

Response to reviews on the NHESS-manuscript:

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

Response to Reviewer #2:

Page 1404, line 10: it would be interesting to learn whether the use of hydrographs instead of block release would have a significant effect on the results. If no hydrograph based model runs were performed I suggest to include a statement in the Discussion on what has been seen elsewhere on this (i.e. in the literature).

REPLY: Thank you for your comments and suggestions.

In the RAMMS-DF model it is possible to select between an initial block release and a hydrograph. This is not the case for the DAN3D model. To keep the same initial conditions for an objective comparison of the models we used a block release for both simulation tools and we did not perform simulation runs using an input hydrograph in this study. We have edited the text to reflect this answer.

p. 1404, line 27: I suggest to generally use the term 'observed' instead of 'mapped'. I think it is clearer (or less ambiguous, because simulation results can also be mapped).

REPLY: This has been changed in the revised version.

p. 1405, eq (4) ff: indicate unit of area (m², I guess?) even though the units are eventually canceled out by the dimensionless term. I suggest to include a sentence saying that value of \hat{D}_e close to 1 indicate high observation-model correspondence.

REPLY: We have changed this in the revised version.

p. 1406, section 3.6: I suggest to define sensitivity more precisely. E.g. sensitivity of runout against μ . p. 1406, lines 9-11: In the figure captions or in the main text you should state at what values you hold the parameters constant while varying one of them.

REPLY: We have defined sensitivity more precisely and added some more information in the text and in the Figure captions.

p. 1408, line 2: I guess the values mentioned for the friction parameters do not relate to forest areas? Could be clarified because it is mentioned in the context of forest areas.

REPLY: This has been clarified in the revised version.

p. 1408, line 22ff: I'm wondering whether you don't want to mention the flow height as an additional stopping criterion. And related to this: is the larger runout for lower stopping criteria/thresholds (5, 10%) relevant in terms of flow height? For instance, if the additional runout area is overrun by flow height of e.g. 5cm this might no longer be relevant. You may explain why you think that a stopping criterion related to mass momentum is the most appropriate one. An additional question to clarify is where the reference mass momentum is measured along the flow path.

REPLY: We agree that the flow height, as an additional stopping criteria, should be mentioned in the text. This is similar to a comment made by reviewer #1. As already mentioned before, we don't have detailed information on flow heights and flow velocities during the event. We pragmatically chose 15% as stopping criteria for RAMMS-DF. By comparing our simulation results with the observed deposits we assessed our best-fit simulations. We clarified this in the revised version and provided some statements on that.

p. 1409, line 23-25: This sounds like that this effect was not analyzed even though mentioned previously?

REPLY: We performed several simulations with different values of the stopping criteria (Fig. 6). Accordingly we have adapted this point in the discussion.

p. 1411, line 9: Bartelt 2013a and 2013b should be distinguished.

REPLY: This has been changed in the revised version.

Figure 7: This figure is interesting but it may also imply some need for further clarification. The processes of the different mass flows included in this figure are obviously very different and the classification into the dimension of the two friction parameters masks this differences, at least to some degree. For instance, an ice-rock avalanche may transform into a highly mobile debris flow or hyperconcentrated flow along the flow

trajectory, and a single friction parameter value may not be appropriate. I don't see a basic problem with the figure because you just report the values from the referenced literature but I suggest to include some clarifying statements. An additional option may be to include a third dimension, e.g. with the water/sediment concentration of the flows (where available, see also Figures in Schneider et al., 2011). In any case I recommend improving the visual distinction of the grey values of the triangles.

REPLY: We have revised Figure 7 as suggested. We included the event volume as a third dimension and changed the visual distinction of the avalanche parameters. Thank you for this comment, this has improved Figure 7 significantly.