Response to Review of the manuscript "The importance of erosion for debris flow runout modelling from applications to the Swiss Alps"

submitted to **"Natural Hazards and Earth System Sciences"**

by **F. Frank, B.W. McArdell, C. Huggel and A. Vieli**

Reviewer: M. Mergili

Response to review by F. Frank and co-authors:

- Answers to and corrections proposed based on comments from reviewer Dr. Mergili
- Answers to and corrections proposed based on comments from both reviewers
- **Some major changes proposed are BOLD.**

The authors use the debris flow module of the software RAMMS to explore how considering erosion or not affects the results of debris flow runout modelling. They first calibrate their erosion model in the Illgraben catchment, before applying and validating the calibrated model with data from the Spreitgraben catchment. The manuscript is generally well written and illustrated, and is certainly interesting for the audience of the journal. I would like to place a number of suggestions which could help to further improve the manuscript. All in all, I suggest a **minor-moderate revision**.

We are grateful for the thorough and helpful review by Martin Mergili, which we think substantially improve the manuscript. The reviewer identified many issues to which we respond here. First we describe some major points common to both reviews, and then we address the general comments by Dr. Mergili, and finally the specific comments he identified.

The suggestion to move some text from the discussion to the results section was also proposed by the other reviewer. Given the sometimes contradictory suggestions of the reviewers we had to decide decision where to place those paragraphs (described in detail below).

Both reviewers requested clarifications on the changes in bed topography between the modeling of the first and second surges. We are grateful for these comments and we decided to re-do the simulations to more accurately model the erosion. The

updated results are attached, e.g. new Figs. 4 and 7. In detail, we updated the bed topography after the first surge before simulating the second surge. The main conclusions do not change, but minor changes to the text will be necessary for the final manuscript.

General comments

I have a few suggestions with regard to the structure and the clarity of the article:

1. The clarity of the description of the results has to be improved. For example, sometimes you mention that the best results were obtained with μ=0.20 (e.g., [2394, 3]) sometimes with μ=0.20 (e.g., [2394, 25]). It did not become clear to me to which settings/criteria the two different values refer. Please make this clearer. It could also enhance the readability of the paper to compile the best-fit parameters in a table (this could be done by extending Table 2).

The other reviewer also mentioned this. We therefore would write ([2394, 2]) "The most realistic model result incorporating erosion modeling…" to make clear that the setting ξ= 200 and μ=0.20 is the best-fit when conducting the standard RAMMS debris flow model calibration method (Bartelt et al., 2013) using the observed front travel times as estimated from the field data.
This statement is then different to the second setting of parameters (ξ= 200 and μ=0.40) which showed the best fit regarding the spatial erosion pattern (Fig. 4a) and the third setting of parameters (ξ= 200 and μ=0.35) which depicted the best fit regarding the cumulative erosion volume (Fig. 4b).

2. You should consider moving some portions of the Discussion (e.g., at least part of the paragraph starting at [2397, 12]) to the results section.

We prefer to leave this paragraph where it is because it refers only to the debris flow modeling just in this section and moving it would result in a more fragmented paper and it might cause some confusion with the other model results. However (based on the other reviewer comment), we would prefer to move these two discussion paragraphs ([2399, 13] to [2400, 22]) from section 6.2 to the end of section 5.2. We prefer this change because these paragraphs discuss the overall modeled erosion results (2010) presented in section 5.2 and therefore it would also help tighten the focus the main discussion of the paper..

Specific comments

2384, 21ff: Review this sentence, something seems to be wrong (it is probably the increase in rock fall activity which is related to snow melting processes, not the daily warming).

The relation of increase in rock fall activity to snow melting processes is mentioned as a hypothesis. There is no field data which would clearly support this hypothesis for the Spreitgraben case. As it is also not relevant for our entrainment approach in this paper, we propose shortening this sentence and reduce it to the pure observation as confirmed by Geotest (2010-12) and our own field observations:

A considerable increase in rockfall frequency has been observed during springtime.

2385, 9: "... most far reaching ..."; "... valley of the Hasliaare River"

We agree. Done.

2385, 22f: It cannot be the reason to use a model that it has been developed at WSL ... it is enough as justification to write that it is widely used.

We agree to make this change as suggested.

