Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-225-RC1, 2019

Natural Hazards and Earth System Sciences
Open Access
Discussions

Interactive comment on "La Palma landslide tsunami: computation of the tsunami source with a calibrated multi-fluid Navier–Stokes model and wave impact assessment with propagation models of different types" *by* Stéphane Abadie et al.

Anonymous Referee #1

Received and published: 22 August 2019

This paper reports on important information for tsunami hazard assessment for countries with exposed costs located or connected to the Atlantic Ocean Basin. As stated by the authors, also a Cumbre Vieja Volcanic (CVV) collapse represents an extreme source of likely very low probability, it must nevertheless be considered in a comprehensive tsunami hazard assessment study. For this reason, this paper eventually warrants publication in a journal such as NHESS, after some of the concerns detailed below are addressed.

Review comments

In introduction, the paper lists some earlier work on CVV tsunami modeling and far-field impact but does not really quantitatively summarize all the important findings. Some references also are missing, for instance Grilli et al. (2016).

In introduction, the paper should better discuss why various propagation models are used to simulate the same cases (for instance to assess epistemic uncertainty). It seems that models were used only based on their availability or the experience within a certain group and not because they really featured the necessary physics. As early as Løvholt et al. (2008), it was made clear that CVV collapses generate highly dispersive wavetrains. This was also reported in the far-field by Tehranirad et al. (2015). Here, both dispersive and non-dispersive models are used and although some of the results presented (particularly in and around Guadeloupe) clearly show significant dispersive effects that in a standard way reduce the leading wave elevation and enhance that of the second and/or third waves and then shed a larger tail of oscillation, the conclusion is more or less that all models are acceptable to simulate this event, as this were a forgone conclusion ?

Also, the conclusions wrongly imply that dispersive effects only express themselves early in the event, during the THETIS and FUNWAVE near-field simulations which are both dispersive, and hence it is fine using a non-dispersive model for the transoceanic propagation. This contrary to established effects of dispersion, which a train of short enough waves, will go growing with time and distance. See for instance the discussion and dispersion indicators (which could be computed here) detailed in Glimsdal et al. (2013), and also applied in Schambach et al (2019) for landslide tsunamis. In this reviewer's opinion, the results presented here would lead to conclude using TELEMAC-2D or SCHISM for anything other then the nearshore inundation should be ruled out. See for instance Grilli et al. (2016) for which the transoceanic propagation of the CVV tsunami was done with FUNWAVE and the local impact on Hispaniola was computed with TELEMAC-2D in an unstructured grid.

Here the authors present new, more realistic, CVV sources, but the comparison between the old sources and the new ones is quite limited. More details would be desirable for both the wave generation and the slide. To begin with, the initial topography and geometry used for each source (both old and new ones and all the considered volumes), such as shown in Fig. 1, should be better described (is it fully available on some data storage site ?). Then, it would be desirable to clearly show stages of the slide development underwater, for the old inviscid source and the new ones. In this respect, Figs. 8 and 9 are hard to get any information from. Having both comparative cross section and planview of slide flow and runout as a function of time would be desirable. For the generated tsunami, only Fig. 10 shows some measure of comparison but there as well more comparison allowing the reader to understand the nature of the tsunami source and how it is affected by viscosity would be helpful. P5 l8, it is mentioned the landslide continues to move for a very long time as in Abadie et al (2012) but this should be illustrated in a figure.

There is no real review of other models that have been used for simulating subaerial or submarine landslide tsunamis, for dense fluid or granular mediums. I suggest for instance to refer to and at least briefly discuss models of Horrillo et al. (2013), Ma et al. (2015) and Kirby et al. (2016) and Løvholt et al. (2017). See also the recent application of the Ma et al. and Kirby et al. models to a volcanic collapse tsunami in Grilli et al. (2019).

