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Earth Surface Dynamics Discussions

Interactive comment on "A hydro-climatological approach to predicting regional landslide probability using Landlab" *by* Ronda Strauch et al.

Anonymous Referee #1

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The paper describes a integration between a physically-based hydrological model based on a steady-state subsurface flow representation and the infinite slope stability model. The model, which is available as a component in Landlab, an open-source, Python-based landscape earth systems modeling environment, has been applied in a 2,700 sq km steep mountainous region in northern Washington, (USA) using 30-m DEM resolution. The model structure is not a novelty (i.e. it is similar to other models like the SINMAP) even if the authors try to use a hydrological model (VIC) in order to derive the recharge R. The authors derived three maps probability of landslide initiation highlighting the presence of elevation dependent patterns. A model validation has been carried out using observed landslides showing performances, which have been declared as "modest" by the same authors. I find the research scope generally

good. The concepts are almost always sufficiently exposed, the topic is important in the hydrology field with a medium-high impact for the hydrological-geomorphological sciences. However, in my opinion the paper could be accepted after a major and careful revision. In the following I report my observations:

Main problems

1. Manuscript structure: the paper is long and a little bit convoluted. In my opinion the length of the paper could be reduced without any important loss of information and the structure could be improved. For example the sections related to the cyberinfrastructure Landlab (sect. 2.2 and figure 3 and the last part of the section 2.3) could be reduced or removed since it is less important for the reader of ESurf (see Aims and Scope of the ESurf Journal). Some parts are difficult to understand (see other comments) and there are some repetitions that can be removed.

2. Basic assumptions:

a. The authors fixed a soil density equal to 2000 kg/cubic meter constant over the entire domain; is this relative to the bulk density of the soil or to the wet soil density? Is this assumption realistic considering that you have different soil type in your domain?

b. The authors assume the soil as incoherent (C=0) assigning all the cohesion to the root? Again is this assumption realistic? Please consider that also a loamy sand could provide a cohesion greater than that given by the root system. Please try to justify this assumption using field data relative to the soil mechanics parameters.

c. The authors assume that the recharge is given by the sum of the baseflow and the surface runoff (page 10) at each VIC grid cell. It is not clear the reason of such an assumption since usually the recharge is given only from the subsurface flow (i.e. part of the baseflow) as highlighted by the authors (page 6 – line 23-25). In similar modelistic approach (SINMAP) R is considered as a climatic factor (rainfall). Please clarify this apparent contradiction.

d. It is not clear how the soil depth evolution model and the stability model are coupled (if they are coupled). I thought that the outcome of soil depth evolution model is provided as soil depth map (in terms of mode) but there is a sentence (page 11 – line 26-27) which is in contrast with my previous thoughts (“Eq (a) and Eq (2) are used to calculate FS within the soil evolution model”. So please try to clarify the connection between these models. I think that a figure with a flow chart describing models and connections together with the setup of the experiment could be useful to the readers. How many simulations did they run?

3. Choice of model parameters: the choice of geotechnical and soil parameters (mode and range of variability used for MonteCarlo simulation) is, at least, not convincing.

a. The internal friction angles are fixed in Table 1 in terms of mode, min and max. I'm not convinced by these values; they seems to be very high especially for the loamy sand and sandy loam. Could you provide references or field data used to fix these values?

b. The authors use different relationship to define minimum and maximum value of trasmissivity T and friction angle. How do they define these relationships? What is the impact of these values on the final results (sensitivity analysis).

4. Low performance of hazard maps: the authors affirm that the performance of proposed approach is modest. I agree with them and if the aim of this model is to create a map of landslide hazard, better results could be achieved using classical susceptibility approach based on statistical methods or data-driven methods. Moreover I think that they can remove the CD approach to test the performance of the proposed approach. The CD approach is aimed to highlight the existence of a statistically significant difference between the two P(F) cdfs (within and outside) for fixed soil depth scheme. The authors can only affirm that the two cdfs are different but this does not imply that the model performances are acceptable. I understand that this could be a first level check, but I think that can be removed without any problem for the paper.

Other comments

* Page 7, lines 9-10: The sentence is not clear. How does the use of maximum annual daily recharge help to define uncertainty in R?

* Page 7, equation (3a): Please define n and n(FS<1).

* Page 9, line 9-21: The difference between options 2 (lognormal) and 3 (lognormal spatial) is not enough clear. Also the option 4 is not clear. Please clarify this paragraph.

* Page 9, line 30: what is core node? Is it a computational element of spatial domain? I think that these details on the computational framework are not necessary since they create a little bit confusion in the main line of the paper.

* Page 10, lines 16-19: please check the sentence since it is not very clear.

* Page 15, line 11: define combined curvature (reference). Is it different from the total curvature used further?

* Page 16, line 22-23: sentence not clear; lines 23-25: this sentence can be removed.

* Page 17, lines 7-8: what is the meaning of “spatially consistent”. You can use consistent when map is compared with the field data. Did you carry out this task?

* Page 17, lines 18-19: how can the authors “confirm” the ranges of soil depth used through a long term evolution model which needs to be calibrated on soil data as well?

* Page 19, lines 15-17: how the authors calculate pore-water pressure starting from the maximum daily recharge? Do they use pore-water pressure in their stability model? I think they use directly the recharge (see equation 2).

* The section 4.1.2 is not clear especially the role of regression based equation which seems to provide the soil depth as a function of slope and curvature. If you run the soil evolution model, why do you need a regression to obtain soil depth? In the same section it is not clear the difference between M-SD and M-SD LT. I think that there is a

lot of information but this is not well-organized.

* Figures 10 and 11 highlight the same information (Probability or return period). Please consider removing one of the two figures.

* Page 32, line 13: The authors are not using “observations” in figure 13 but model results. Please change the sentence.

* Figure 13c: in the legend line relative to M-SD LT is missing.

* Page 34, line 16: since the authors use 10% of highest elevation cell, I suggest to remove 20% and 30%.

* Page 34, lines 24-26: I suggest specifying the number of DA source cells and the number of DA outside source.

* Figure 14a: I think there is an error in the plot. The sum of all the bars must be equal to 1. If this is true for Outside DA, it cannot be true for Source DA since for each bin the relative frequency is lower.

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