# 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 #2

We wish to thank referee #2 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**:

*Good paper on an subject gaining in importance in the understanding of the behavior (stability) of mountain debris slopes underlain by permafrost : the so-called destabilization of rock glaciers. On the basis of an extended original dataset the authors spatially model the susceptibility of a location to be affected by a destabilization process in the French Alps. The preparatory work of inventorying the destabilization indices on rock glaciers in the entire French Alps is impressive and constitutes for me the most attractive part of the paper. The statistical modelling approach appears to be good, but can only provides results which are very difficult to validate in my point of view. It is so partly less convincing. Some of the terminology used in the paper (permafrost destabilization, rock glacier destabilization, stable/unstable rock glacier, hazardous rock glacier) is somewhat unclear and even questionable. It has to be checked carefully all the paper along. I also have some questions about the interpretation/use of some the destabilization indices. Maybe some of the results may change in accordance. After having been revised the paper will be definitely very worth of being published in TC. I hope my comments/suggestions can be useful for the authors.*

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.
5. Basic lithological analysis has been introduced

**Specific comments:**

*Title is not good. "Inferring the destabilization susceptibility of mountain permafrost: : :" has no real sense, permafrost being a thermal phenomenon. This is not the scope of the paper, which is conversely dealing with the mechanical "destabilization" of rock glaciers. There is however a much better alternative proposed by the authors on P3 L15, which can give a title like "Evaluating the destabilization susceptibility of active rock glaciers in the French Alps".*

Agree. Title changed to "Evaluating the destabilization susceptibility of active rock glaciers in the French Alps"

*Abstract : may have to be adapted after revision of the paper*

Abstract adapted to the revisions

*P1 L17 Express what is meant precisely by (widespread) permafrost degradation. This is not clear at all, but a very important concept for this paper. Check then in the whole paper if the concept is used always exactly with the same sense. Prefer "Permafrost has shown signs of w. degradation for the past decades in the European Alps".*

A better definition and explanation of permafrost degradation is now provided in the introduction:

*"Warmer mean annual air temperatures (IPCC , 2013) are linked to a general trend of increasing permafrost temperature (e.g.Harris et al., 2003) and water content (e.g. Ikeda et al., 2008) causing permafrost degradation, a phenomenon widely observed inthe European Alps (Haeberli et al., 1993, 2010; Springman et al., 2013; Bodin et al., 2015). Permafrost degradation occurrence is dependent on the ground properties, snow cover interactions and permafrost ice content (Scherler et al., 2013) and is therefore an heterogeneous phenomenon. Permafrost grounds affected by degradation experience a loss in stiffness due to the increasing ice ductility and reduced internal friction caused by the warmer ice and increasing water content (Davies et al. , 2001; Haeberli et al., 1997; Harris and Davies , 2001; Nater et al., 2008; Huggel et al., 2010).Abnormal rockfall activity at high elevations (e.g.Ravanel and Deline , 2010) and increasing rock glaciers displacement rates (Delaloye et al., 2008) are indicators of this changeof state in the mountain permafrost"*

*P1 L18 : The connection between air temperature and ground temperature is tricky (snow buffering effect) and I do not really understand the meaning of "extreme warm air temperature" in the sentence. I would suggest to simplify it as "Warmer climatic conditions are expected to cause: : :" (eventually is a redundancy and can be omitted). Is there not more recent and more adapted references ? Finally, I do not see the link with the previous sentence.*

It is now acknowledged that ground properties and snow cover have a significant impact on the connection between air temperature and ground temperature (see above). "Extreme warm air temperature" omitted. The whole section has been re-arranged since, as you suggested, there was poor connection between sentences.

*P1 L20 : The "thermal inertia" is in particular highly related to the ice content, which can be relatively high in a rock glacier. This should be mentioned. Ground instead of soil.*

This is now acknowledged (see above), thank you for the advice.

