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Natural Hazards and Earth System Sciences
Discussions

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

Séverine Bernardie et al.

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Dear Referee #1,

Please find below and in attached file our answers to each question and comment you have provided for the review of the paper. I thank you for your relevant and interesting comments.

with best regards,

Séverine Bernardie

Anonymous Referee #1 Received and published: 28 February 2020

1. I've carefully read the manuscript "Modelling landslide hazard under global change:

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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.

The introduction will be shortened, but other additional elements may be introduced, in relation to Anonymous Referee #2 ; the description of the study area will be strongly shortened, as well as the abstract.

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

All the figures have been revised according to this relevant comment and the comments concerning some specifically figures.

3. 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 "methodol-

ogy" 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.

The paper will be modified with considering harmonised adequate terminology ; the terms land cover, shallow, moderately deep and deep landslide, hazard, method, type, and rainfall have been kept.

4. 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.

Concerning the terminology hazard/susceptibility, the paragraph Page 6 lines 14-16 is written to clarify and justify the choice of hazard ; to address this comment as well as the further comment on this subject, we will add some other elements, as indicated below : Landslide hazard assessment considers run-out, magnitude, and return period for a given intensity (Varnes, 1984). As in many cases, the hazard analysis is not completed. Notably, run-out is not accounted for in this study. Nevertheless, the landslide susceptibility assessment is converted into landslide hazard assessment by expert knowledge (Van Western et al., 2006, 2008, Corominas et al. 2014).

5. 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.

Among the few land use/land cover maps obtained, we have considered that those of 2040 and 2100 are the most representative of the period 2021-2050 and 2071-2100. It will be clarified in the paper in page 12 lines 10 - 11

6. 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

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review. This should be better clarified.

In the paper Vandromme et al., 2020, one of the principal subject introduced is the development of the strategy, for improving the accuracy of the calibration and the validation. It will be added in the paper Page 5, lines 24. Moreover, this paper is now published (https://doi.org/10.1016/j.geomorph.2020.107307)

7. 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 correct.

There were indeed a mistake : The majority of these landslides are considered active or may be triggered by rainfall (346 out of 426), the others being associated to more complex landslides, and not considered active any more. It will be clarified in the paper.

8. 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.

In the text it is written that four types are considered: i) shallow translational landslides, ii) shallow rotational landslides, iii) moderately deep and iv) deep landslides For better understanding we will add some terms in the text

9. 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)?

I completely agree with you that it is not explained why we provide the simulations for 10 ground water filling ratio ; the objective of this Figure is to demonstrate the sensitivity of the model to the evolution of the water filling ratio ; it shows that the evolution is different according to the 4 types of landslide ; moreover, we can see that the evolution is not linear, and that a threshold of behaviour appears in the results, depending on the type of landslide. Some explanations will be added.

10. 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.

We will provide additional maps, considering all landslides, in figure 7 and in figure 15.

11. 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.

Additional information on the definition of the classes of hazard will be incorporated (references), in particular this following table :

Landslide hazard class Value of simulation expressed in FoS Very high FoS $\leq$ 0.9 High 0.9 < FoS $\leq$ 1.1 Moderate 1.1 < FoS $\leq$ 1.35 Low 1.35 < FoS $\leq$ 1.5 Null FoS > 1.5

12. 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.

In section 5.2.2., the analysis of the impact of future climate on landslide hazard incorporate in fact the effect of land cover as well, since the scenario “abandonment of the area” is included in this analysis. Indeed the land cover map in 2040 is considered for the period analysis 2021-2050 ; the land cover map in 2100 is considered for the period analysis 2071-2100 ; this is not clearly explained, so we will modify the text accordingly. The results show that the effect of CC is more significant than the land cover ; that is why in the paper we have only presented the results with the combination of CC and 1 land cover scenario (scenario “abandonment of the area”), even if we have computed all the combinations.

Overall, I believe that the manuscript deserves publication after a thorough revision.

13. 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.

Done

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

Done and clarified – the abstract is anyway shortened and modified. Page 1, line 27: “significant despite being small” is also vague. Done and clarified – the abstract is anyway shortened and modified. Page 2, line 3: what do you mean with “small” landslides? This should be better explained.

It has been changed to “shallow”

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

Done

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

The “s” has been removed

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; Robinson et al., 2017 dx.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.

Done – I thank you for these relevant references

Page 3, line 18-24: I would suggest also reading the works by Gari-

ano 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.

Done – I thank you for these relevant references

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 that the cited ones.

Done

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

Done

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

Some explanations will be add in this part, to explain what is analysed in this study, and the choice of adequate term “hazard”.

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

This part will be entirely removed

Page 8, line 5: API is not defined.

Done

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

Done

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

Done, one decimal digit is considered

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.

Done – Yes we agree ; “contrasting” will be removed

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.

Done

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

This analysis considering both solid and liquid precipitation has been done, since this information is considered and analysed within the hydrogeological model Gardenia.

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

Done

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.

Done

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

Done

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

Done

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

Done

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

Reformulation done : “Climate conditions are considered equal between current and the 2 future periods

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

Done

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

Done

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.

Done

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

Done

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 read-

ability.

Done, except that we left “landslide inventory” as it constitutes the entire inventory of the area.

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

Done

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.

Done

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.

Done

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 2071-2100 (lighter for RCP4.5 and darker for RCP8.5). Done Figure 6. Please add labels for y-axes. The whole figure seems a bit compressed, please check. How the five hazard levels were defined?

Done – explanations of the 5 hazard levels will be indicated in the text.

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.

We removed “susceptibility” term, and left the term “hazard”; the explanations of this choice is indicated in Page 6.

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).

Done

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).

Done – we maintain the 4 classes, as the difference between maps are more important (until 0.2) than in figure 9 (until 0.1)

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.

Done

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).
Done

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-311/nhess-2019-311-AC1-supplement.pdf

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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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