

Interactive comment on “Effective surveyed area and its role in statistical landslide susceptibility assessments” by Txomin Bornaetxea et al.

Anonymous Referee #2

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Dear Authors, I carefully read your paper about the role of effective surveyed area in susceptibility mapping and I think it is very interesting. You pointed out the need of a good knowledge about both the presence and the absence of landslides in the study area and you developed a software to define the ESA, where landslide presence or absence should be well known once the survey has been carried out. You also compared the susceptibility maps defined using different map units (grid cells and slope units) for different areas. In general, the paper is well written and clear, it has been properly structured and almost all phases of the work have been described; however, it needs to be improved. There are many issues that need to be fixed, before the paper can be considered for publication. Most of the cited works are self-citations or refer to co-worker papers, whereas several relevant papers from other scientists

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about susceptibility mapping techniques have been ignored. It is not clear the utility of using the `r.survey` code to define the ESA, instead of using a simple portable GPS to record the surveyed area; its utility should be better described. The results of some experiments have been presented as they are, without providing any explanation or demonstration of their correctness (see sections 3.3 and 3.4). Main concerns regard the susceptibility mapping: why do you used different stability thresholds for WA and ESA? How you defined the threshold values? You cannot compare these maps if you used different criteria to perform the analyses. You should do a comparison using the same threshold value for both of them. One of the main outcomes is that the use of slope units increases the quality of the results, but the performance differences between the different approaches are very low; I believe that suck small difference do not justify the whole work. If a costs/benefits analysis would be performed, it could result that the WA-PM approach would be the best one. For the aforementioned reasons I believe that the paper has to undergo a major revision, before it can be considered for publication. Further comments are reported in the attached file.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2018-88/nhess-2018-88-RC2-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-88>, 2018.

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