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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Including informal housing in slope stability analysis – an application to a data-scarce location in the humid tropics" *by* Elisa Bozzolan et al.

Anonymous Referee #2

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General comment

The manuscript (MS) deals with modeling possible impacts of informal urbanization on the hydrologic and geo-mechanical response of hillslopes, also with the aim at understanding which of the factors of such an urbanization process are the most detrimental for slope stability. The modeling is built as an extension of a previously released model (CHASM).

I really enjoyed reading the MS, which is well written and structured. The supplementary material explains in detail the CHASM+ model and other aspects of the MS, and it is really an added value to the main text.

From a general standpoint, the conclusion that slope cutting is the most detrimental among the other factors included in the modeling could be somewhat expected/or reached without the use of the massive modeling in the paper. However, I think that the main contribution given by this MS is that the model enables to QUANTIFY the response of the hillslope to the most important factors of informal urbanization and that it presents the application of some interesting statistical techniques to resume and communicate the main results of the modeling.

Processes are represented in a somewhat simplified manner, but still the resulting model is quite complex and has several input parameters. Perhaps one could argue about some of the choices made in the model and the definition of the parameters' probability distributions (see also referee 1), but my opinion is that the authors have made all those choices in the most reasonable manner possible.

For all the reasons above, I finally think this is a very good work, and my opinion is that that the MS can be accepted after minor revisions. In the following I provide just some suggestions to improve it.

Specific comments

L 83 The MS "promises" that somehow the modeling exercise will take into account climate change. I think this is quite weak in the analysis presented. The authors should discuss a little if climate change projections could be used to define future values of rainfall based on Representative concentration scenarios and simulations by Regional/Global climate models, and mention literature on the subject: e.g. https://doi.org/10.1016/j.jhydrol.2016.02.007, https://doi.org/10.1016/J.JHYDROL.2018.10.036

LL 198-200 The water table height is varied between 0 and 90 % of the slope height. This seems a quite wide range. Perhaps the reasons for this choice could be better explained.

L 234 Perhaps a reference explaining the Latin Hypercube sampling technique can be useful for readers.

Section 4.2 and LL 263-275 of the supplement: The objectives of the multi-optimization are quite unusual. Perhaps in this case, an optimization based on ROC (receiver operating characteristics) analysis (i.e.: True and false positives/negatives) could have been employed and would have been more meaningful. At least, literature in the subject should be mentioned: e.g. https://doi.org/10.1007/s10346-020-01420-8, https://doi.org/10.1029/2012JF002367, https://doi.org/10.5194/hess-18-4913-2014

Fig S1 (supplement): Panel (a) is repeated in panel (b), so perhaps it could be removed. Possibly add to the plot the rainfall time series (cumulated sum)

Section S1. Perhaps the case of houses WITH gutters should be explained.

Technical corrections

L60 of the supplement: CHAMS -> CHASM (check the entire MS)

L137 the comma before “ranges” seems not necessary

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