Anonymous Referee #2
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The manuscript show a study of the influence of climate change on slope stability in a valley of the Pyrenees. Authors describe a very inspiring exercise integrating different inputs and models to simulate the effects on slopes' propensity to failure of possible future land use and precipitation scenarios. To do this, they use up-to-date tools based on spatially distributed models and perform a complete procedure to achieve their objectives. The article is a model for how such work could be conducted in other areas and is a suitable contribution for the journal.
I have only several corrections and suggestions that I proceed to expose.

1. There is a major problem with the terminology throughout the manuscript. Authors use the term "landslide hazard" but they did not estimated that in their study stricto sensu. Hazard implies spatio-temporal probability. Authors are really estimating the change in the Factor of Safety (FoS) (i.e. Slope Stability) of the slopes according to different conditions. It is true that the results of their calculations are spatially distributed and they are providing temporal information. Nevertheless, their model outputs are not the expected number of landslides per year and per area. The nature of the data have its implications because, for example, the FoS do not serve to estimate risk. If authors want to be precise, they have to use in the text and in the title the term "slope stability" instead of "landslide hazard".

Concerning the terminology hazard, the paragraph Page 6 lines 14-16 was written to clarify and justify the choice of hazard ; to address this comment as well as the further comment on this subject, we will had 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)

2. Many researchers have described how anthropic activities have high impact on the stability of slopes (cf. Glade, 2003; Remondo et al., 2005). Crozier (2010) state "Changes resulting from human activity are seen as a factor of equal, if not greater, importance than climate change in affecting the temporal and spatial occurrence of landslides". This is reasonable because slope modifications due to infrastructure construction or urbanization and significant land use changes produce great alteration on slope conditions. Please, discuss your results taking this paradigm in your mind. In the presented study area the human activities have a minimal disturbance to the environment, which may explain that the increase of precipitation due to climate change could have more impact than human action. This is not the situation in many countries, specially across the Global South. This idea must be stressed because, if not, other researchers can underestimate the human action over the physical medium.

On this topic we have discussed about the effects that are not considered in this approach (vegetation succion with runoff/infiltration balance). But I completely agree on your comment, that the modification of the land cover is quite small on this territory (as seen in figure 3). Moreover, some anthropic modification that may appear, such as slope modifications, are indeed not considered in this approach ; this will be stressed and argued in the discussion.

3. Authors explain in the introduction section that there are two ways to simulate future scenarios of landslide activity: physical and statistical models. They use an approach

based on physical modelling to investigate failure processes at regional scale.
I suggest authors to justify the selection of a physical model and discuss about other approaches. To do so, I suggest them to consult several papers about comparisons between physical and statistical models (e.g. Cervi et al., 2010; Zizioli et al., 2013; Davis and Blesius, 2015; Ciurleo et al., 2017; Bartelleti et al., 2017; Galve et al., 2017; Oliveira et al., 2017).

I thank you for these interesting papers, that will be added in this part of the paper ; we have indeed focused the discussion on the fact that physical models permit to quantify the impact of future climate and socio-economical scenario into landslide hazard ; but we will complete this part with integrating justification of the choice of physical models by providing some information on the performance of the models and the assumptions made.

4. In order to enrich the literature and discussion of the manuscript, I suggest authors to read the following papers dealing with the effects of land use change on landslide susceptibility and hazard: Vanacker et al., 2003; Van Beek and Van Ash, 2004; Reichenbach et al., 2014; Galve et al., 2015; Persichillo et al, 2017.

I thank you for these relevant papers. They will be included within the introduction section.

5. I also suggest authors to discuss about the application of their model and the extrapolation of their results to other regions (data and model requirements).

I completely agree with this comment ; this part is missing from the paper and will be added.

6. It is needed a large map where all the cited toponyms are included.

Finally, as the paragraph which concerns the description of the site has been strongly shortened, all cited toponyms have been deleted. Thus the map is not necessary now.

