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Discussions

Interactive comment on "Sensitivity analysis and calibration of a dynamic physically-based slope stability model" *by* Thomas Zieher et al.

Anonymous Referee #2

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Peer review of nhess-2017-73, "Sensitivity analysis and calibration of a dynamic physically-based slope stability model"

The authors are to be congratulated on a well-written and detailed calibration and analysis of slope stability in the Laternser valley of western Austria. The approach is very interesting and appears to be a significant step forward in calibrating process-based models for rainfall-induced landslides, even though it may be similar to calibration procedures used for hydrological models. I have a few comments about the results and discussion and found a few items in the text that need clarification:

P.2 line 5. What are "settlement objects," houses, residential structures?

Figure 2c. The legend to the geologic map lists the map unit names mostly in German. It would be helpful to list the main lithology for each unit in English.

p.12, lines 2-3. What is the lithology of Leimernmergel and Dursbergschichten?

Table 2, Figure 2c and d., and p. 12 lines 2-3. Based on the information presented, it appears that landslides initiated in a number of map units that were not sampled. How do you know that the range of physical property values presented in table 2 represents the range parameter values to be expected throughout the map area? What effects do you think any sampling bias that might be present in your field and lab program had on your calibration results?

Figure 11. Landslide density is noticeably higher in the western part of the area than in the eastern half. Despite the favorable overall results for your model ensemble, the high percentage of failures predicted along the southeast rim and northeast sector of the study area compared to the relatively low number of actual landslides seems to indicate that your model calibration is biased toward the western part of the study area. This might have to do either with the regolith depth model or cohesion and friction parameters. What additional insight can you share relative to the apparent east-west bias of your model results? Could the possible sampling bias noted previously have any bearing on this?

P 22, lines 1-2 and Figure 12a. The compensation between angle of internal friction and cohesion should be expected based on the structure of equations 2 and 3.

p. 23, lines 12-14. Based on available data (Fig. 5), what other predictors might be worth considering?

p. 23, line 16, Please clarify, which parameter values are conservative, the geotechnical or vegetation parameters?

P. 24, lines 1-5, As noted previously, the calibrated model appears to be strongly biased to the west half of the area. Table 2 indicates a wide range of lateral variation in model input parameters. Property zones need not be as detailed as individual lithologic units. In the case studied here, the area might be divided into two or three zones, with division at major drainages or drainage divides. Then calibration of the separate zones could proceed without a need to deal with potential runoff interactions identified here.

P. 24, lines 6-15. The difficulty identified here could easily be overcome with slightly more work. After completing the 10,000 model runs and converging on the 25 best models, it would be a fairly simple matter to rerun those 25, or a subset of them, with hourly precipitation inputs to see whether the outputs change significantly. Experience has shown that averaging precipitation into longer time steps can effectively reduce the total number of rainfall inputs needed (Baum et al. 2011; Alvioli & Baum, 2016). Testing to find the most effective combination(s) of time steps to represent a particular rainfall sequence can be done fairly quickly using single-grid cell models.

Discussion section: Please compare and contrast the relative advantages and disadvantages of the approach used here and a probabilistic approach to initializing the input parameters for TRIGRS (Raia et al. 2014).

General comments: Given the requirement of high-performance computing to complete the modeling exercise described in this manuscript, the authors should be aware that an MPI version of TRIGRS is available (Alvioli & Baum, 2016).

Throughout the manuscript, please change "effective cohesion" and "effective angle of internal friction" to "cohesion for effective stress" and "angle of internal friction for effective stress." The fact that these parameters are for effective stress is an important distinction that is glossed over in far too many recent papers about landslides.

Although the English is generally very good, the manuscript would benfit from editing to make minor grammar corrections throughout.

References cited:

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