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Interactive Comment

Interactive comment on "Evaluating performances of simplified physically based models for landslide susceptibility" *by* G. Formetta et al.

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The paper is interesting and worth publishing in principle. I broadly agree with the comments of Reviewers #1 and #2 but have some additional comments the authors should consider before the manuscript is published.

From a purely technical point of view, the authors present – as far as I can see it – a clear and clean way of parameter calibration/optimization for slope stability modelling. However, I have some major concerns with regard to the scientific meaningfulness of the approach: while it may be useful to calibrate the material parameters I am not sure how much sense it makes to calibrate such a large number of variables, including the intensity and duration of rainfall. The fact that even the magnitude of the triggering

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Interactive Discussion

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event has to be calibrated means in my opinion that the physically-based model by itself may completely fail to reproduce the processes under investigation, but the input may be tuned in a way that the results somehow fit to the observations. Consequently, the model would have no capability to be applied for making predictions e.g., for a potential future rainfall event of a defined magnitude in the study area. For just mapping the general landslide susceptibility, a comparatively simple and easily reproducible statistical approach would do the work. Consequently, I suggest to at least define more clearly in the introductory chapter what are the specific aims of your study and what you finally intend with this very comprehensive calibration. Further, this issue has to be addressed appropriately in the discussion.

Strictly speaking, a landslide inventory should only be used for the evaluation of a coupled hydraulic-slope stability model if it relates to the same triggering event as applied in the modelling (see also comment above!). In general, more information on the landslide inventory should be provided: does it cover only the initiation areas of the landslides, or also the runout zones (in the latter case, it should not be used for evaluating a slope stability model).

In summary, I have the feeling that the authors have done a really fine work in implementing and explaining the computational aspect of their calibration and evaluation procedure. In contrast, they still have to reflect the scientific meaningfulness of the case study employed. At least some aspects should be explained and justified in a clearer way. I would even suggest to rethink the concept and maybe redo the analysis, calibrating only the material parameters. If the data allows, I suggest to use subsets of the landslide inventory which can be assigned to well-defined rainfall events, and to apply the corresponding rainfall intensities and durations to the model.

The authors should feel free to contact me at martin.mergili@univie.ac.at in case they disagree with my comments or if they would like to discuss the one or the other issue.

With best regards, Martin Mergili

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