2387, Eq. 4: I suggest to use a different symbol for the slope, the one you use is too much associated with the internal friction angle.

We prefer to keep the variable "ϕ" for the slope angle because that is what is used in the the RAMMS debris flow manual (Bartelt et al., 2013). By clearly defining what we mean by the slope angle the first time we use this variable, we think that the reviewers will not be confused.

2388, Eq. 5: I do not understand how the slope can directly be used to compute a stress ... shouldn't it rather be some kind of sin, cos, or tan of the slope?

Yes, the depth-slope product is (density)*g*depth*sin(slope angle), and we used the short form which is commonly used for work in gravel-bed rivers (where the sine of the slope angle is approximately equal to the tangent of the angle which in turn is approximately equal to the slope of the channel (where the slope is in units of m/m). This is how Schürch et al. (2011) worked for their approach the gentle slopes at Illgraben. For the sake of consistency we suggest to correct Eq. 5 to "$\tau = \rho \cdot g \cdot h \cdot S$" and we propose to get rid of the same equation on the x-axis in Fig. 3 **(see new figure 3 in appendix at the end of this file).**

2389, Eq. 6: You should add a third line showing what happens when dz/dt is above 0.25 m/s. This is explained in the text, but it would enhance the readability to show it also in the equation. Further, I suggest to use variables instead of the thresholds of 1 kPa and 0.25 m/s. You mention in the text that these thresholds may be adjusted. So, it would be better just to use variables and to mention the values used rather in the text.

Thank you for pointing-out a typesetting error in equation 6 (which was mistakenly propagated into the text). We suggest adding "$\frac{dz}{dt} = -0.025 \ for \ e_t \leq e_m$" as Eq. 6b. The condition $\frac{dz}{dt} = 0$ for $e_t > e_m$ is awkward to put in the equation because it cannot be larger than e_m, so we prefer to write in the text that the erosion stops when $e_t = e_m$. To avoid adding variables which we do not change in this paper, we prefer to leave the numerical values here. Although not a part of this manuscript, initial results from other field sites suggest that these values work at steeper slopes.

2390, 9: "from the values determined ..." could be better.

We agree. Done.

2390, 24: "... entrained at a specific rate ..."

We write "... entrained at the specific rate ..."

2391, 8ff: Better remove the sentence starting with "However ...". This reasoning, in my opinion, is not completely valid as varying the parameters could further improve the insight in the importance of erosion.

We agree that this sentence can be deleted.

2391, 14: "... also works as expected ..." could be better.

We discussed this among the authors and we prefer to leave the wording as in the original manuscript.

2392, 13: "... more work needs to be done ..."

We agree with this suggestion.

2392, 14: Better start a new sentence after the reference.

We agree and propose starting the next sentence with: "This topic . . . "

2393, 14: "... hence is difficult ..."

We agree with this suggestion.

2394, 11: “... When choosing”

We agree with this suggestion.

2394, 13: What do you mean with “standard diameter” here? Please explain or reformulate.

We changed the term to “*one standard deviation less*”.

2395, 11: “... the simulation with ...”

We agree with this suggestion.

2395, 14: I have the feeling that the sentence should not end after “considered”, but that some information is missing here.

We carefully re-read this sentence and we propose changing “*considered*” to “*noted*”. That might sound clearer to readers.

2397, 25: “Different propagations ...” does not appear to me as a good formulation.

We agree and propose to instead start the sentence with “*The similar front arrival times of modelled hydrographs…*”

2398, 12: “... with a specific thickness ...”

We agree.

2398, 19f: Delete either “determine” or “assess”

We delete “*determine*” because “*assess*” is the more appropriate formulation here.

2399, 3: “... larger flow heights, larger volumes ...”

We agree.

2399, 14: “... modelled and observed erosion depths using the ...”

We agree.

2399, 25: Better: “... different from the best-fit ...”

We agree with this suggestion.

2400, 11: “... values of more than ...”

We agree.

2401, 27: The statement that incorporating erosion improves the model result is not supported by Fig. 5. For this, the observed impact area of the flow would have to be indicated in the figure.

