Interactive comment on "Inferring the destabilization susceptibility of mountain permafrost in the French Alps using an inventory of destabilized rock glaciers" by Marco Marcer et al.

Authors' response to Anonymous Referee #1

We wish to thank referee #1 for the valuable comments and effort put in this constructive revision. We believe that the study has significantly improved thanks to this contribution. Please find below the specific responses to each comment.

General Comments

This paper uses a generalized additive model to model rock glacier stability resulting in a susceptibility map highlighting potential areas of permafrost destabilization. This is possible as the stability of rock glaciers is a reflection of the underlying permafrost stability. Results from this research are an important contribution to improving our understanding of periglacial risk and of broad significance in mountain permafrost regions. However I do have a number of comments that should be addressed before moving forward with publication.

Overall, we agree with the concerns of the reviewer. We made few major revisions in agreement to the referees' comments:

1. The perspective of the study has been changed as we don't talk anymore about general permafrost destabilization nor degradation. We agree with the referee that rock glacier destabilization is not representative process for permafrost degradation as destabilization may have external trigger and is preconditioned by geometrical factors. The study focuses now on rock glacier destabilization and understanding these preconditioning factors. The definitions used in the manuscript have been modified in agreement to this. The manuscript title has been changed accordingly.
2. We decided to delete the section relative to the measurements of rock glacier displacement rates. The section does not fit with the study and creates confusion with the general purpose of the manuscript.
3. Debris flow gullies are not considered surface disturbances anymore as they are not linked to destabilization. Destabilization rating and susceptibility map have been updated accordingly.
4. Rock glaciers showing destabilization linked to cracks were separated from rock glaciers showing destabilization linked to crevasses and scarps. This was done to acknowledge the fact that we are not completely sure about the significance of cracks and crack clusters in the destabilization process. See P5L30 for more details.
5. Basic lithological analysis has been introduced (added a new table)

The introduction could be improved. The relationship between rock glacier stability and permafrost destabilization needs to be more concisely and clearly presented.

The introduction has been significantly improved. Considering the remarks from the referee #2 we decided to avoid to infer a relationship between rock glacier stability and permafrost destabilization, as at the current state of the art we cannot support this hypothesis. The study entirely focuses on rock glacier stability now.

Some key information about the study area is missing such as general information on ice content and active layer characteristics. There are a number of sentences that allude to its importance as observed from other papers but there is nothing specific to this paper. If this data does not exist this should be acknowledged.
Ice content and active layer are characteristics strongly varying in rock glaciers also at the site scale. Allusion to the importance of these characteristics have been removed as they tried to relate rock glacier stability and permafrost destabilisation (see answer above).

It may be useful to have a Data section in the Methods where you introduce the different orthoimagery datasets and their time periods, resolution and source, and, the DEM, including its resolution, source etc. I found myself having to jump around to look for this information.
Thank you for the comment. Nevertheless, we would prefer keeping this format as data are necessary to explain the methods (e.g. orthoimages used to map destabilization). Also, since the section concerning the displacement rates has been deleted, data explanation within the sections is not redundant anymore.

The methods explaining the can be improved. The introduction of the GAM is ok, but it is missing some critical information. For the GAM, what type of smoothers were used and did you control for their flexibility, what were the degrees of freedom? This information is important particularly for overfitting which depends on the flexibility of a smoother, which can be controlled by the degrees of freedom. In the methods you state you will examine the accuracy of the model using sensitivity and specificity, this is not followed up on in the results.
We would like to thank the reviewer for pointing us to this omission. The following text was inserted in section 2.3:

"All numeric predictors were represented using spline-based smooths, for which we chose a maximum basis dimension of 4 in order to limit their flexibility and reduce overfitting. The actual degree of smoothness of the spline smooths is determined by a generalized cross-validation procedure (Wood, 2017)."

Some degree of overfitting to the training set is of course always present, which is why independent test sets are needed for model performance estimation. We addressed this issue using spatial cross-validation (see Methods and Results). We reported the cross-validation test-set AUROC of 0.76, which is lower than the AUROC of 0.86 obtained on the training set; the latter value was not reported because it is over-optimistic.

