

## ***Interactive comment on “Modelling landslide hazard under global change: the case of a Pyrenean valley” by Séverine Bernardie et al.***

**Anonymous Referee #1**

Received and published: 28 February 2020

I've carefully read the manuscript “Modelling landslide hazard under global change: the case of a Pyrenean valley”, by Séverine Bernardie and co-authors. I've found it interesting and I believe it might be interesting to all NHESS readers. It deals with a debated topic, i.e. the evaluation of changes in landslide hazard due to global change (both climatic and environmental), proposing a quantitative approach.

Overall, I've found the whole manuscript a bit lengthy. I believe that it could be strongly shortened, in particular, in the introduction (Section 1) and in the description of the study area (Section 3, e.g. lines 4-30 at page 7 are not very useful). Also the other sections could be slightly shortened. Moreover, the abstract is very long and should be reduced, at least reaching the 70% of the current length.

In my opinion, the whole article misses homogeneity. The numerous figures are very

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heterogeneous: as an example, the scalebars in the maps are different among each other, as well as the dimensions of the figures themselves; in some maps, north arrow and scalebar are present and in some other they are missing; the used colors do not allow an immediate comprehension of the maps. I would suggest a review of all the figures (please see also all my comments below).

Moreover, several language issues are present. As an example, Authors use small and shallow as synonymous when referring to landslides, as well as big and deep. However, this is not correct: e.g. a shallow landslide is not always small. Furthermore, there is a bit of confusion in the manuscript among the terms hazard and susceptibility, which are not synonymous. Finally, also land use and land cover are not exactly synonymous. I would suggest using a rigorous terminology and correcting accordingly all the manuscript. Furthermore, I would suggest using “method” instead of “methodology” and “type” instead of “typology” everywhere in the text. Moreover, I would suggest using only “rainfall”, uncountable. Finally, I would suggest to check all the acronyms used in the text.

Besides these comments regarding the structure and the terminology of the paper (even if the confusion between hazard and susceptibility is not only a matter of terminology but is a matter of method), I have some other comments regarding the proposed methods and procedures.

I have not understood why the Authors used two different periods for the short-term analysis: 2011-2040 for land use/cover change analysis and 2021-2050 for climate change analysis. I would suggest using the same period, also for a better comparison.

Regarding the ALICE model, it is not clear which parts of it are already present in the literature and which parts are introduced in the mentioned paper that is still under review. This should be better clarified.

Regarding the data, at the end of page 7 Authors state that 426 landslides were identified. However, the sum of the landslides reported in Table 1 is 346. Please clarify and

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correct.

Furthermore, at line 12 (page 8) Authors state that only landslides with translational and rotational shear surfaces were selected to be modelled. However, at line 20, they state that four types are considered. Please clarify.

I would suggest to better clarify the procedure adopted for defining the maps of the factor of safety. It seems that some steps are missing.

In section 5, Authors state that the simulations were made for each landslide type and according to the 10 ground water filling ratios. Why? Why not considering only the most frequent ratios (as shown in fig.5)? Moreover, I see in the manuscript different analysis for the four groups of landslide types. It would be interesting to provide also a comprehensive analysis considering all the landslides together. I suggest producing also current and future maps showing the results obtained considering all the landslides together.

Still in Section 5, it is not clear how the five classes of hazard are defined. What does a “high” hazard mean in quantitative terms? Please explain.

Finally, I would suggest performing another analysis considering both climate and land use changes together, at least for the long-term period, so evaluating the combined effect of the future scenarios. If feasible, this would represent an element of innovation for the paper, and would improve its appeal.

Overall, I believe that the manuscript deserves publication after a thorough revision. Besides the above reported general comments, I made a list of some specific comments.

Page 1, line 22: please add “change” before the acronym LULC to be consistent.

Page 1, line 23: “climate change inputs” is somehow vague.

Page 1, line 27: “significant despite being small” is also vague.

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Page 2, line 3: what do you mean with “small” landslides? This should be better explained.

Page 3, line 1: “Gariano and Guzzetti (2016)” is not reported in the reference list.

Page 3, line 9; please correct “rainfalls” here and elsewhere in the text.

Page 3, line 15: It seems to me that the 3 cited references do not deal with future evolution of hydro-meteorological conditions that implies modification in the frequency of landslide. I would suggest replacing them with the following examples: Alvioli et al., 2018 <https://doi.org/10.1016/j.scitotenv.2018.02.315>; Gariano et al., 2017 <http://dx.doi.org/10.1016/j.scitotenv.2017.03.103>; Peres and Cancelliere 2018, <https://doi.org/10.1016/j.jhydrol.2018.10.036>; Rianna et al. 2017, [doi:10.3390/hydrology4030034](https://doi.org/10.3390/hydrology4030034); Robinson et al., 2017 [dx.doi.org/10.1139/cgj-2015-0602](https://doi.org/10.1139/cgj-2015-0602), Sangelantoni et al. 2018, <https://doi.org/10.1007/s11069-018-3328-6>, Turkington et al., 2016 <https://doi.org/10.1007/s10584-016-1657-6>.

