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Interactive comment on "Modeling glacial lake outburst flood process chain: the case of Lake Palcacocha and Huaraz, Peru" *by* M. A. Somos-Valenzuela et al.

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General comments:

This paper presents a modeling approach for glacier lake outburst floods in Peru, considering the multiple processes from ice avalanche flow, to lake impact, wave generation, lake dam overtopping, erosional process at the moraine dam and flood propagation downstream, including into the Huaraz urban area. There exist currently only very few studies that have developed and applied a similar approach to such multiple cascading mass flow processes. This papers offers important progress on several aspects, is well and comprehensively written and I would like to see this paper published after revisions. In the following I list a few more general points which I think need to be

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considered, and then follow on with more specific points. Most of the points are rather minor but there are a few that have more a major character.

The coupling of different, existing (partly commercial) models to mimic the cascading processes is among the most novel and interesting aspects of this paper. The interface of the models, with corresponding output/input, however, is not always crystal clear and I made some specific comments at the respective point in the ms where I think the text has to be more clear.

Use of past events to calibrate the models: there was a GLOF in 1941 from the same lake which unfortunately has never been studied in any reasonable detail so far. The breaching process at the lake was different than what today could happen because the moraine at that time was intact. So not of much use for the lake overtopping / dam breach process but there is probably interesting information out there in terms of flood propagation and flood levels and flow characteristics down to Huaraz. I'm definitely not asking for a detailed study and comparison of this historical case, this would be way beyond the scope of this paper (also considering that the paper already is quite packed). However, I think the authors should make reference to this event and the potential to compare or calibrate the flood models applied. I recently visited the area and there are some flood deposits visible in the flow channel (in the Cojup valley) which may be used to calculate discharge per cross-section. Or they may want to use the historical photographs available to compare their inundation areas with the historical case (at least in a qualitative sense).

I note that the authors do not consider the scenario of a moraine collapse as occurred in 2003, producing a (relatively small) overtopping and downstream flood. This should be discussed.

The effect of flow transformation downstream of the lake with different flow rheologies (e.g. from debris flow to hyperconcentrated flow and back to debris flow) has not been considered but is likely to occur and probably important, e.g. for travel times. The authors should at least discuss possible effects and limitations in their model setting with respect to this process.

The uncertainties of the models and their propagation through the models is not well assessed or discussed. This should be included. The authors may want to consider a new publication on this, on the example of the Lake 513 nearby: Schaub et al., Landslides, 2016. I think the authors should make a statement concerning the robustness of their model results (especially in terms of the final inundation maps).

An important point concerning the hazard map: a hazard map should never be a direct result of a model output. Field evaluation and validation is an essential part of a hazard map. I suggest to call the map a ‘preliminary hazard map’, making reference to the importance of field evaluation (which could not be done for this paper).

Overall, the paper is well written and I really appreciate the comprehensive literature review and the methodological details which help any reader to better follow and understand. However, I think there is a bit of redundancy here and there.

Specific comments

Page 5, lines 3-8 is redundant

p. 7, l. 23: do you have evidence of increased frequency of extreme precipitation? I did not see any study on this so far.

p. 9, l. 5: I think this should be hazard rather than risk assessment

p. 10, l. 8: I suggest to explicitly state the type of avalanche p. 10, l. 28: slab failures can also be produced at larger glaciers

p. 11, l. 7ff: there is an important mis-understanding here that needs to be corrected. The formula of Huggel et al 2004 relates avalanche volume to average slope of the runout (i.e. from the point of failure to the furthest point of runout), and NOT to the slope of the failure surface/glacier!

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p. 13: the interface of RAMMS avalanche model, and FLOW-3D could be described somewhat more explicitly.

p. 14, l. 2: I suggest to change conservative risk perspective into worst-case approach

p. 16: the interface of FLO-3D and BASEMENT should be described more clearly and in terms of the calibrated parameters. Also, where exactly is BASEMENT started? I think an additional table with the parameters could help.

p. 18, 18: Actually, not many models are currently capable of simulating entrainment processes, most examples mentioned are not.

p. 20, l. 7: the area reduction factor could probably also be higher than 20%, considering the building density in Huaraz.

p. 20, l. 23ff: according to Table 2, the intensity matrix for floods and not for debris flows (of the Swiss system) is applied. The model simulates debris flow, so the debris flow intensity levels may be more appropriate.

p. 21, l. 17ff: I see a need to extend how hazard zones were mapped. As mentioned above, a direct conversion of model output to a hazard map is not appropriate (preliminary hazard map may be more appropriate here).

p.22/23: I suggest to include the results of the comparison with the Heller and Hager model in Table 4. This is of interest.

p. 24: I found the evaluation of different lake lowering scenarios particularly useful from an engineering point of view and represents a work that is hardly done.

p. 25, l. 20ff: I'm not whether failure is the best term here because it may be ambiguous in a case where a breached moraine already exists. I'd rather use full breach development, implying that the lake drains completely. Please clarify this.

p. 26, l. 27/28: Almost one hour to cross the urban area seems high to me for a GLOF. Please check whether you may need to adjust the FLO-2D model parameters for the urban areas.

p. 27, l. 1-6: I would be good to also show the arrival times for the small/medium scenarios (cf also Fig. 7).

p. 27 (4.6): The decision which scenario to eventually include in a hazard map is also a political and just a scientific question. I would explicitly mention this. To me, the approach taken seems reasonable. We have discussed this issue in a workshop in Huaraz (with participation of Rachel Chisolm, a co-author of this paper). There was not a clear opinion or statement on this. I think assessing the worst-case is something science should do, and its inclusion in terms of a residual hazard zone seems reasonable to me (considering that all hazard zones presented here should be labeled preliminary).

p. 28, l. 21ff: data on past events is available (ie the 1941 GLOF), at least for the downstream mass flow, and this should be discussed, as previously mentioned.

p. 29, l. 9-11: I agree that the use of a 3D model is adding value to the assessment of lake displacement waves and is likely to capture the complexity better than simpler models. However, I don't quite agree with this statement which seems to me to be overly confident with this model. Overall, there is only limited experience with this kind of model for such environments and there is substantial number of model parameters to be calibrated. I suggest to discuss the uncertainties that are related to this model.

p. 30, l. 6: I suggest to use worst-case instead of conservative approach.

p. 32, l. 10: I guess you are talking about hazards since the paper does not contain any material on risk.

Figures are of good quality and I particularly like Fig. 7. Table 8 can probably be avoided.

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