Some more work needs to be done to better lay out the relevance of the selected terrain variables and their connection to rock glaciers/permafrost degradation. You try to link permafrost degradation to rock glacier activity and while you do provide evidence of this through the literature the link within your own study is not clear. I assume you are using the terrain variables (i.e. PISR, slope etc) as possible proxies for surface/ subsurface conditions i.e. ice content, active layer thickening: : : etc.? Often terrain variables are selected as they serve as proxies for surface conditions that are difficult to represent through spatial datasets, however this connection is not well made throughout the methods, results or discussion.
A more precise explanation to the relationship between rock glacier destabilization and terrain attribute has been added in section 2.3.2 and in the discussion section 4.2 :

"The relationships withpredictor variables were found to be consistent with topographic settings observed in known cases of destabilization. Highslope angles are suggested to increase internal shear, making the landform more susceptible to destabilization (Schoeneich et5al., 2015). Convex slopes cause an extensive flow pattern as creep velocity is higher downslope the convexity (Delaloye et al.,2013). This is suggested to cause a thinning of the permafrost body and the generation of traction forces that may enhance the occurrence of surface disturbances. The PTP was found to be a significant predictor of potential destabilization. In particular, increasing potential in permafrost thaw was linked to increase susceptibility of destabilization, indicating that destabilization was more likely to occur where the permafrost zone was expected to be thawing. This seems to be consistent with the relation between destabilization and elevation, as potentially destabilized rock glacier as more often located around 2800 m.a.s.l., which roughly coincides with the lower margins of the regional permafrost zone."

The way you present the methods used to select the points to build the model is unclear. You say 5 randomly selected points (the size is not stated) were used to extract terrain information from each rock glacier, and that this was done as there were only 58 potentially unstable rock glaciers compared to 79 suspected destabilized rock glaciers and 119 unlikely destabilized rock glaciers. You also state that the response variable, which is representative of stable and unstable rock glacier zones were then assigned a defrost index of 1 or 0. Where those 5 points, which I assume were classified as potentially unstable, collected to increase the unstable (1's) inventory? If yes, and if you then extracted the terrain information for each of those 5 points, it's possible that the model is biased towards terrain information specific to the potentially unstable rock glaciers represented in your study. This could reducing the overall usefulness of the model when applied to the rest of landscape or perhaps lead to an overestimation of areas modeled as potentially unstable.

Methods presentation has been improved taking into account the reviewer's remarks. Points are extracted from a point grid at 25x25 m resolution (i.e. one point per raster pixel, P7 L14). The five points are extracted from both potentially destabilized and stable/likely stable rock glaciers. Section 2.3.1:

"Polygons of both unstable and stable areas were sampled using a 25 m x 25 m point grid in order to assign the response variable to the modelling database. The point values were then used as binary response variable with values of 0 for stable areas of (likely) stable rock glaciers, while 1 was assigned for unstable areas of potentially destabilized rock glaciers in the modelling stage."

The size of inventory used for modelling is now stated, making clear that the model is not biased towards destabilization. Section 2.3.1:

"Overall, the model was computed using 225 evidence of instability and 1785 evidence of stability."

The addition of a table outlining the size of the inventory (1's and 0's) that was used to build the model would be very helpful. This may make it easier to present the different methods i.e 5 points being extracted from unstable rock glaciers. Currently, it is not easy to get a handle on the size of the inventory and how this is integrated into the model.

Thank you for the comment, size of the inventory is now added (see answer above)

Specific Comments

P1 L3 The description of the imagery used in the abstract does not match with what is presented in the methods. In the following sections you state you have imagery from 2012-2013.

Here it is specified that the orthoimages collection has frames from 2000 to 2013. In the methods it specified that the orthoimages belong to three main sub-period: 2000 – 2004, 2006 – 2009 and 2012 - 2013.

P1 L6 Be more specific on the time periods being used to compare the deformation patterns.
It is now specified that deformation patterns are observed on the “available orthoimages”. Time span is presented in the pervious sentence. Abstract:

“At first, using recent orthoimages (2000 to 2013) covering the study region, we mapped the geomorphological features that can be typically found in cases of rock glacier destabilization (e.g. crevasses and scarps). This database was then used as support tool to rate rock glaciers destabilization. The destabilization rating was assigned also taking into account the surface deformation patterns of the rock glacier, observable by comparing the available orthoimages, and the type of morphological features involved.”