The statement about the impact area of the flow observed by Geotest (2010) for the modeled debris flow (12 August 2010) can be found at location 2396, 24ff. The crucial observation is that the entire debris flow completely remained within the channel and that it showed no lateral bank overflow at all (Geotest, 2010). In our opinion, it is not feasible to visually represent this simple but nevertheless very crucial observation in Fig. 5 due to the lack of spatial data describing the exact impact area within the channel. This would wrongly imply that there is such a spatially accurate documentation of the impacted area based on aerial flights or similar for the area within the channel.

We therefore suggest that we additionally/instead refer to "Fig. 5 and section 6.1" at location (2401, 27) to support our final statement that "*incorporating erosion can substantially improve the prediction of the spatial runout pattern*".

Apart from the issue raised in [2401, 27], the figures are well prepared, I only have one small suggestion: in the Figs. 4 and 6, the symbols for the observation should be more different from those for the modelling (e.g., by choosing not only a different colour, but also a different type of symbol). This could further enhance the readability of the figures.

We agree to change the symbols (black dots to black line) of the observed data sets in Figure 4b **(connected to the suggestion that we use the results of our new approach, see above and figure appendix at the end of this file)**. For Figure 4a, we think that the observed data can be already distinguished quite well in the Figure. Figure 6 doesn't show any observed field data but only model results; the observed ranges are indicated in the text.

Figures appendix (new figures: 4/7 and corrected figure 3 are suggested):