Model grids are not clearly enough defined. Better (more readable, less dark) figures of grids on a global map, plus some regional zoom outs, and tables of parameters are required. Lines 22-29 p 5 are too implicit. For instance, for SCHISM ii t is said that "the resolution is adapted to accurately reproduce wavetrains of 2 minutes or more with at least 20 nodes per wavelength in deep ocean". Is this applicable to all 4 models ? how was this decided about ?

Please explicitly explain models and grid used and how. L32 p5, saying all models use the same basic equations is meaningless. Please specify common physical assumptions and differences among models. Listing eqs. for 3 out of 4 of the models without

background is meaningless. Equations are not necessary here but model physics, features and limitations are. If the authors do want to list eqs., they should do it for all models and in appendix. The end of section 2.3 would be a good discuss anticipated dispersive effects based on earlier work on landslide tsunamis and on CVV.

Regarding Calypso, saying that it switches between NSW and Boussinesq equations if "the area of simulation shows dispersive effects" is not sufficient. Please explain how this is done (on the basis of which criterion). For Telemac-2D and SCISM, eqs. are given but it is not clear how these are expressed in spherical coordinates (which is necessary for transoceanic propagation).

More details of the mu(I) rheology should be added (e.g., top of p4) as many readers may not be familiar with it. Also, the use of mu(I) appears more as an afterthought and it should be clearly said from the onset that 2 rheologies (Newtonian and mu(I)) were simulated for each source (say on p3 line 23).

Section 2.5 should be more about "Tsunami impact (or hazard) assessment". Hazard can be expressed as a function of runup, but also flow depth (maximum surface elevation), velocity, momentum force, etc... Regarding runup, the use of the analytical solution for a plane slope in areas of coarse grid seems far-fetched. As stated later in the paper, focusing-defocusing and friction effects, will dramatically change the runup estimated this way. I suggest eliminating this part entirely from the paper as it seems to this reviewer to bias the tsunami hazard assessment for CVV. Uniformly computing and comparing flow depth for some isobath (even at the shore) would be more meaningful.

Regarding friction, if this adjusted as a function of land use for some areas (e.g., Guadeloupe), this should be documented in the paper with a map of land use and a table of corresponding friction coefficient.

About slide results, l14 p10, as these are discussed in the text, slide cross- and along-sections should be provided in figures, plus runout contours for the various rheology cases so they can be compared.

Sentence on l24 p10 is fully implicit. Instead, one should explicitly discuss slide results and related wave generation, for each scenario, and then point out that wave elevations significantly vary, etc... Note the t=5 min (300s) is not a case in Fig. 11. Other figs are needed as discussed before. Saying l18 that MSL drastically lowers in the inviscid case as compared to the present viscous cases should be shown on a figure.

Fig. 12 (which by the way is highly distorted ?) shown in filter could be discussed in previous section when explicitly presenting results for slide and waves for each volume scenario.

L10 p11, again an example of an important discussion that should both be explicit and expanded. Say that Fig. 14 clearly shows well developed wavetrains illustrative of the high dispersive effects.

Some results cannot really be see on some figures which are poor in terms of color shade/scheme, brightness, etc.. Fig. 15 for instance does not allow seeing some "waves" (or amplitudes ? or surface elevations ?) are larger than 5 m as stated L20 p11. Fig. 17 is another one with poor color scheme. Also make sure that all the names of localities listed L25-31 p11 are marked on the figure in a visible manner (most are in Fig. 6), with a separate table providing their geographic coordinates. Table 1 caption is not explicit as of where gauges 1-6 are and what they are; it seems to be stations n Fig. 7, but then caption for each table/figure should refer to the other one.