P1 L11 On P9 L31 you state that the slopes associated with higher destabilization rates is 20-40 however here in the abstract you only state that up to 30. Which is correct?
Thank you for noticing, this has now been corrected to 25 – 30 ° (accordingly to model modifications).

P2 L16 What methodology? No methodology is mentioned but I assume you are referring to methodology in the Sattler et al. paper? Suggest stopping sentence at “: : :degrading permafrost” or briefly layout methods of paper that show that the initiation points of debris flows weakly correlate with the spatial footprint of degrading permafrost.
This part has been removed in the new version and the issue of “spatial footprint of degrading permafrost” is no longer treated.

P4 L5 Additional details on the multi-temporal orthoimagery should be included i.e. dates, resolutions, source (satellite/airplane/UAV). Is this the same imagery that is introduced in section 2.2, if yes, some effort should be made to rework/better connect this information.
This explanation has been removed as non-significant as already Marcer et al. 2017 provided to select only moving rock glaciers and in this manuscirpt data from that study are directly used.

P4 L5 I see in the discussion you address errors associated with your mapping however, you should include a sentence in the methods explaining how you plan on assessing this.
Yes good point, this is now stated early in the methods/ Section 2.2:

“Nevertheless, several limitations during the mapping process were encountered, as image distortion or illumination, and will be discussed in section 4.4.1.”

P4 L5 What features are you using to attribute a classification of active or inactive?
Moving rock glaciers are considered as active. In Marcer et al (2017) activity is attributed by observing movements on orthoimages collections.

P4 L6 What was the final inventory of active rock glaciers? This should be included here.
Added the sentence to specify the inventory used and the number of active rock glaciers. Section 2.1:

“This inventory compiled between the years 2009 – 2016 by inspecting aerialimagery in Geographical Information System (GIS)and revised by Marcer et al. (2017), revealed the high incidence of active rock glaciers in the region (i.e. 493 landforms). This inventory was used in the present study to identify active rock glaciers locations and investigate the occurrence of destabilization”

P4 L13 Can you be more specific or provide better descriptions of the different surface disturbances, their morphology and triggering causes. As it currently reads, it is very vague.

Specific description of each surface disturbance is provided in Table 1 and Figure 1. This reference is now made more explicit in the text. Section 2.2

“Surface disturbances are described in detail in Table 1 and illustrated in Figure 2.”

P4 L 222 In the Figure 1 caption you say you used UAV images to map distinct destabilization features. But when I read the description here, there is no mention of UAV imagery. While the imagery you state you have is fairly high resolution, mapping cracks with 2 m imagery vs. mapping cracks with 50 cm imagery can be quite different. Were there resolution issues when mapping? How confident are you that you mapping all the features equally? Do you have any metrics on your mapping error? I see that you acknowledge this in your Discussion but like I mentioned in an earlier comment, a couple of sentences should be included in your methods addressing how you plan to do this.

Thank you for pointing out this lack of clarity. UAV-retrieved hillashades were used here with the only intent to show the characteristics of the different surface disturbances. Nevertheless, we understand that this can create confusion. We therefore preferred presenting field images of the surface disturbances (new figure, now Figure 2). Concerning the mapping error, figure 2 has the intent to show how the smallest surface disturbances (i.e. cracks) look like on the orthoimages used for mapping, showing that these features can be identified. We made an error in describing the resolution of the orthoimages: older orthomosaics are at 1 x 1 m, not at 2 x 2 as stated before. We anticipated at section 2.2 (see answer above) the upcoming discussion on the issue concerning the challenges in mapping surface disturbances.

P4 L25 What is the biggest factor, image quality or the availability of multi-temporal images? What is the minimum resolution needed to map these features, are your images high enough resolution?

That is quite difficult to declare in general, as the distortion may make useless images with proper lightning by creating unrealistic creeping patterns. There is a whole spectrum of difficulties that can be encountered. Nevertheless, the “methods” section may not be the best moment to start treating the subject. Difficulties concerning mapping are discussed later and now the reader is made aware of this upcoming discussion (as you suggested, see comment above). Concerning the image resolution, as specified above, a dedicated figure (2) has been added.