*P1 L21 : I do not understand the meaning of this part of the sentence in the context of the present study. Why currently ? Is really the reference adequate ? Would not be for instance Scherler et al. 2013 (https://doi.org/10.1002/jgrf.20069, see in particular fig. 5, where the modelled impact of climate warming on two sites with very contrasted ice content is illustrated) more appropriate ?*

The sentence has been removed as not meaningful. Thank you for the reference.

*P2 L1 : What kind of "other processes" ?*

Now omitted.

*P2 L3 : Rock glacier destabilization can be caused by other factors than only climate induced permafrost warming (e.g. cited Roer et al. 2008, Delaloye et al. 2013). The sentence must be adapted in consequence. This is a very important point, because the susceptibility model appears to be based on the assumption of a climate impact only.*

Yes, that's a very crucial point and thank you for the comment. The manuscript has been adapted to this comment by acknowledging that destabilization may be triggered by different factors i.e. mechanical and climatic). Nevertheless, the occurrence of destabilization is finally discriminated by the landform predisposition to destabilization, i.e. the "geometrical factors" (Delaloye et al, 2013). This is a very important point which allows to make clear through the paper that:

1. The modelling part aims to investigate the predisposition to rock glacier destabilization only
2. Testing the significance of the PTP in the predisposition means testing the hypothesis that rock glaciers located at the lower margins of the permafrost zone are more susceptible to destabilization.

Modifications are made through the text to adapt the study to these two concepts. Specifically, your point is acknowledged in the introduction:

*"An overload on the glacier surface caused by a landslide or glacio-isostatic uplift can cause a compressive wave that propagates through the landform increasing its displacement rates and consequent destabilization (Delaloye et al., 2013; Roer et al., 2008)."*

*P2 L4 : Delaloye et al. 2013 (and not 2008). To be changed also further in the paper.*

Thank you for noticing it.

*P2 L4-5 : These events are far from all "representing a serious threat for alpine communities". Sentence to be adapted.*

Sentence (and general paper "tone") has been adapted. In introduction:

*"[..] destabilization and increased displacement rates may precondition significant mass movements that in particular topographic setting may represent an hazard (Kummert and Delaloye , 2018)"*

*P2 L5 (and many times in the paper): Permafrost destabilization. What is this ? The authors are rather talking about the destabilization of frozen ground inducing almost significant mass movements (>100'000m3 ?). Permafrost destabilization appears to be an inadequate terminology that must be replaced by rock glacier or debris slope destabilization and adapted all the paper along.*

Good point. Permafrost destabilization has been replaced by "rock glacier destabilization" or "creeping permafrost destabilization". Also, through the manuscript is made clear that we specifically investigate rock glacier destabilization only. In Introduction:

*"The purpose of this study was to obtain regional-scale insights into the issue of destabilizing rock glaciers in the French Alps."*

*P2 L7, L9-13: Permafrost degradation. Again, what are we talking about ? About a complete ice melt = permafrost has disappeared, the temperature is now above freezing point ? Or about an increased liquid water content (partial ice melt) by warmer permafrost temperature (without any permafrost thaw) ? Is a permafrost warming from -1 to -0.5_C consecutively increasing the liquid water content a permafrost degradation ? In my point of view yes, making that almost all permafrost in the Alps (and in many places elsewhere) is currently degrading ! L13 : It looks that permafrost degradation is here*

*considered as where permafrost is still occurring where it should not be (that is, despite current ground surface thermal conditions that could no more permit its occurrence) ?*

Good point. It is now acknowledged in the paper that permafrost degradation occurs everywhere in the Alps (see introduction above). It was incorrect to blindly put on the same level degrading permafrost and permafrost still occurring despite unsuitable thermal conditions. This distinction is now made clearer, emphasizing that the PTP is only a proxy of permafrost degradation under the assumption that permafrost at lower elevations is temperate, richer in water and more sensitive to climate variations. Section 2.3.2