L6-7 p 12 not only is a weak discussion regarding the SCHISM-FUNWAVE comparison in Fig 19 but is not quite right based on results. Here is a case where dispersive effects for long distance propagation of an initially identical train of (quite dispersive waves) are very well illustrated with dispersion causing a marked redistribution of the elevation/energy of the leading wave onto the 2nd and/or 3rd-4th waves. SCISM likely has some numerical dispersion that causes high frequency oscillations in the tail of the wavetrain compared to FUNWAVE. An energy spectrum would also be useful here in comparing where energy is located as a function of frequency. Differences between

model results in the tail also results from different wetting-drying algorithms schemes at the shore and dissipation in breaking waves that may cause vastly different reflected wave trains that interact with the incident wavetrain in the tail. FUNWAVE-TVD in particular features an improved shoreline algorithm following results of a benchmarking workshop. What is the status of the 3 other models in this respect ? None of this is properly discussed anywhere in the paper. In any case, this reviewer's opinion is that results in Fig. 19 show SCHISM is not applicable (or an appropriate) model for the transoceanic propagation of CVV wavetrains. Other results shown later confirm this opinion. A more adequate modeling, if SCHISM needs to be used nearshore, would have been to nest a SCHISM grid (around Guadeloupe or similar), within the ocean basin grid in which waves would have been simulated using a dispersive model.

Likewise, with Telemac-2D, which is non-dispersive, it is fully expected to see large discrepancies (as mentioned in L20 p12) of results with those of Calypso and FUNWAVE-TVD) in Figs. 21-24. Clearly the same conclusion as for SCISM applies to Telemac-2D, which should only be used in a nearshore nested domain to compute onshore inundation, but is not appropriate for the long distance propagation of dispersive wavetrains.

Statement in L26-28 p 12 is not apparent to this reviewer, unlike stated in the text. Be more explicit or use better support. L29- discussion of gauge 3; as indicated before, model results are also influenced by differences and implementation of breaking dissipation and wetting-drying shoreline algorithms (affecting reflection), and also open boundary conditions/sponge layers if any (not discussed here) that, if insufficient) may reflect some waves back into the domain.

L1-2 p13 is another case where a Fourier transform would help clarifying results and their interpretation.

In section 3.4, the runup discussion includes many coarse grid areas where comparing flow depth at the shore would have been sufficient. This reviewer's believes the analytical runup computation and discussion in 3.4.1 does not really apply or is useful here. I suggest removing section 3.4.1. Another argument in support of the latter is statement on L9 p14 that the analytical formula overpredicts results due to lack of friction that "drastically influence(s) inundation patterns".

L12-13 p14, this work is tsunami hazard assessment not risk. The paper uses those terms alternately as if they were synonymous. They are not.

L24 p14. How can the extreme 450 km3 scenario be entirely ruled out by a simple statement and how is 80 km3 more believable. Day (personal communication) still thinks this is a potential collapse scenario should a large enough earthquake/eruption affect the island.

L5-25 p15, clearly for reasons of their own and certainly not based on results, the authors are trying to claim that all 4 models have equal skills in predicting the far-field impact of the CFF tsunami. This is clearly not the case and this discussion is quite moot. For this reviewer it is clear from results that SCHISM and Telemac 2D are not proper model to compute the propagation of the CVV tsunamis (eg see Figs 19, 21-24).

Differences between model results are not only due to resolution (L12 p15), but besides dispersive physics, also to the shoreline and breaking algorithms. I strongly disagree with the discussion of L20-23 p15 and conclusions of L13-15 p16 and L16-20 (see Figs. 19, 21-24). These conclusions are misleading and should not be a recommendation for the modeling community to adopt. Also, please compute the dispersion index of Glimsdal et al (2013) and see for yourself.

Editorial comments

Inconsistency of terminology in the text to refer to surface elevation, such as wave, wave amplitude, height, tsunami amplitude, etc. . . I suggest using surface (or maximum surface) elevation in the ocean up to the coast, then flow depth at the coast, and runup onshore. It should be also clearly defined what hazard and risk (i.e., exposure to hazard) mean and that these terms are not interchangeable. Use of a term indicating

return period should be uniform. The paper uses frequency or probability or probable or more frequent, source, etc. . .. This reviewer suggest long or short return period events.