P5 L5 Oblique photography? This is the first time this is mentioned, is this different than the imagery presented earlier?

Thank you for noticing. That was a mistake, now it is removed.

P5 L7 You finished the previous paragraph stating that you will present a slightly different definition of a destabilized rock glacier. I think the start of this paragraph should begin with a clear and concise explanation of your definition followed by the examples/ observations you use to support it. As it currently reads I do not actually know what your definition a destabilized rock glacier is. A definition needs to be presented before you describe your destabilization rating on L24.

We agree that the text was rather confusing concerning this issue. The text has been modified in order to provide a clear definition of destabilized rock glacier already in the introduction:

“While active rock glaciers commonly present moderate interannual velocity variations that correlate with the ground temperature (Delaloye et al., 2008; Kellerer-Pirklbauer and Kaufmann , 2012; Bodin et al., 2009), destabilized rock glaciers are characterized by a significant acceleration that can bring the

landform, or a part of it, to incredibly high velocities (Delaloye et al., 2013; Roer et al., 2008; Scotti et al.,2016; Lambiel, 2011; Eriksen et al., 2018). During this acceleration phase, morphological features typical of sliding processes,as crevasses and scarps, appear and grow on the rock glacier surface. This suggests that the destabilization consists of the onset of a basal sliding process over the normal creep of the rock glacier (Roer et al., 2008; Schoeneich et al., 2015). In this sense, crevasses and scarps are interpreted as the possible transition between the creep-driven and the sliding parts of the landform (Roer et al., 2008). This acceleration phase, also referred as "surge" (Schoeneich et al., 2015) or "crisis" (Delaloye et al., 2013), may last decades and it resolves in a deceleration or inactivation of the landform. Exceptionally, destabilized rock glaciers may collapse in a landslide (Bodin et al., 2016)."

P5 L24 Be more specific, what combination of surface disturbances/qualitative assessment of recent deformation patterns merits a rating of 1 or 2: : :?

Specific description of each destabilization rate is provided in Table 2. This reference is now made more explicit in the text.

P5 L29 What was specifically modelled in arctic permafrost, this description is very vague.

It is now specified that permafrost slope failures were modelled. Section 2.3

"The modelling followed a statistical approach similar to previous spatial prediction studies on landslides (Goetz et al., 2011) and arctic permafrost slope failures (Rudy et al., 2017) that used the Generalized Additive Model (GAM) with logistic link function"

P6 L5 If these 5 points were extracted from a potentially unstable rock glacier (1- presence), I assume all 5 points were represented as 1's in the model?

We agree that the text was not clear enough and we made substantial changes. In the text it is now specified that 5 points were selected within each rock glacier perimeter. Points in unstable area belonging to potentially destabilized rock glaciers were 1's. Points in stable areas belonging to stable and likely stable rock glaciers were 0's (see answers above).

P6 L5 What size were the multiple points that were extracted from the rock glaciers. Are you confident there is no overlap of these points? You say that model performances stabilized for more than 5 points, how was this discovered? What was the minimum and maximum number of points used? Was model performance assessed for the different number of points using only the best model?

We are not sure to understand what it is mean by size of a point, as a point is defined only by a coordinate. Points are unique for each raster pixel and assigned a unique ID that ensures no overlap. Performance stabilization was discovered using an explorative analysis of model performance's sensitivity with respect to point sample size per rock glacier. Point sample size varied between 1 and 10 points per rock glacier. Yes that was done using only one model (i.e. with elevation instead of PTP). A more detailed description of the process that led to the choice of the sample size of 5 points per rock glaciers is now added at section 2.3.1

"Since the rock glacier inventory counted a relatively small number of potentially destabilized cases (46 individuals), selecting only one point per rock glacier would have caused large uncertainty in the model outcome. It was therefore performed a simple exploratory analysis aimed to identify a proper amount of points per rock glacier to be used in modeling. Multiple points, from one to ten, were randomly selected within each rock glacier perimeter and used to compute a model. This was repeated ten times per each point sample size, in order to measure the variability of the model performance in relation to size of the point sample per rock glacier. Since model performances were found to stabilize for more

than five points selected per rock glacier, this number of points was randomly extracted per rock glacier for modelling."