Page 3, line 18-24: I would suggest also reading the works by Gariano et al. 2018, <https://doi.org/10.1007/s10113-017-1210-9>; Persichillo et al. 2017, <https://doi.org/10.1016/j.scitotenv.2016.09.125>; Pisano et al. 2017, <http://dx.doi.org/10.1016/j.scitotenv.2017.05.231>; Promper et al. 2014, <https://doi.org/10.1016/j.apgeog.2014.05.020>, Reichenbach et al. 2014, <https://doi.org/10.1007/s00267-014-0357-0>.

Page 4, line 2: I would suggest repeating here some of the above-mentioned references on future impact of climate change on landslides, which are more recent than the cited ones.

Page 4, line 26: please replace “GIEC” with “IPCC”.

Page 4, lines 29-32: I would suggest deleting the acronyms ALICE and GARDENIA here, since they are explained in the next paragraphs. Conversely, please add the meaning of the acronym FoS.

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Page 6, line 16: intensity and occurrence in space? In time? Please explain.

Page 7, lines 4-30: please reduce this part.

Page 8, line 5: API is not defined.

Page 8, lines 16-17: this part should be deleted.

Page 13, lines 28-29: I would use always the same number of decimal digits, here and elsewhere in the text.

Page 14, line 14: the two selected RCP are not exactly contrasting, as reported. RCP4.5 is a “mid-way” scenario, while RCP8.5 is a “business as usual” scenario. If RCP2.6 and RCP8.5 were selected I would have said that they were contrasting.

Page 15, line 1: “increase in extreme precipitation events” is quite vague. I would specify if the increase is in the number, the frequency, or in something else.

Page 15, lines 7-10 (and figure 4): is this differentiation among solid and liquid precipitation useful?

Page 16, line 16: please note that you are mentioning Fig. 8 before Fig. 7. Please correct.

Page 17, line 1: I would add a reference to Fig. 6A after “translational typology” (that should be corrected into “translational type”). Analogously, I would refer to Fig.6B, 6C and 6D where the hazard of the other type is discussed after in the paragraph.

Page 17, line 20: only figure 7 show maps. Please correct.

Page 17, line 7: “the area that is susceptible to landslides”.

Page 18, line 15-16: I would move the definition of TPR and FPR before the description of ROC curve.

Page 20, line 11: “Climate conditions are constant and set to the actual conditions”. This sentence is not very clear.

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Page 28, line 19-20: correct “Future the land use”.

Page 28, line 28: I would replace “surface” with “percentage”.

## Tables

Table 2. If the table reports the “main predisposing factors of each landslide” – as the caption says – what’s the meaning of the first row reporting all the information on the landslides themselves? I would suggest deleting this first row.

Table 3. I would replace “Classification according to Houet et al., 2017” with “Classification (Houet et al., 2017).”

## Figures

Figure 1. In the caption I would correct “landslide inventory” with “examples of inventoried landslides”. Moreover, in panel E, I would suggest using non-overlapping values for the boundaries of the classes. Finally, check the font size to ensure readability.

Figure 2. I would add simple labels for the 4 scenarios, e.g. only Scenario 1, 2, 3, 4.

Figure 3. Also in this case I would use simple labels for the 4 scenarios, e.g. only Scenario 1, 2, 3, 4. Moreover, I would use only one decimal digit. Finally, in order to improve the readability of the graph, I would split it in two, let’s say up and down, according to the two years 2040 and 2100. The dotted lines could be removed.

Figure 4. I would suggest adding labels for RCP4.5 and RCP8.5 in the respective columns and for short- and long-term in the respective rows. The readability of the figure will benefit from this. Moreover, I would suggest using non-overlapping values for the class boundaries. As an example, a point with a mean annual precipitation of 1000 mm is in the first or in the second class? Please correct.

Figure 5. In order to improve the readability of the figure I would suggest using similar colors for 2021-2050 and 2071-2100, respectively. As an example: two shades of red for 2021-2050 (lighter for RCP4.5 and darker for RCP8.5) and two shades of blue for

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2071-2100 (lighter for RCP4.5 and darker for RCP8.5).

Figure 6. Please add labels for y-axes. The whole figure seems a bit compressed, please check. How the five hazard levels were defined?

Figure 7. Please note the caption of the figure says “hazard” while the legend says “susceptibility” and the values represent Factor of Safety. The two terms hazard and susceptibility are not synonymous! Please correct. Probably, “maps of FoS values” would be the best way to describe the map in its legend.

Figure 9. I would add two labels (2040 and 2100) in the two graph rows. Please note that the scalebar is not readable. I would suggest using a scalebar as in Figure 7, which is smaller (use only km instead of kilometers). Figures 10 and 11. Why not using the same symbology used in Figure 9 (only two classes, one for increase and another for decrease). I believe this would increase the immediate comprehension of the figures and the comparison among them. Alternatively, if the Authors want to maintain this difference, I would suggest using non-overlapping values for the class boundaries. As an example, a point with a FoS equal to -0.1 is in the first or in the second class? Please correct. Please note that the scalebar is not readable. I would suggest using a scalebar as in Figure 7, which is smaller (use only km instead of kilometers).

Figure 12. Please correct the x-axes labels (“pourcentage”). I would suggest using only one decimal digit. The legend seems a bit stretched and the green color in the legend seems different from the bars, please check.

Figures 13 and 14. Please note that the scalebar is not readable. I would suggest using a scalebar as in Figure 7, which is smaller (use only km instead of kilometers).

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2019-311>, 2020.

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