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Discussions

Interactive comment on "Back-calculation of the 2017 Piz Cengalo-Bondo landslide cascade with r.avaflow" *by* Martin Mergili et al.

Brian McArdell (Referee)

brian.mcardell@wsl.ch

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This manuscript describes the application of r.avaflow to a two-process sediment cascade involving a rock avalanche and subsequent debris flows. While the manuscript is interesting, clear and concise and is certainly of interest to users of r.avaflow, the manuscript has the character of a case study and the application of the model and concepts to other field sites is limited by the fact that the model must be calibrated for every new application. This suggests that r.avaflow is perhaps only useful for post-event analysis and not for predictive hazard analyses. r.avaflow is not the only model capable of describing such coupled processes, but readers may well get this impression after reading the manuscript. At a minimum, a discussion of other models and how they also described similar process transitions would be helpful for the reader,

e.g. Iverson et al.'s work on runout modelling of the Oso landslide. The manuscript requires some new literature sections (other models), some new discussion sections (shortcomings and advantages of both models), and a few relatively minor clarifications (below) before it can be published. Some minor points are listed below.

General Comments

1. Is it accurate to call r.avaflow a GIS-based model? Certainly the user interface relies extensively on a GIS interface, but I was under the impression that the model is based on a numerical solution of the shallow-water equations for granular flows, e.g. the flow model written mainly by Shiva Pudasaini. 2. While it is certainly efficient to refer to previous publications about the model and to not re-state the equations used in the model, I think that some description of the equations solved in the model would be appropriate for the reader. Otherwise, it is impossible to understand the manuscript without referring to the paper where the model is described. Does it include terms to describe the influence of curvature on the solution? 3. Section 5: Please define CSI, D2PC, and FoC. Interested readers should at least be able to see how they are defined without having to refer to the Mergili et al., 2018b paper. This is only a few sentences and it would save interested readers a fair amount of work. 4. Lines 211, 284, 290, 340, and possibly elsewhere: I have no idea what you mean by the term "empirical adequacy" which sounds like it could include anything that is physically possible (instead of physically plausible). Please use a different expression. 5. Lines 284âĂŤ292: I've heard similar statements by Prof. Florian Amann and I believe it was also mentioned by S. Demmel in her masters thesis, or in the WSL 2017 report. I have seen this idea presented this several times at different conferences, so it is unfair to not at least mention that it is not your hypothesis. 6. Others (e.g. Iverson & George) have developed similar two-phase models that are capable of describing process chains. This paper gives a false impression that r.avaflow is the only model capable of describing the runout of coupled processes. Please include a few paragraphs describing and/or comparing and contrasting your model with previous ones.

Specific comments

Lines 85âĂŤ86: The statement that the deposit did not connect to the main channel of Val Bondasca is incorrect. The 2011 deposit certainly covered the uppermost part of the Val Bondasca and therefore is connected hydrologically and geomorphologically. The main torrent channel after the 2011 event went over and eroded the deposits of the 2011 rock avalanche deposit. Please correct this error, or more precisely state how it was “not connected”. This is clearly evident in publications of others, e.g. Frank et al. (2019) which illustrates the triggering of debris flows in Val Bondasca following the 2011 rock avalanche. Frank, F., Huggel, C., McArdell, B. W., & Vieli, A. (2019). Landslides and increased debris-flow activity: a systematic comparison of six catchments in Switzerland. Earth Surface Processes and Landforms, 44(3), 699-712. https://doi.org/10.1002/esp.4524.

Lines 102âĂŤ103. These data are almost certainly from the Canton of Grisons and not from the WSL, although they were certainly repeated in the WSL 2017 report.

Lines 129âĂŤ130. This statement about the pore-water pressure rise is somewhat speculative and should be treated as a hypothesis and not as a fact. Rock avalanche deposits (e.g. the 2011 deposits) are often described as having relatively low porosity, and therefore one could, possibly, also expect dilation upon compression which would cause a drop in pore-water pressure and not a rise. The river-bed sediments, and possibly the moraine deposits, upon which the 2017 rock avalanche flowed would be more reasonable candidates for the mechanism you describe here.

Lines 221-222: Did you try other grid resolutions? This resolution, based on my own experience using CFD models (also with a GIS user interface) indicates to me that the grid size is fine for the rock avalanche runout modelling but too coarse for the debris-flow runout modelling.

Line 237: What do you mean by total discharge? The sum of the discharge of all of the phases, or the sum of different flow paths comprising the flow. Or do you mean

maximum discharge or the maximum discharge sum of all of the phases? From the text is appears to be a peak discharge value and not just a total value.

Line 346: A brief re-statement of the Scenarios would be reasonable here for the reader.

Table 1: It’s confusing to have Zones A, B, and so on, as well as scenario sets A & B. perhaps you could use lower-case letters for scenarios a and b, or use something like SA or SB to make it completely unambiguous.

Figure 5 is a nice graphical depiction of the two scenarios.

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