7. I do not like how authors describe landslide typology and morphologies. For example, they use "landslides with rotational shear surfaces, landslides with translational shear surfaces". Why are they using this long descriptions if they can use widely accepted landslide classifications such as Cruden & Varnes (1996) or Hungr et al. (2014)? Regarding the landslide associated landforms they use "(i) the landslidetriggering zone (LTZ) and (ii) the landslide accumulation zone (LAZ)" to designate parts of the mapped landslides. However the term accepted by the international community for their "LTZ" and "LAZ" should be "Zone of depletion" and "Zone of accumulation" (Varnes, 1978). The use of appropriate and widely accepted terminology avoid the necessity of explaining the not so widely used terms, as authors have to do in the second paragraph of page 8.

We will indeed refer to Cruden & Varnes classification and reduce the description. *The landslide triggering zone (LTZ) and the landslide accumulation zone (LAZ) will be replaced by "Zone of depletion" and "Zone of accumulation" (Varnes, 1978).*

8. Models seem to indicate that "Bare soils" are always stable. Please, explain that?

I agree, this is a mistake : this is “Bare rock”, and not ‘Bare soil” ; it has been changed in Page line.

9. I would appreciate a table with the model validation results and a figure with the ROC and PRC curves. How can explain the high performance of the models? In my opinion, the prediction capability is very good for a physical model applied at regional scale.

AUC has been calculated, as well as other indicators according to Brenning, 2005, and provided in figure 8 ; it permits by that way to validate the quality of the model. We consider that these indicators permits to quantify the performance of the model.

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OTHER COMMENTS

Table 2 "Defined using related literature based on field investigations". Local or global literature?

It concerns local literature ; it will be added in the table

Figure 1. - Colour landslide according to their type. - Add coordinates. - Authors only mapped active landslides? - Caption: Change "layer" and "layers" by "deposits".

This figure has been modified accordingly. Colouring the landslide according to their type was not really visual in this figure. But in figure 7, all the landslides are separated between the 4 types.

Figure 2. - Change "Mineral surfaces" by "Bare Rock" (as Corine Land Cover terminology)

Done

Table 3 How was additional cohesion calculated?

The additional cohesion is determined from literature (Wu et al., 2004 ; Norris et al., 2008). It will be added in the paper.

Section 3 "Gave" is a term used for creeks or streams in the western Pyrenees. Please, change the term to the appropriate English word or define "Gave" in the text. Please, define what the "Soum de Grum" and the "Grand Barbat" are. Are they a place, an area, a district, a landform?

This section has been shortened as asked by the first referee. Thus all these terms have been removed.

Page 4 Line 25. What is GIEC?

It has been replaced by IPCC.

Lines 29 and 31. Assign citations to ALICE and GARDENIA tools.

Done

Page 6 Line 21. What are the RTM services?

RTM means Restauration des Terrains de Montagne; it is the French survey of hazard and forest management in mountainous territories, and constitutes a part from the French Forest Office (ONF). It will be added in the paper.

Page 9 Line 10. Please, define "moraine colluviums"?

There is a mistake in the text : it will be replaced by “moraine deposits or colluviums”

Page 14 Line 15. Add a citation to ALADIN-Climate model of Météo-France.

Done

Page 16 Line 14. Please, explain the method applied to define the hazard classes.

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 ≤ 0.9
High	0.9 < FoS ≤ 1.1
Moderate	1.1 < FoS ≤ 1.35
Low	1.35 < FoS ≤ 1.5
Null	FoS > 1.5

Page 17 Reducing the first paragraph could make the reading more fluent.

Done

Page 18 Lines 3-16 aprox. This is an explanation of the validation techniques and it may be displaced to the metholodology section.

Done

In this regard, how were no-landslide/stable points selected to produce ROC curves?

In this approach, we consider the class very high and high hazard that can be compared to landslide inventory ; it corresponds to FoS < 1.1. It will be added in the paper.