P6 L11 Was the forward and backward stepwise variable selection not used to select the best multiple variable model (model with the lowest AIC)? The way it currently reads is that you populated a number of models with different combinations of variables, found the one with the lowest AIC and then used stepwise (forward, backward) variable selection to Identify which predictors were the best in the what would already be the best model. Suggest rewrite for clarity.

Thank you for noticing the confusing section. Section is now made clearer by avoiding the repetition of using the AIC to find the best model. Section 2.3

"The multiple variable models were computed using different combinations of predictor variables. Different models were compared using the Akaike Information Criterion (AIC), which Is a measure of goodness of fit that penalizes more complex models. The best multiple variable model was selected by iterating a backward-and-forward stepwise variable selection, aimed to identify which combination of predictors was better at describing the response variable by means of lower AIC."

P6 L18 What was the size of each cluster, how many rock glaciers in each?

Clusters are created by dividing the dataset in five groups of equal size.

P7 L5 Assigning your response variables a DEFROST index and then assigning a DEFROST index to your DEFROST susceptibility map decreases the clarity of inputs and outputs for the model. I would refer to your response variables as either values of 0 for stable or 1 for potentially destabilized rock glaciers

Agree, the DEFROST acronym is now avoided (as also not fitting anymore with the aim of the study).

P7 L8 What was your final count of 1's and 0's used to train the model? This needs to be included in the text

Agree, count of 1s and 0s is now added (see answer above)

P7 L11 Probability of thawing permafrost?

We cannot define it as probability as not constructed on a statistical method.

P7 L15 The DEM is a bit on the course side, do you think this could have affected your results? What is the average size of the rock glaciers? This general information of size, stability, mapped destabilization features should be included as a table

Coarse DEM is not considered to have affected too much the results compared to others sources of uncertainty. Rock glaciers are large features compared to the DEM resolution. Too high resolution could be disturbed by local features of the rock glacier surface (as large boulders or crevasses) leading to non-representative values of, for example, slope and PISR. Rock glacier size in the region is not exceptional compared to others regions. Mapped destabilization features and stability are already presented in table 4.

P7 L20 Would there be a complete absence of snow cover throughout the summer? For what periods of the year was PISR computed?

Snow free periods are reduced to few months in summer. PISR has to be considered as proxy of several processes, involving as well snow cover duration (which is higher at low PISR values).

P7 L21 – 34 I don't think the description of PFI should go in the Model predictor variable section. It is not actually used as an input to the model but is almost used a threshold for which you limit your resulting susceptibility map to.

PFI is introduced here to specify how the PTP is evaluated (and used as predictor variable).

P7 L21 It doesn't appear that you evaluate potential permafrost thaw using analytical methods presented by the others (cited in text) in this paper. You state that "Here, we used the Permafrost Favourability Index: : :.). I would remove the first part of the paragraph as it doesn't appear useful in the context of this paper. Also was the PFI recomputed specifically for this paper or are you using the results from your 2017 paper. If you are, you do not need all of the details that you include in this section.

We did use the same method proposed by the others (cited) and applied to our data, represented by the PFI. The PTP for this region however is novel and computed for this paper specifically and therefore need to be specified with some detail. Section 2.3.2

"The spatial distribution of degrading permafrost was evaluated following the method already presented by other studies(Hoelzle and Haeberli , 1995; Lambiel and Reynard , 2001; Damm and Felder, 2013), which consisted in artificially shifting a permafrost map proportionally to the estimated climate warming occurred between the period of validity of the map and the current climate. Here, as permafrost distribution map of the region we used the Permafrost Favourability Index (PFI) map (Marcer et al., 2017). The PFI map was calibrated using active rock glaciers as permafrost evidence and it represents the permafrost conditions during the cold episodes of the Holocene, e.g. Little Ice Age (LIA). The climate warming between the years 1850-1920 and 1995-2005 was determined using the HISTALP database (Auer et al., 2007) over the region. A permafrost distribution map was then recomputed taking into account of these temperature variations and represented the theoretical permafrost distribution in equilibrium with the current climate. By comparing this theoretical permafrost distribution and the PFI, it was obtained the Potential Thawing Permafrost zone (PTP, i.e. the so-called "melting area" in Lambiel and Reynard (2001)). In order to use the PTP as predictor variable, it was represented by an index ranging between 0, i.e. no thaw expected, and 1, i.e. potential thaw."

