



Interactive comment on “Modelling catchment-scale shallow landslide occurrence by means of a subsurface flow path connectivity index” by C. Lanni et al.

Anonymous Referee #3

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General comment:

The manuscript describes a very interesting approach to assess shallow landslide susceptibility and it takes into account some of the hydrological complexity like the lateral connectivity which is known to be very important. The model approach is elegant and sound. The paper is well written.

However, the paper leaves me with a disappointing feeling, it does not proof it works nor what is the gain of this method over classical data driven susceptibility modeling (at the cost of more work, more data and uncertainty). The bottom line is in my opinion

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that the proposed approach is a relatively complex way of making a susceptibility map. Then the question to address by the authors should be: what is improved compared to a classical map overlay susceptibility map?

In my opinion the weakest point of the paper is the comparison with field data. Figure 4 shows typical landslide due to undercutting or stream erosion activity and typically these landslides are too large for shallow landslides. Why are there no landslides in the upper ranges of the catchment? I doubt strongly if this data set can be used to validate your model or even qualitatively indicate the model works for the right reason. Secondly, comparing your model results with a debris flow data set is doubtful. Are debris flows generated in the same way as shallow landslides? Why would the proposed method to assess shallow landslide susceptibility be validated with a debris flow data set?

I do not really understand table 2. But it seems that he observed landslides are in 0-10y and 10-30y return period classes. But how often are you wrong? A more standard way of showing how well your method is doing is by a kind of success rate, prediction rate method (see for example Chung and Fabbri 2003, Validation of spatial prediction models for landslide hazard mapping, *Natural Hazards*, 30 (2003), pp. 451–472).

The spatial variability of the soil depth is a critical parameter in your model, but it is determined in a quite rudimental way associated with quite some uncertainty. There are 49 points on 7.5 km² or one point per 0.15 km². Could you assess the uncertainty and indicate the effect of these errors in the susceptibility map. Could you compare or at least discuss this soil thickness estimation with different techniques to estimate the soil thickness.

In conclusion, the authors present a sound and very worthwhile methodology but do not show it works. I encourage the authors to bring this interesting work to a higher level. The authors could in my opinion elaborate on success and prediction rates, on the effect of uncertainty of especially the soil thickness and compare the results with

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more classical susceptibility mapping. I look forward to an interesting discussion with the authors about their work.

Specific comments:

- Could you elaborate a bit why lateral flow occurs on hill slopes and that can be modeled without taking into account explicitly preferential flow?
- Is the infinite slope assumption the best to use? Is it valid to apply? See for example: Milledge et al (2012): DOI:10.1002/esp.3235
- Eq20: Is the threshold of 2000m or 2500m based on something? I do not see why this differentiation could be relevant, especially with only 49 observations.
- Above 40 or 45 degrees slope there is an assumption of negligible soil thickness. I did not get from the model description where is the precipitation going that applies to these pixels? Is it laterally transferred to the downslope cells or is it that water neglected?
- P.4115, L8: there is more than 50% forest coverage in the catchments and there are soils of only a few decimeters till 1 meter. How can we then assume cohesionless soil? Could you overlay the landslide inventory with the land use map? How many landslide occur in forested part of the catchments?
- P 4115, L 24-25. The authors state they do the modeling for a predictive procedure, not for diagnostics. As stated before, the modeling approach is really interesting, but is a deterministic model that needs some calibration / validation / verification. Why otherwise doing it and not adopt standard map overlay techniques?
- P.4116, L 5. Why using the rainfall statistics and not the measured rainfall?
- P.4116, L.16: I do not see that the results are good. You do not show this. Here I find the paper very weak. If I glance to figures 4 and 5, I think you are mostly wrong and sometimes correct. All the high susceptibilities in the upper part of the catchment are not in your inventory. See my general comment to follow the success rate, prediction

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rate procedure or make a cross table of prediction and measured susceptibility classes.

- P.4117, L.17. The authors are totally right that other methods do not account for (unsaturated) hydrology and surely not lateral contributions. But do you proof it is important? Does taking into account of unsaturated hydrology and lateral flow improve the susceptibility mapping?

Technical comments:

- There is too much methodology in the introduction section. p.4104L12 until p4105L12: This should mainly be part of section 2.

- P4107 L9: Troch et al (1992) Not in reference list

- Eq 15a-b contains several typo's (missed (x,y) notations at several places.) However, I believe eq 15a-b are redundant to 14a-b. The sentence they are spatially conditioned (x,y) should be enough.

- Eq 20c is not needed.

- P.4118, L.28 (filled circles? Or open circles)

- Figure A1: there are 60 dots but the caption text states there are 50 scenarios?

- Table 1: is a Ksat of 10E-3 m/s realistic? It seems very high. Especially combined with a saturated water content of only 30%

- Fig. 2 explain a and b a bit.

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