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.

Anonymous Referee #1

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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.

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

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.

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.

The methods explaining the models 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.

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.

The way you present the methods used to select the points to build the model is un-

clear. 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.

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.

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.

P1 L6 Be more specific on the time periods being used to compare the deformation patterns.

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?

P2 L16 What methodology? No methodology is mentioned but I assume you are re-

ferring 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.

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.

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.

P4 L5 What features are you using to attribute a classification of active or inactive?

P4 L6 What was the final inventory of active rock glaciers? This should be included here.

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.

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.

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?

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

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.

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

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

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?

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?

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.

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

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

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

P7 L11 Probability of thawing permafrost?

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

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

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.

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.

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?

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.

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.

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.

P9 L21 Unsure as to why the PTP model results are with the modelling results?

P9 L25 Earlier you mention that you also used sensitivity and specificity to assess the model performance, where are these results?

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.

P10 L18 Do you think that adding a surficial geology variable or a variable that highlights jointed bedrock would be useful?

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?

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.

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.

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?

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.

Technical Corrections

P1 L12 Model performance should be singular

P2 L13 Do you mean altitudinal?

P2 L9 change relationship to relation

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

P2 L23 Remove "so-called"

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.

P3 L19 1500 m a.s.l

P4 L3 Improve the sentence. . . "Although activity. . ."

P4 L4 Suggest changing the word "noticed"

P5 L23 patches

P5 L31 Remove (response variable) is it redundant.

P7 L30 Suggest changing The so-called "melting area". . . to the "thawing area"

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

P8 L28 . . .involving 256 active rock glaciers

P8 L23 vegetation patterns

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.

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?

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Figure 2 – Add arrows to the 1990 and 2003 images to point out changes as well

Figure 3 – Clean up spelling. . . Surface Disturbances doesn't need to be capitalized

Figure 4 – Refer to this more in the text

Figure 5 – It doesn't seem as though the boxplots and the scatter plot are both needed. The scatterplot does a good job portraying the differences in velocity between the periods

Figure 6 – The hillshade on the DEM makes the landscape look inverted, higher elevation areas appear as valleys. This should be corrected. The circles representing the destabilization rating are difficult to pick out for a lot of the rock glaciers, particularly those with a lower rating, I would suggest making the outline of each point thicker and perhaps a darker colour.

Tables

Table 3 – Beside each descriptor put the numerical destabilization rating

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