P7 L29 What do you mean when you say the "The resulting map, which corresponded to a theoretical permafrost distribution in equilibrium with the current climate, was finally subtracted from the PFI, obtaining the Potential Thawing Permafrost zone"? In the previous sentence you say the PFI map was recomputed using the model parameters and then in this sentence you say the resulting map was subtracted from the PFI: : :? If you are subtracting two PFI maps, how are these maps different?

The PFI map is representative of the LIA climatic conditions. The new permafrost distribution map computed by taking into account the climate warming occurring since the end of the LIA. Therefore the two maps differ as at the LIA permafrost equilibrium with climate was reached at lower elevations than nowadays. The section has been rephrased to make this concept clearer (see above).

P8 L28 Suggest you move the sentence, "This indicates that more than 50%...." to after the next sentence where you actually provide the percentages of unstable glaciers.

Deleted the words "this indicates" as they suggest that this sentence is a consequence of the previous one (which is not the case). Section 3.1:

"More than 1300 surface disturbances were digitized, involving 259 active rock glaciers (Figure 6). Overall, more than the 50% of the active rock glaciers may be affected by some degree of

destabilization as 46 rock glaciers (9.7%) showed potential destabilization, 86 (17.0%) were suspected of destabilization and 127 (25.7%) were unlikely destabilized."

P9 L10 This is just personal preference but it makes the paper easier to follow if the results are presented in the same order as they are presented in the methods. Methods related to section 3.2 were the final section of the methods but here are presented as the second set of results.
Yes, now the methods and results are in the same order.

P9 L10 How many glaciers was this analyse done on?
P9 L11 If the orthoimages have resolutions ranging from 0.5 to 2 m how are displacement rates of 0.3 m/year detected and how does this correspond to _3-5 pixels? Is this possible because dates have been grouped and zero movement is inferred in the missing years. If not, how are you accounting for movement in the years you don't have imagery?
P9 L12 What was the limit in distorted orthophotos?
P9 L15 It appears as though you have grouped the first two periods, 2000 – 2004 and 2008 – 2009. This should be stated in the text. Again, how are you inferring movement for the years you do not have data? It seems like a bit of a reach to present the second period as 2009 – 2013 when you only have data from 2012 – 2013. That's a big chunk of time with no data.
Thank you for the comments even though we deleted the related part.

P9 L21 Unsure as to why the PTP model results are with the modelling results?
True, sentences deleted. These general information about the PTP features are furnished already in the methods now. Section 2.3.2:

"It is emphasized that PTP is only a proxy of permafrost degradation, which occurs at all the elevations while the PTP zone consists in a belt of 250 to 300 meters elevations that affects about 50% of the lower margins of the permafrost zone"

P9 L25 Earlier you mention that you also used sensitivity and specificity to assess the model performance, where are these results?
Yes. The mention is now removed as we did not actually used them in the assessment.

P10 L8 Why do you think the model is overestimated these areas? This should be expanded on in the Discussion section on the susceptibility model.
Very good point, as talking about "overestimation" is incorrect. Section 3.3

"The susceptibility predicted high destabilization susceptibility in areas belonging to stable rock glaciers."

P10 L18 Do you think that adding a surficial geology variable or a variable that highlights jointed bedrock would be useful?
Good point. We added a discussion and table to highlight the relationship between destabilization rate and lithology (and yes it is useful, thank you for the comment). In section 4.1:

"In these areas the densely jointed lithology was suspected to generate mainly pebbly rock glaciers (Matsoukaand Ikeda , 2001; Ikeda and Matsuoka , 2006). This suggested that destabilization may be more likely to develop in pebbly rock glaciers, as observed in the Berard, Roc Noir and Lou rock glaciers. Also, no rock glacier developed in crystalline lithology showed potential destabilization. However, recognizing surface disturbances on pebbly rock glaciers may be easier than in "blocky" rock glaciers, as smaller cracks are more evident. This may create a bias which should be studied more in detail."

