for your good attention, this duration is related to the release event. We will correct this mistake.

(25) Referee: p14, l28: 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.

Authors: Thank you for this note. We will revise the explanation here describing the decrease in flow velocity due of the attenuation process of the wave itself where the bay floor increase its depth, while on the other side (north to the island) the wave acceleration is due to a breaking process because of the lower bay floor depth.

(26) Referee: p15, l23: Is the rockslide considered to be a turbidity current?

Authors: In this section we present the propagation of the sliding fluid on the bay floor as an application available in Flow-3D to observe the mixture process between two fluids with different density, an application that can be adopted also to observe natural phenomena. We agree that is not properly correct to state that we are observing the propagation of the slide material along the bay floor, but actually a mixture process of the denser fluid approach. We will clarify this aspect in the revised manuscript and we will consider to be consistent with terminology where the term "rockslide" will be used when referring to literature and the description of the observed event, and the terms "denser fluid" and "sliding fluid" when referring to the simplified modelling concept of the current work.

(27) Referee: p15, l26: Why is a high-quality reconstruction of the bathymetry more important where the wave characteristics (more used than 'features') change rapidly? l32: And how do you know that a reliable bay configuration has a high influence on model performance and outputs?

Authors: Thank you very much for this note. We assumed here that it is most important to provide high-quality topography and bathymetry in the impact area since this is the location of wave generation. However, it is obviously correct, that with our results this assumption cannot be proven. We will consider this note during revision of the manuscript in the way that we highlight the general need for appropriate pre-event information of the terrain and, more focusing on the wave features during propagation, especially in areas of lower water depths and interaction with the surrounding terrain. Further numerical analyses on the influence of the quality of topography and bathymetry are out of scope for the present article.

(28) Referee: p15, l36: 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.

Authors: As previously mentioned, we will provide further simulations with a finer mesh to better quantify the convergence rate and to observe if any instability during the calculation process is present. See also issue (3).

(29) Referee: p15-16: Can some of the results deviating from the general trend be explained by numerical instabilities? E.g. violating the CFL criterion?

Authors: In all accomplished simulations numerical instabilities or indications for it were not observed. This is of course related to the applied computational meshes. With regard to sensitivity analyses (see issue (3)) simulations with an even finer computational mesh are considered in the revised manuscript and any potentially occurring numerical



problems will be discussed. As far as we know, the CFL-criterion, which is basically important when solving the Saint-Venant-equations (1D and 2D), is not considered in 3D hydrodynamic computations.

(30) Referee: p16, l25: A smooth surface? But p11, l6 mentions a sort of a numerical roughness (see comment above).

Authors: Here we refer to a zero-value for the roughness height (equivalent grain roughness). So, for these conditions there is a certain form roughness which is related to the processing of the topography with the FAVOR-method (depending on the size of the mesh cells) present and no further additional roughness. We will better describe this aspect.

(31) Referee: p16, l35: The influence of rockslide characteristics on tsunami genesis is discussed in several papers.

Authors: We will rephrase this sentence, where we will state our contribution to this aspect (regardless of the applied modelling approach).

(32) Referee: p18, l38: 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.

Authors: By considering a numerical modelling approach for a very complex topic (multi hazards event) we don′t think that this can be evaluated as computationally highly costly since the simulations finish in terms of days on a “standard” work station (see issue (5) of the authors' comment to referee#1 for specification) which is in our opinion still acceptable. So, we support this as a possible approach for hazard analysis (even if it takes longer than valid empirical approaches). We fully agree that uncertainties have to be treated by running several scenarios, but this is basically even more relevant on the course of forward-oriented indications (e.g. analysis of potential future hazards) compared to the reconstruction of a historic event where observation data for model calibration is available.

(33) Referee: fig. 14: Wave run-up seems to be diverging with mesh refinement. This deserves some discussion.

Authors: This is explained considering the 3D effect of the topography and considering from which direction the wave approaches and runs upon the topographic surface (from the front in case of section A-A′ and from the side in case of section B-B′ as shown in the attached figure 2)

SEE ATTACHED FIGURE 2

Attached Fig. 2: Sections where the two maximum run up heights are calculated. The light blue arrows show the direction of the wave flowing upon the topographic surface.

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

Authors: We will include further references in this context, as for instance:

Holmsen, G. (1936): De siste bergskred i Tafjord og Loen, Norge. Svensk geografisk Arbok 1936, Lunds Universitet, Geografiska Institutionen Meddelande, 124, 171-190.

Furseth, A. (1985): Dommedagsfjellet - Tafjord 1934. Gyldendal Norsk Forlag A/S.

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**Fig. 1.** Sections where the two maximum run up heights are calculated. The light blue arrows show the direction of the wave flowing upon the topographic surface.