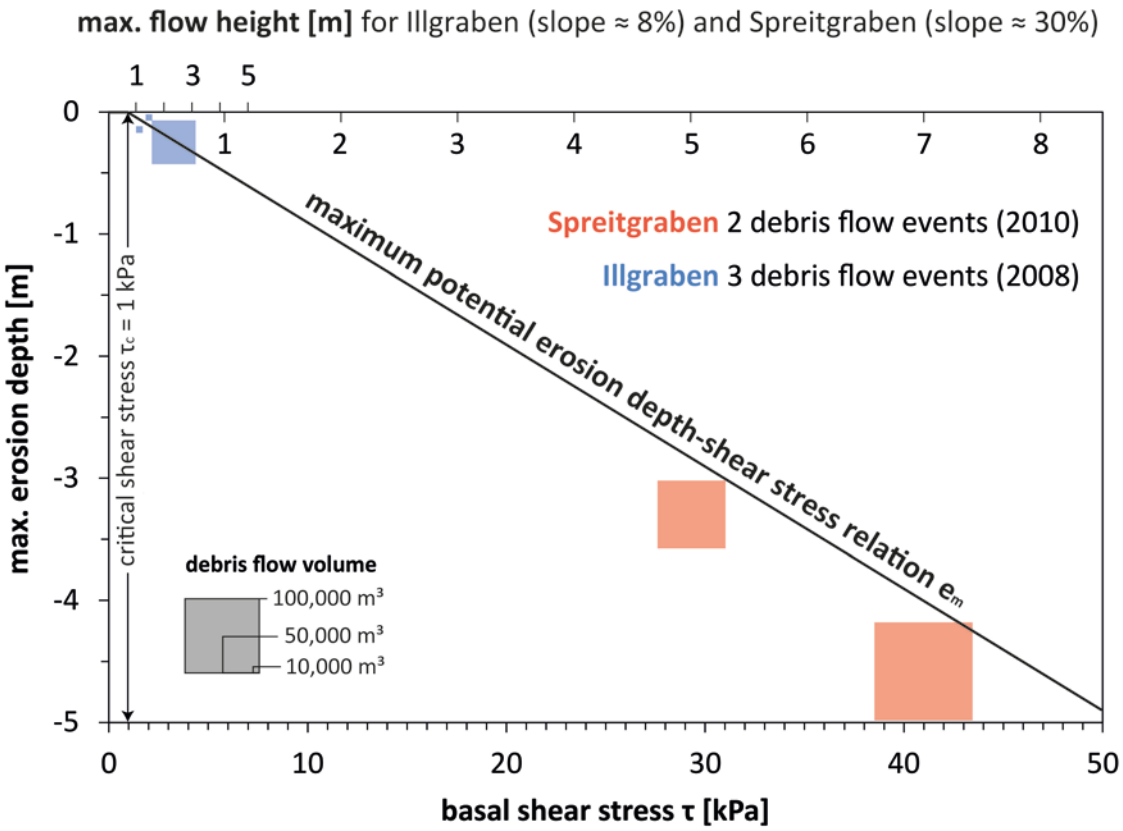


Figure 3. A linear relationship for maximum erosion depth as a function of basal shear stress forms the basis of the model. The size of the boxes is proportional to the estimated event volume at the Illgraben (3 debris-flow events, Berger et al., 2010) and Spreitgraben (2 events, Geotest AG, 2010). The upper axis indicates the flow height at the Illgraben (8% channel slope) with the numbers above the axis, and at the Spreitgraben (30% slope) with the flow depth values placed below the axis; the corresponding shear stresses (Eq. 5) are plotted at the bottom of the figure.

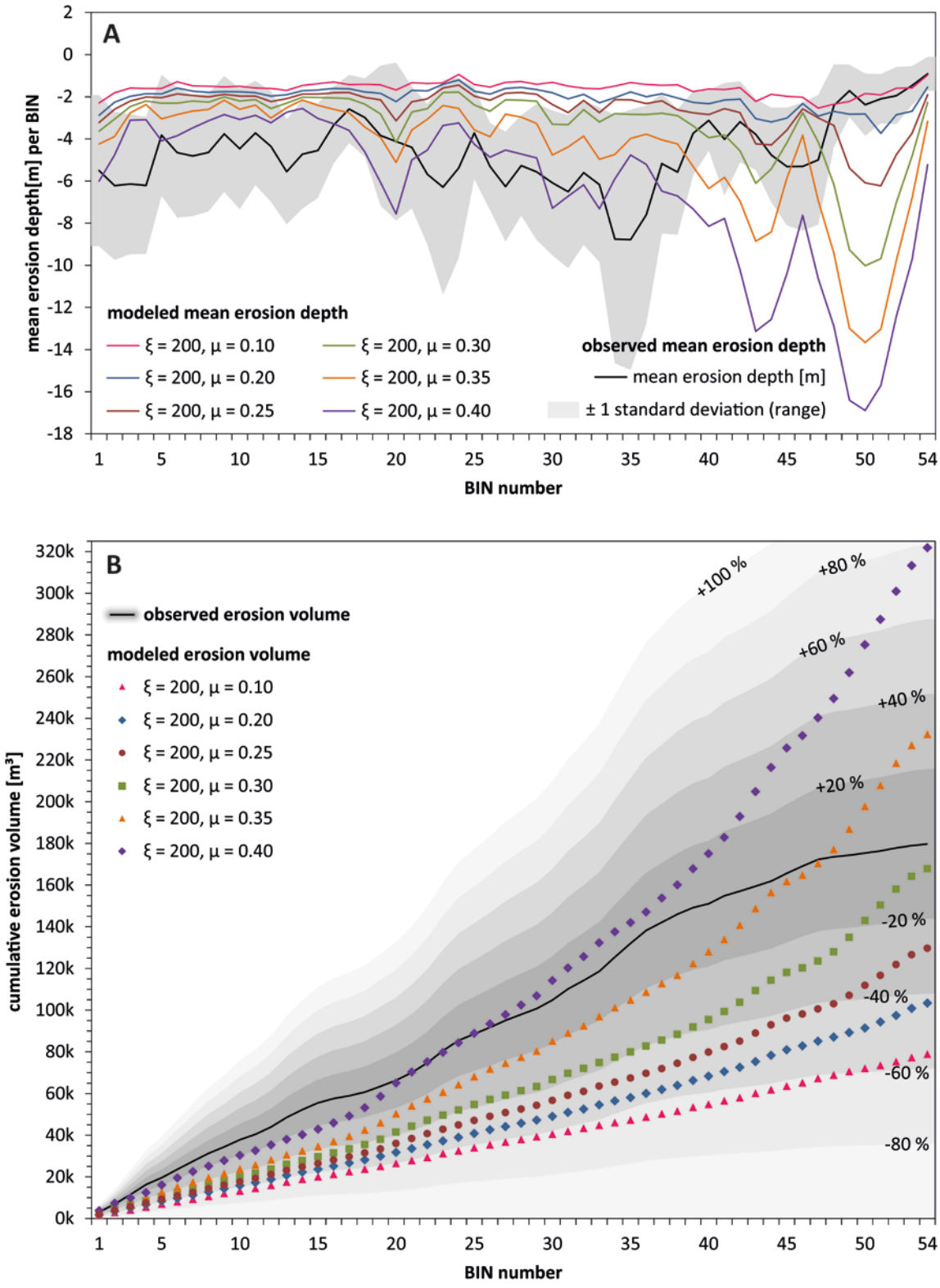
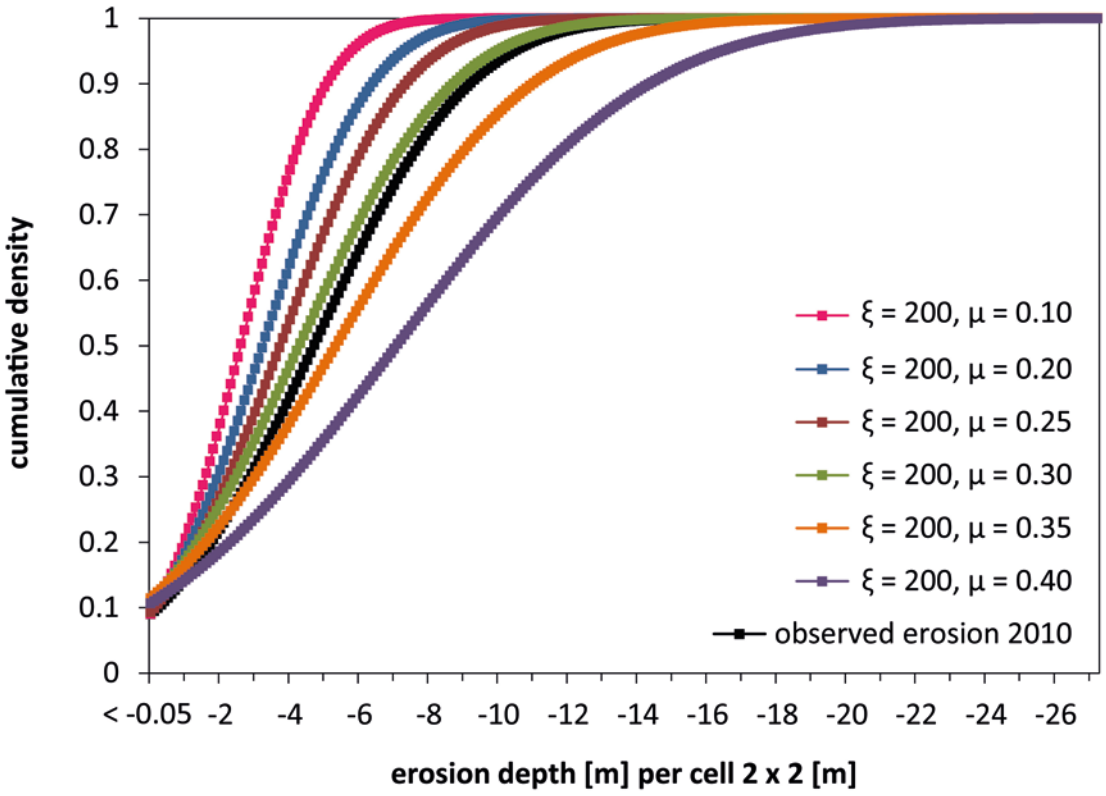


Figure 4A. Range of modelled compared to observed mean erosion depths for the two events of 2010 (Table 3). **B**. Modelled cumulative erosion volumes compared to observed cumulative erosion volumes using the bin-based systematic analysis. The gray shaded areas depict the ranges of percental volume difference compared with the observed erosion volume.



225

226 **Figure 7.** Cumulative density of modelled and observed erosion depths for the two events of 2010
227 (Table 3) based on a grid resolution of 2 m by 2 m in bins 1 to 54, for a total of 12,621 cells, using
228 the DTMs of April 2010 and August 2010 **to calculate the observed erosion 2010. To model the**
229 **second event (08/12/2010), the DTM of April 2010 was updated based on the erosion modeled**
230 **in the first event (07/23/2010).** Erosion is represented on the x-axis (< -0.05 m) while no erosion
231 cells and cells with deposition are not included.