

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC1
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Comment on nhess-2021-364

Anonymous Referee #1

Referee comment on "Impact of spatial data uncertainty in debris flow susceptibility analysis" by Laurie Jayne Kurilla and Giandomenico Fubelli, Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2021-364-RC1>, 2022

Dear authors,

first of all thank you for the nice reading, I enjoyed going through your manuscript.

I have suggested the editor for minor revisions and below what I will do is to initially summarize what I understood of the work you proposed and then report my suggestions.

What you present is a susceptibility model at the continental scale. The phenomena you model are debris flows, which you access from the NASA repository. The mapping unit you chose are grid-cells (did you mention their size in the text?) while the covariates you chose span from climatic to terrain ones, to land use and more. You run this experiment by making use of the locational uncertainty provided in the debris flow metadata. As a result you can select debris flows with different level of certainty of their positional accuracy, then run a susceptibility model for each group respectively.

The modeling framework is solid. I only have one suggestion on this. You can remove the term presence-only from the text because it is true that MaxEnt is often referred to as presence only model in the ecological literature. But, in a landslide context, all the model implementations we run do exactly the same thing that maxent does. For instance, even a logistic regression does exactly what you did here. It starts from a set of locations where you consider your presences, then it extract absences or pseudo-absences at random, with a number equal to what you set here to be your background. So, shall we call all the other models presence-only? I think it is more of a phylosofical definition but in the daily life of every susceptibility paper out there what happens is that the two framework coincide.

Also, your exact model, with only 5 debris flows is quite difficult to justify. There I would stress the limitations even more in the text.

One thing I have noticed is that you use the natural break method to classify your susceptibility. This is something that Lombardo et al. 2020 stress in their work. Often authors use one method or not to justify the classification they opt for. I would suggest to write a couple of lines on why you chose this over any other criterion.

Ref: Lombardo, L., Opitz, T., Ardizzone, F., Guzzetti, F. and Huser, R., 2020. Space-time landslide predictive modelling. *Earth-Science Reviews*, p.103318.

As for the last comments, in all figures you use the acronym for kilometer as Km. This is incorrect as the symbol for kilometer in the international system is km. I would suggest to change it across all figures.

Good luck with the progress of your paper.

Kind regards,

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