

Response to reviews on the NHESS-manuscript:

"Modeling debris-flow runout patterns on two alpine fans with different dynamic simulation models"

Response to Reviewer #1:

My main concern is on the topographic base used in the simulations. It is not clear what Digital Elevation Model was available in the study sites. From what is written at P. 1398 L. 9-10 in the abstract, it seems that the same 1m-resolution DEM was used for both study sites whereas a grid resolution of 2 m is reported at P. 1404 L. 15-16. Some more information can be found in the discussion chapter at P. 1407 L. 8-9 (airborne LiDAR DEM dating back to 2006) where the problem of DEM representativeness is just mentioned. I think this is a very important point to be addressed since the authors are back calculating a 2009 debris-flow event and several morphological changes may have occurred in the 2006-2009 time window and a 2006 debris-flow event (the LiDAR survey was carried out before or after this event?). In my opinion, a short but comprehensive description the topographic base is needed and the assumption that the DEM is representative of the pre-event conditions should be strengthened (e.g. how many events occurred in the period 2006-2009 in the Reiselehnrinne Creek?).

REPLY: We have clarified the question of DEM and calculation grid, which we agree is an important issue in numerical modelling. The simulations performed in this study are based on DEMs with 1 m resolution. In both models we used a coarser calculation grid of 2 m to reduce calculation time and to avoid possible numerical instabilities. We also agree that the DEM representativeness, and the use of an up-to-date DEM is a very important point and we have added a short description on that. Based on these comments we have edited the corresponding text in the revised version of the manuscript.

To this end, I suggest to restructure the Methodology chapter, that could be renamed "Methodology and study area", by moving the subchapter 2.2 (Study sites) at 2.1 and add here the information on the topographic base. The current subchapter 2.1, which describes the two models, is quite long and could be reduced.

REPLY: Thank you for this comment. We have restructured the Methodology chapter according to these suggestions.

All the figures are relevant and informative with the need of just few refinements (in Figs. 1, 3, 4 and 6 a scale bar is missing; the symbols for Debris Flow DAN3D and This Study DAN3D seems more yellow than brown as written in the Figure caption).

REPLY: This has been changed in the revised version.

More specific comments are given below:

P. 1398 L. 2: "For this" -> "To this end"

REPLY: This has been changed.

P. 1402 L. 17: "..writes" -> "is as follows"

REPLY: This has been changed.

P. 1403 L. 1: you can remove yr-1 and use only mm as it is already stated this is annual Rainfall

REPLY: This has been changed.

P. 1404 L. 15-16: why did you use a 2 m grid resolution and not 1 m of the original DEM? To reduce calculation time or for other reasons? This point needs to be clarified

REPLY: Thank you for these comments. We have clarified this point above and considered these statements in the revised version of the manuscript.

P. 1405 L. 11-14: It is not clear why you choose to increase roughness to an area at the left channel bank of the transit reach to force the simulation to the real debris-flow path. I guess that the different deposition patterns are due to "the outdated DEM" (P. 1407 L. 8-9) so why not adjust the topography?

REPLY: Thank you for this comment. We agree that adjusting the topography could also be possible, to drive the simulated debris flow into a certain direction. However, we think that there are different possibilities, and we hesitated to change the topography without

any quantitative information of the situation at the time of DF occurrence. So we kept the DEM and defined an area with increased roughness that acts like a deflection dam.

Figs 3 b and d are not cited in the text

REPLY: This has been changed.

P. 1406 L. 7: "is favorable"-> "was considered"

REPLY: This has been changed.

P. 1407 L. 4-5: Consider to remove "simulation" (repetition) and use "both" instead "either"

REPLY: This has been changed.

P. 1408-9 L. 30-1: "The DAN3D code can be stopped manually or automatically after a predetermined duration. Here we manually stopped the simulation when the flow front visually came to a halt." -> this approach seems a bit subjective. What about choosing a threshold of maximum velocity below which setting the stopping condition. However, the approach you choose can be considered acceptable in light of the observations given in the following paragraph

REPLY: Thank you for this comment. We fully agree that this approach appears to be subjective. Because we have no detailed information on flow heights and flow velocities we did not determine a certain threshold of maximum velocity. Within this paragraph we want to demonstrate that the variation of the stopping criteria (see Figure 6) could also influence the simulation results when there is less information on the deposition pattern available.

P. 1411 L. 10-13: this sentence is not clear. Please consider to reformulate

REPLY: This has been changed in the revised version.

P. 1411 L. 26: "in the models"

REPLY: Changed.

In table 1 for RAMMS-DF use "Eulerian framework" to be consistent with the text

REPLY: This has been changed in the revised version.