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Discussions

Interactive comment on "Rockfall modelling in forested areas: the role of digital terrain model spatial resolution" *by* Barbara Žabota et al.

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General comments

The proposed topic is interesting and relevant for rockfall hazard assessment. The spatial resolution of the digital terrain model (DTM) used in all numerical simulations is critical and a good trade-off needs to be found between best possible resolution and computation time, but also possible artefacts of too small DTM cell sizes. The authors calibrate the Rockyfor3D rockfall model parameters with a 1-m DTM using a past rockfall that occurred in 2017 and had a quite large volume (nearly 30'000 m^3). They the vary the DTM cell size from 1 to 25 and assess the effect of spatial resolution on the modelled run-out distance and area in comparison with the true extent of the

2017 rockfall. This approach is sound, but the data analysis and interpretation have many major flaws that need to be addressed prior to publication in NHESS.

Specific issues

1. I have doubts that the large volume of the 2017 rockfall is appropriate for a study with Rockyfor3D, as the model is more intended for fragmental rockfalls (single blocks) instead of large volumes that fragment during the event. Using a scree slope formed by multiple rockfall events might provide a more realistic test site. The study would anyway also benefit from several test sites in order to gain more substantial conclusions.

2. The study lacks details on the method used to locate the trees (using the FInT tool provided with Rockyfor3d?) and how the tree locations have been adapted at different spatial resolutions. If the tree location file created by FInT for the 1-m DTM is used for all simulations, I do not understand why there would be so significant differences in number of trees, tree diameter and kinetic energy as shown in Table 7. If FInT is used with different spatial resolutions this should be explained and differences should be shown on maps. In order to provide a complete study of the topic, the authours should also test the alternative approach in Rockyfor3D with the raster files containing tree density and tree diameter etc. How is this approach affected by changes in DTM resolution?

3. Regarding the rockfall model parameters, there are several problems in the calibration:

- The best-fit surface roughness (Rg) parameters are the smallest ones (calibration run 01), while higher values yield poorer results. Please test also with Rg values smaller than those used in calibration run 01, i.e. until the values give worse results. Like that you tend to the real optimum parameter set.

- The initial fall height is set to 50 m which seems excessive considering that you have high-resolution LiDAR data that should correctly depict the location of the rockfall source area and thus of the height difference between the source area and the toe of the cliff. The additional fall height of 50 m is probably the reason why the run-out area is always overestimated.

- Using a variation of the rockfall dimensions by 50% is appropriate for hazard assessment as it expresses the spread in rockfall volumes observed in the field. For this study, I would however use only a fixed rockfall volume (0% variation) in order to focus the test only on the effect of DTM spatial resolution and on the forest.

4. The goodness-of-fit indices and modelling accuracy statistics need to be used more appropriately and carefully:

- The authors use many different statistics to assess the goodness-of-fit between modelled and observed run-out areas. The authors should select fewer indices, as many of them are related to each other and the reader gets lost. The sensitivity and the false negative rate always sum up to 1 (idem for the specificity and the false positive rate). The whole second paragraph in section 3.2 is therefore redundant with the first paragraph in section 3.2!

- The indices TPR and FPR are used in the text and in Table 4, but they are not defined in the text or tables.

- In Table 4, the headings FPR and TPR cannot be correct. The best TSS value is also obtained for calibration run 01. Based on those results, I suppose that the calculation of the SI cannot be correct and should also be best for run 01.

- Regarding the results of the changing spatial resolution (Table 5), the results should be corrected for the change in cell size and how to attribute cells that are partially in the real run-out area and partially out of it. Attributing the whole cell to the TP or FP might lead to false results; taking instead the exact area located inside or outside will likely be more correct. This effect amplifies with larger DTM cell sizes.

5. The entire section 3.3 on the comparison of model outputs with and without for-

est needs to be refocused and corrected. Many of the statements in the text are in disagreement with Tables 6 and 7. Furthermore, Table 6 presents several errors:

- It is unclear how the E_mean, Ph_95CI, Nr_d, Nr_p parameters are computed. Is it for the entire modelled run-out area or only for the cells located within the observed run-out area? I would rather use a fictive rockfall fences (or screens) in the central and distal parts of the observed run-out area in order to assess the number of blocs, their energy and passage height at those screens, and use those results in order to assess the effect of forest and spatial resolution.

- The Ph results in Rockyfor3D are usually the passage height and not the maximum kinetic energy, but values provided in Table 6 cannot be the passage height.

- The number of blocks deposited and number of blocs passing through a cell need to be corrected for the total number of simulated rockfalls. With larger DTM cell sizes you have fewer source cells and thus a smaller number of total simulated blocs, which should explain most of the differences observed in the number of passing and deposited blocks.

6. All analyses and interpretations need to be checked again in light of above comments and the entire discussion and conclusion section needs to be reworked. The present conclusions seem not relevant enough for publication in NHESS.

Technical corrections

- The use of the term "DTM spatial resolution" can be somewhat misleading when writing about "better resolution" (=smaller cell size), "increasing resolution" (=smaller cell size) or "decreasing resolution" (= larger cell size). Using "DTM cell size" instead of "DTM spatial resolution" avoids this ambiguity.

- How did you resample the DTM for larger cell sizes? A raster aggregation function with the median elevation value is generally recommended. Using a resolution of 12.5 m might be problematic as it is not an entire multiple of the original resolution, which likely leads to resampling artefacts.

- Most numbers are given with too high precision (e.g. line 84: area of 19,342 m^2, whole section 3.3, Tables 4, 6 and 7), especially when considering the uncertainties in the modelling –> reduce to 3 significant digits.

- Cited references: The references should be more focused on the intended point they refer to. One general reference would suffice for example for the description of the rockfall phenomena (line 32) (Petje et al. 2006 and Lopez-Saez et al. 2016 are well not the first to describe the phenomenon of rockfalls). More pertinent references could also be given for other statements in the introduction (lines 34, 37, 40).

- Lines 64-65: It would be interesting to summarize the findings of other studies focusing on DTM resolution and compare them to your findings.

- Line 98: the definition of the maximum kinetic energy is too vague. Later you use the 95% confidence interval, but also the mean of the maximum kinetic energy. –> specify what is what. . .

- Line 115: there is a mismatch in the size of the 2017 event (4000 m^2 here against 19,342 m^2 in line 84)

- Lines 190-192: Explain why the sensitivity is higher in models without forest, while the specificity is higher in models with forest. This seems contradictory and needs thus explanation.

- Line 211: explain where the underestimation occurs (in the SW) and explain why the underestimation occurs there (morphology etc.)

- Table 3: correct the formula of the specificity (FP instead of TP in the denominator), provide also the range of values and optimal value (as in Table 2)

- Figure 1: add a local map of the study area, a field photograph and provide the dimensions of the rockfall in the aerial image.

- Figure 4 cannot be correct. I suspect that the graphs depict the number of cells and not the area (multiply the number of cells by the square of the cell size)

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