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

Anonymous Referee #1

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The authors present an application of the model r.avaflow to the back-calculation of a complex landslide occurred in Switzerland. The case-study is indeed interesting and the scientific question about the two scenario is stimulating (I also really like fig.5). However, as you stated yourself, the investigation of the process through a two-phase numerical model did not allow to discern between the two scenario. So what is really the "take home message" of your work?

I do understand that negative results are results but in a way you do not really present them as such. For example, most of your introduction praise the capabilities of two phase depth averaged model in "support the confirmation or rejection of conceptual models" stating the intrinsic epistemological potentiality with respect to one phase models. However, you then proceed with your modelling, that anyhow requires calibration

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and the selection of vague "physical plausible" parameters, that has numerical issues that constrain you to use "physically implausible" parameters and that do not perform well in the reconstruction of the actual phenomenon. So rather than titling your paper as "back calculation of the 2017…" I would suggest to switch it to something such as "challenges and open issues regarding the modelling of the 2017….".

These are my other comments regarding your paper

BROAD COMMENTS

1) Optimization and equifinality: The entrainment in your code is calculated with a calibrated coefficient and based on a depth averaged kinetic energy. You defined 6 zones with different friction angles and other calibrated coefficient. How did you suppose it could lead to a selection to confirm of reject a conceptual model (l 51) when, as you stated yourself in the end, there is an obvious problem of equifinality? This issue is common in back analysis, especially when several parameters are involved in the calibration process. To try to give some "physical plausibility" to the whole parametrization of the backward calculation it is important to:

i) define a straightforward and explicit optimization method

ii) use parameters that are somewhat geotechnically believable with respect to the observed phenomena

iii) provide a clear geomorphological/mechanical reason for each zonation – otherwise of course the more are the zones in which the parameters may be changed, the more the equifinality issue arises.

In my opinion point i) is lacking in your paper. The metrics you use are not so straightforward, especially if you need to jump between two papers to reach their definition and reason of being (is it really Mergili 2018b the best paper to refer to or is it better to go directly to Formetta 2015 and Mergili et al., 2017?). Please devote a paragraph to the interpretation of these metrics rather than cut the discussion off with "indicators of a

reasonably good corrispondance". And what is reasonably anyway? Please also show in the picture a zoom of the deposition pattern (modelled and observed) in the alluvial fan. Point ii) is also important. Finding a 45° angle of friction in the E zone is rather "physically un-plausible" as it is, especially when your solid fraction decreases. You discuss this too briefly in the end of the discussion chapter. You have to explain better what is the issue with the code, is the 10 m sampling? is a projection issue related to the conversion of the coordinates? This should be better discussed, also in the light of the new titling of the manuscript. The zoning (point iii) should be more extensively discussed, the definition that it is found in table 1 is too synthetic. For each zone and limit there must be a defined reason to be it that way.

2) Mass balance: in 220 you write that "only heights <0.25 m are taken into account for the visualization and evaluation of the simulation results". That's ok for the visualization part but what about mass balance? how much impact do have diffusion effects in your model? how much material you discard when you filter at 0.25 m?

SPECIFIC COMMENTS

l 47-49: as a matter of taste I do not think that putting 14 references after a sentence contributes much to the clarity and readability of a paper and to the whole general usefulness for supporting a scientific discourse (that should be the main reason for inserting citations in an introduction)

l 88: insert a couple of words to explain how these displacements were monitored

l 94 and following: check that each acronym has its own definition the first time they appear in the text. Moreover the VAW and WSL reports are written in German so it is not easy to extract the required information. Please if you refer to these works add a sentence summarizing the useful findings.

l 100-102: insert data about the average steepness of the tract. In fact in the whole paper little information about the local heights and steepness are inferable. In Fig 1 the

contour lines labels are missing and in the following maps the contour lines are missing altogether. I would suggest the authors to add the labels in fig. 1 and to insert a table or a figure with the average steepness of the channel profile in each of the 6 zones.

l 109: is it possible to talk about rock avalanche with an angle of reach of 28° and no brandung? refer to Nicoletti 1991, Corominas 1996 and Hungr 2005 and discuss

l 116: did you filtered the errors in the volume estimation? If yes, how?

Fig 8 – please put hydrograph a and b to the left

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