There are many acronyms in the text, some of these not defined (e.g., SHOM). They should all be defined the first time they are used and I suggest adding a "table of acronyms" at the end. Note, accepted acronym for Nonlinear Shallow Water eqs. is usually NSW.

Figures have inconsistent format and color schemes and also some are difficult too read (i.e., very dark color scheme etc..). This is likely due to different modelers producing different figures with different software but it would really be important to improve the figures as they report the key findings of the work. Using a single software and color scheme to produce figures would be important.

There are some improper use in the text of English words and grammar that would warrant a final editing by a native speaker. Some rewordings are suggested below.

Fig. 3 caption, please replace eta by mu.

Figs. 4 and 5 (and also Fig. 6,7): use brighter more discriminant color scheme, also show a measure of bathymetry (contours). Label grids and give a table with grid characteristics.

Figs 8-9 are more qualitative than quantitative. Actual cross-sections (vertical and horizontal) should be shown with a metric scale.

Color scheme in Figs. 10-14 should also be used in following figures showing surface elevation rather than the red-pink/blue or blue/gray schemes. Also, in general, color scheme should be selected to make figures more readable in terms of having nicely spread out colors which does not occur in most figures which are based on the maximum value. Fig. 17 is a good example of this where one can't really see much of the tsunami elevation as the scheme goes to too high a maximum. Fig 17 is another example of a difficult to read color scheme in the low values

Figs. 18,19: make figure more readable by removing the first 5h or so where nothing happens, and moving legend to caption.

This is the case in Fig. 15 where colors should be selected based on making the far-field impact more visible rather than having it in more or less a uniform color. Panel on the left is unreadable in this respect. Caption of Fig. 15 says 2.7 km resolution. Aren't these spherical coordinate grids for transoceanic propagation. This is not clear since grids are not clearly defined

Additional references

Glimsdal, S., Pedersen, G. K., Harbitz, C. B., & Løvholt, F. (2013). Dispersion of tsunamis: Does it really matter? Natural Hazards and Earth System Sciences, 13, 1507–1526. https://doi.org/10.5194/nhess-13-1507-2013.

Grilli, S.T., Grilli A.R., David, E. and C. Coulet 2016. Tsunami Hazard Assessment along the North Shore of Hispaniola from far- and near-field Atlantic sources. Natural Hazards, 82(2), 777-810, doi: 10.1007/s11069-016-2218-z Grilli S.T., D.R. Tappin, S. Carey, S.F.L. Watt, S.N. Ward, A.R. Grilli, S.L. Engwell, C. Zhang, J.T. Kirby, L. Schambach and M. Muin 2019. Modelling of the tsunami from the December 22, 2018 lateral collapse of Anak Krakatau volcano in the Sunda Straits, Indonesia, Scientific Reports, 9, 11946 (open access) doi:10.1038/s41598-019-48327-6.

Horrillo, J., Wood, A., Kim, G. B., & Parambath, A. 2013. A simplified 3âĂŘD NavierâĂŘStokes numerical model for landslideâĂŘtsunami: Application to the Gulf of Mexico. Journal of Geophysical Research: Oceans, 118(12), 6934-6950.

Kirby, J. T., Shi, F., Nicolsky, D. & Misra, S 2016. The 27 April 1975 Kitimat, British Columbia, submarine landslide tsunami: a comparison of modeling approaches. Landslides 13, 1421–1434, https://doi.org/10.1007/s10346-016-0682-x.

Løvholt, F., Bondevik, S., Laberg, J. S., Kim, J., & Boylan, N. 2017. Some giant subma-

rine landslides do not produce large tsunamis. Geophysical Research Letters, 44(16), 8463-8472.

Ma, G., Kirby, J. T., Hsu, T.-J. & Shi, F 2015. A two-layer granular landslide model for tsunami wave generation: Theory and computation. Ocean Modelling 93, 40–55, https://doi.org/10.1016/j.ocemod.2015.07.012.

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,doi.org/10.1007/s00024-018-1978-7 (published online 09/03/18).

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2019-225, 2019.