P11 L 2 – 16 This is great and addresses a number of my prior questions. It is great that you acknowledge the challenges but are you able to quantitatively provide an idea of the error? Was any field validation done for any of the mapping?

No we are not able to provide a quantitative assessment as our field validation was reduced to very few sites (mostly presented in the new Figure 2). For this reason, it is emphasized in the conclusions to include future surveys in the inventory in order to spot systematic biases or other errors.

P11 L30 Multiclass AUROC, what do you mean here. Velocity isn't used in the model so if you evaluated other variables in a different way that should be presented in the methods and the results.

Thank you for the comment. The section and relative issue have anyways been removed.

P12 L8 This sections needs to be strengthened. What else to these variable tell us about process? I already made this comment in the General Comments section but I will state again that more work needs to go into explaining the importance or the terrain variables or the potential surface processes they represent.

Thank you for the comment. A more detailed explanation has been now provided both in section 2.3.2, and in the discussion section 4.2 (see answer to general comments).

P12 L12 Was PTP strongly correlated with the DEFROST index? At what susceptibility class was it most correlated with i.e. are areas modelled as high susceptibility the areas where the permafrost belt is expected to be thawing most?

Spearmann correlation is equal to 0.246. The correlation is positive, indicating that areas susceptible to destabilization are found more likely in zones with high potential of permafrost thaw.

P12 L15 Is it possible that north-facing slopes may have greater ice contents closer to the surface in part due to increased soil saturation but possibly also due to shallower active layer depths? In your area do you know how ice content varies or if there is a relationship with aspect? Also, did you look at the terrain information for your glaciers? Are there more glaciers perhaps on north-facing slopes? This would be worth looking at and presenting.

Yes it is possible but we don't have systematic information about it. There is no available information of ice content in relation with aspect and there may not exist a correlation at all. For example, the Lou rock glacier, despite being uniformly north facing, presents very varying ice content, due to glacial/periglacial interactions mainly. Also, on this rock glacier, high ice content in areas that are not destabilized and ice-versa. Yes there are more rock glaciers on north facing slopes (low PISR), as observable in figure 7.

Technical Corrections

P1 L12 Model performance should be singular

Corrected

P2 L13 Do you mean altitudinal?

Sentence deleted

P2 L9 change relationship to relation

Corrected

P2 L15 Suggest remove permafrost from permafrost initiation points: : :

Sentence deleted

P2 L23 Remove "so-called"

Done

P2 L24 I would suggest changing dynamical to dynamics throughout. I recognize that this term may be more common in your field but it doesn't fit in every place it is used.

Word not used anymore

P3 L19 1500 m a.s.l

Removed (as suggested by RC2)

P4 L3 Improve the sentence: : : “Although activity: : :”

P4 L4 Suggest changing the word “noticed”

Sentences modified

P5 L23 patches

Did not find the referred word

P5 L31 Remove (response variable) is it redundant.

Done

P7 L30 Suggest changing The so-called “melting area”: : : to the “thawing area”

“Melting area” refers to how it is defined in Lambiel 2001. Here, later in the study it is referred as (potentially) thawing permafrost.

P8 L2 remove so-called, what else would the map be called?

Removed

P8 L28 : : :involving 256 active rock glaciers

Done

P8 L23 vegetation patterns

Section removed

Figures

A study area map is needed. You refer to a number of specific locations ex. P9 L1 Vanoise National Park. The study area map should include these areas to put this research into context. You could possibly use Fig 4 but this figure needs to be put into a larger geographical context as well.

Thank you for the advice. A new figure has been added presenting the study area, cited mountain ranges and periglacial characteristics (new Figure 1).

Figure 1 – The hillshades and orthophotos were both acquired by UAV imagery? This is the type of imagery that is available for the different temporal periods for all of France?

UAV images are no longer presented in (now) Figure 2 as confusing. To answer your question, these were data we acquired locally to investigate geomorphometry of surface disturbances.