Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-233-RC1, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "LABS: an agent-based run-out program for shallow landslides" *by* Richard Guthrie and Andrew Befus

Ivan Marchesini (Referee)

ivan.marchesini@irpi.cnr.it

Received and published: 25 October 2020

GENERAL COMMENTS

The manuscript describes a completely new agent-based software, LABS, devoted to modeling of debris flow runout, erosion e deposition at regional scale and given a limited amount of information about boundary conditions and physical parameters. In the first part of the manuscript the authors provide a rapid introduction to the software. Then they describe two application of the code: in Indonesia and Canada. Finally they conclude discussing the code and its limitations.

The manuscript is interesting and within the scope of NHESS even if I think that its scientific significance, quality and its presentation quality can be improved, also to

Printer-friendly version

Discussion paper

better support the interpretations and the conclusions derived from the results.

About the structure of the paper I note that frequently in the text (e.g. page 5, line 13 and line 23, or page 6 line 6 or page 8 lines 16 to 23 and others) there are comments and interpretations that should be reserved for the discussion or the conclusions. I also suggest to split the discussion of the model (and its limitations and performance) from the discussion of the results in the two study areas and to move there the text and concepts of sections 3.2.4 and 3.3.5.

The manuscript includes many figures (27) and possibly some of then can be further combined and or excluded or also improved (screenshots by themselves are not very informative). I think that legend is important in the figures showing the erosion and deposition cells.

Authors state that the software will be freely available for to no-profit groups. I wonder why not to release the code under an open source license (GPL as an example) in order to facilitate the re-usage and improvement, of this interesting tool, by the scientific community. I also wonder if the tool will be provided as a binary code for multiple OS (Win, MacOS, GNU/Linux)

SPECIFIC COMMENTS

Among the different papers dealing with regional modeling of debris flow runout cited in the introduction I would suggest to introduce these other two papers that are exactly dealing with the topic: âĂć Mergili, M., Krenn, J., Chu, H.-J. (2015): r.randomwalk v1, a multi-functional conceptual tool for mass movement routing. Geoscientific Model Development 8: 4027-4043. doi:10.5194/gmd-8-4027-2015 âĂć Mergili, M., Chu, H.-J. (2015): Integrated statistical modelling of spatial landslide probability. Natural Hazards and Earth System Sciences Discussions 3: 5677-5715. doi:10.5194/nhessd-3-5677-2015

In the description of the program it is said that the software uses 5m resolution DEM.

Is it not possible to run the tool on a grid having different size? If not I think that some comments on the memory requirement for running the tool on a given portion of the territory should be provided since this can hamper to use of the tool, on normal laptop or workstation, for modeling large areas.

Description of the program is rather short and not very detailed. As the tool is proposed to the scientific community I suggest to enlarge the description of the algorithm. As an example section 2.4 about spread is not very clear to me and the 3D Gaussian surface of figure 2 is not very informative. Moreover I'm not really sure that all the possible parameters visible in figure 20 (fan maximum slope, etc. . .) are fully described in section 2. I also suggest to clear if the model, after the run, alter the DEM, carving or uplifting it in correspondence of erosion and deposition areas. This would be an important tool since, when a an agent passes through a given cell, it could find the the DEM altered by an antecedent agent coming from another source and this could have an effect on the propagation of the second one. It seems the DEM is modified, from what we can read at lines 10-11 at page 6, but probably it should be made more clear.

Some concepts are repeated. As an example page 5/6 lines 26-27/1-2 or page 7 lines 10-11

I suggest to add information to the background for the first case study. As an example it is relevant to know the size of the study area, which DEM was used and if the DEM is pre- or post-event.

Section 3.2.2 is about calibration. However it is not clear how the model is calibrated. I would have expected that some of the parameters used by the model would have been changed to make the model match with the observed data but it seems to me that there is not such type of action. Why didn't the authors tried to tailor the model results to the ground truth? This has also to do with the model sensitivity. I really suggest to better discuss this point.

Lines 1-5 at page 7 describes figure 10 and rainfall triggered landslides and storm re-

turn period. I think that the chart should contain the known data in order to understand how they are fit by the curve and how many point were used to build such a relationship.

In section 3.2.3 it is said that random source location are placed based on a susceptibility map. It is relevant to describe how this susceptibility map was generated. Authors also mention existing terrain polygons but, if I'm not wrong, they were not mentioned before and I don't know what they are. It is not clear to me what they are. In the same section lines from 1 to 8 are not very clear to me. Probably experiment settings should be better explained. Again, in the same section, at line 10, authors say "once the historical event were calibrated.." but calibration phase was in section 3.2.2 and I'm not sure they are talking of that part of the manuscript but rather about the six storms.

At lines 14-16 of page 9 authors discuss a sort of susceptibility map. The citation is Palmer (2018) but the reference is "Palmer: Lake Cowichan and Youbou Slope Hazard Assessment, 2018." that I wasn't able to find.

At line 20 of page 9 authors say "The model was calibrated by simulating landslides within the study area, comparing the results to mapped and expected landslide behavior..". I think they can improve the description of what they intend with "mapped and expected landslide behavior".

At line 24 of page 9 authors say "magnitude frequency curves that are similar to other coastal BC data sets (Figure 18) with a similar rollover and distributions" but, taken the same area value, the model data differ till 1 order of magnitude of cumulated probability with the other curves. It seems, as a consequence, that area distribution is underestimated. Why non to try to better calibrate the model parameters to improve the matching of the model outputs with the frequency size distribution of the real inventories?

In section 3.3.2 manuscript declares that "Landslide initiation locations were created by importing randomly distributed points, a uniform distribution of points, and manually in the GIS tool within LABS". However there is no discussion about the effect of these different methods used to define the landslides initiation locations on the performance

of the models. There is only a rapid comment on section 3.3.3. On section 3.3.3 authors start with: “Once tested..”. Are they meaning “Once calibrated..”?

At line 9 on page 10 there is the following text: “(both random and manually selected)” . But just a paragraph above authors state: “A user-based initiation-point selection method was used for the final model runs as this method generally resulted in landslide generation somewhat more frequently than randomly or uniformly generated points that would sometimes occur on a flatter portion of the slope”. Sorry but I don’t understand which is the method used, at the end.

Comment on lines 12-14 of page 10 are interesting but I wonder if they can be considered conservative given the fact that the magnitude frequency curve of the modeled landslides resulted in smaller landslides respect to those observed in other similar zones.

In section 4.3 there is a discussion about DEM resolution, that is fixed to 5m. As I said, I think that this should go together with an analysis of the memory requirement for running the tool. Having a fixed value for the resolution there are probably limitations (memory) about the maximum size of the area that can be studied using a computer.

TECHNICAL CORRECTIONS

Page 8, line 29: “700 mm” and “6000 mm” I suppose.. Is the -1 an error?

Page 10, line 29: are authors meaning Figure 24 and 25?

page 12 line 6: please remove “is”

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-233, 2020.