Review to the paper

Hidden Hazards: the conditions that potentially enabled the mudflow disaster at Villa Santa Lucía in Chilean Patagonia

Submitted to NHESS by *Marcelo A. Somos-Valenzuela* et al.

Reviewer: *Martin Mergili*

The authors describe a cascading landslide event in the Andes of southern Chile. Besides an analysis of the general situation and of geotechnical samples, a particular focus is put on the numerical simulation of the mud flow involved in the process chain: thereby, the authors use and compare the software tools FLO-2D and r.avaflow to back-calculate the propagation of this flow-like event. The work represents an important case study, that adds another piece to the hard-to-solve puzzle of understanding complex landslide events in changing mountain areas. As such, I would like to see this paper published. Before, I recommend some **major revisions** which mainly concern the consistency between title/abstract/introduction/conclusions and the main content, and the clarity of some explanations. Please find my general and specific comments below.

General comments

1. Even though the paper is mostly written in an understandable way, there are various language issues which require substantial polishing (if possible, by a native speaker).

2. Title: the title sounds nice, but does it really describe the main content of the paper? I have the feeling that the paper is not so much about the conditions that potentially enabled the mud flow disaster, but rather on the propagation of the mud flow.

3. Abstract: in my opinion, it could be condensed and a little bit more focused, but this is an issue of preference. The general statements at the beginning are maybe too long, given that the paper mainly describes a case study.

4. Introduction: the same as for the abstract. It starts with “Climate change”, and the first three paragraphs deal with climate change-landslide relations. Even though this is certainly an important topic, it is not the subject of the paper when looking at the methods, results, discussion and conclusions. If the authors would like to stay with the focus on climate change, this aspect has to be more strongly included in the main content of the paper. Otherwise, the introduction should be restructured and reformulated, shortening the part on climate change and coming more quickly to the core topic of the paper.

Specific comments

L36: Please mention already here the region (southern Chile), many readers might not know the Yelcho mountain range.

L47 (and in general): r-avaflow -> r.avaflow

L49-50: You cannot determine the total water content from simulations and soil tests – you can just estimate it. Further, the precision given in the volume number (also in some other places) is too high, considering the uncertainties. In this case, 2.8 million m^3 would be sufficient.

L113: "… alluvial and river processes …": aren't alluvial processes also river processes?

L124 NO -> NW

Figure 2: Nice figure, but there are some hispanicisms is the legend … "Leyend" -> "legend"; "Hidrology" -> "Hydrology"; further, in the map itself the lake polygon should be deselected before exporting the map.

L136: What would be the average annual rainfall in this area?

L156: A new section about the description of the event should start here.

L157: I would expect a little bit more information on the landslide-glacier interaction. Was there some glacier ice entrained, which was included in the flow downstream? This can be an important issue, even though it is not always straightforward to analyze its importance (see e.g.: *Mergili, M., Jaboyedoff, M., Pullarello, J., Pudasaini, S.P. (2020): Back-calculation of the 2017 Piz Cengalo-Bondo landslide cascade with r.avaflow. Natural Hazards and Earth System Sciences 20: 505-520. doi:10.5194/nhess-20-505-2020*)

L175: Maybe add some brief information (1 sentence) about damages and casualties, readers might be interested in that. As the term "disaster" is included in the title of the paper, there should be at least some information on the socio-economic component.

Figs. 4-8: They are very informative, but I recommend to put them together into one full-page figure with 7 panes or so.

L273: "Topography". Further I recommend to shift the section 3.2.3 farther up, as it rather concerns data acquisition. In the place where it is now, it disturbs the flow of reading from the models to the parameterization.

Table 1: you may round the inundation area and the flow velocity at Villa Santa Lucía – the numbers indicate a precision which is probably not justified by the data.

L289: How did you perform the calibration? Did you just use an iterative optimization procedure ("trial and error"), or did you use some automated, systematic procedure? Please explain! These things are explained a little bit in Section 4 (results), but they should already be explained in the methods section.

4.3.: The heading "Numerical modelling" is misleading as, in this section, just the modelling domain and the calibration procedure are briefly described. This is something I would rather expect in the methods section, as it is not a result. Further: did you also consider simulating the entire event (including the initial landslide?) This could be an interesting task for the future and, as such, could be mentioned in the discussion. There is now the multi-phase model of Pudasaini (*Pudasaini, S.P., Mergili, M. (2019): A Multi-Phase Mass Flow Model. JGR Earth Surface. doi: 10.1029/2019JF005204*), which could also serve for the simulation of the interaction between the landslide and the glacier.

Section 4.4/Table 4: Again, some Hispanicisms (Si->Yes). Further, there is no information about entrainment. Did you allow entrainment and, if yes, which value did you set for the entrainment coefficient? The "environmental resistance coefficient" is the "ambient drag coefficient", I think. Further, the Quasi Reynolds number and mobility number are 10^4.5 and 10^3. It is the logarithms which are given in the r.avaflow input.

Fig. 12 (and some other places): flow high -> flow height

L341/342: Better: "… We varied the percentage of water between 20% and 70% …" – the formulation as it is now is misleading.

Fig. 14, legend: revise the thresholds: e.g., to which class would a flow height of 49.95 m belong?

Section 5.2.: You should also briefly mention the limitations of your calibration due to issues of equifinality (e.g. *Beven, K. (1996). 12 Equifinality and uncertainty in geomorphological modelling. In The Scientific Nature of Geomorphology: Proceedings of the 27th Binghamton Symposium in Geomorphology, Held 27–29 September 1996 (Vol. 27). John Wiley & Sons.*), and the multi-dimensional parameter space (e.g. *Saltelli, A., & Annoni, P. (2010). How to avoid a perfunctory sensitivity analysis. Environmental Modelling & Software, 25(12), 1508-1517.*).

L400: The water content leading to the empirically most adequate results was approx. 30%. Is this fraction also plausible from a physical point of view, and from the observations? Please briefly elaborate on this aspect more explicitly (some indirect information is given in the paragraph below).

L404, 405: Oh, you computed entrainment! This is good, but it is mentioned here for the first time (unless I overlooked it). You should appropriately address this important aspect also in the methods and the results sections.

L409: Now, the entrainment of glacier ice comes into play! You have to introduce this aspect already in the event description (see comment above).

L420: FLO-2D is not a freely available software.

Conclusions, second paragraph: I would rather suggest to move this text to the discussion. The conclusions should rather focus on the key messages from your work (just extend what is written in the first paragraph).

This is all from my side. If the authors disagree with the one or the other comment, or would like to discuss issues, they should feel free to contact me at martin.mergili@univie.ac.at.

With best regards

Martin Mergili