*"PTP is used under the hypothesis that degradation is more intense at the lower margins of the permafrost zone as permafrost may be temperate, richer in water and more sensitive to climate variations"*

*P2 L14 : What are stable and unstable rock glaciers ? In addition, I cannot agree with the sentence, which seems to be based on the assumptions that all rock glacier destabilizations are induced by climate warming (permafrost warming) and that all rock glaciers with "degrading" permafrost conditions have to destabilize.*

Yes, misleading and inaccurate sentence, now it has been completely rephrased. Difference between stable and unstable rock glacier is defined 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)."*

It is now clarified that increasing temperatures and (possibly) degrading permafrost may trigger destabilisation if the rock glacier geometry allows it (i.e. rock glaciers on flat topography will go towards inactivation if degrading). It is also explained that there is a large number of observation that recognized external factors as destabilization triggers P2 L17 -19. Introduction:

*"Nevertheless, not all rock glaciers experiencing permafrost degradation or mechanical overload are, or will be, destabilized. Permafrost degradation generally causes permafrost thaw in the landform and consequent inactivation (Scapozza et al., 2010). Destabilization was observed only in rock glaciers presenting a topographical predisposition to mass movements, as steep slopes and flow across a convex section (Roer et al., 2008; Delaloye et al., 2013). This suggests that there is a terrain predisposition of the rock glaciers to the onset of a destabilization phase."*

*P2 L20: "Observing rock glacier dynamics and morphology can be rather useful" for what ?*

Deleted. Not relevant in the context as the study.

*P2 L21: Permafrost degradation (complete ice melt ?) in ice-rich landforms does not directly cause the mobilization of significant amount of materials. It makes the material easily erodible, but does not put it in motion. It does not trigger debris flow, but only precondition it. Moreover, this is the (increased) motion of the rock glacier, which is making (more) materials available for later debris flow events (e.g. Kummert)*

It is now acknowledged that rock glacier destabilization may precondition mass movement if the landform is located in particular topographical settings. Introduction:

*"[..] destabilization and increased displacement rates may precondition significant mass movements that in particular topographic setting may represent an hazard (Kummert and Delaloye , 2018)"*

*P2 L23 : An increase of the liquid water content is assumed to cause the so-called destabilization (and not can).*

Sentence modified. Introduction:

*"Warmer climate and linked permafrost degradation on the other hand, its assumed to cause an increase of water content in the permafrost body and the onset of water saturated shear layers where sliding may occur, possibly triggering the crisis"*

*P2 L24-29: About the occurrence of destabilization of active rock glaciers, see also Lambiel et al. 2008. Proceedings of the Ninth International Conference on Permafrost, Fairbanks, Alaska, 1 pp. 1019-1025, in particular Table 2 and related text.*

We are sorry but we couldn't find this reference in the proceedings document.

*P2 L26 : : : : exceptionally (instead of eventually ?) lead to the collapse of the rock glacier (or a significant part of it)*

Yes, it is now specified that rock glacier collapse is an exceptional event.

*P2 L29: Lambiel and Reynard (2001) has nothing to do with destabilization*

True, wrong reference.

*P3 L13: Is DEFROST the most appropriate name for the model, because it helps to evaluate the destabilization susceptibility of active rock glaciers only, and not permafrost (or all permafrost slopes)?*

True. Although we were very proud of this beautiful acronym, we decided to not use it here as not pertinent. It is now referred to rock glacier destabilization susceptibility.

*P3 L19 : Sorry but 15'000 km2 fits with the total area of the French Alps (50-75 x 250 km) and consequently not with the area above 1500 m. And why to mention this latter area?*

True, we must have made a mistake. As you suggest, useless sentence, now omitted.

*P3 L22: Climate is changing fast. Indicate the reference time period for the elevation values of the annual 0_ isotherm.*

Added reference period (and corrected actually wrong values)

*P3 L23: What is the Great Alpine Region ?*

The European Alps as defined in some climatology papers but simply "European Alps" will be better.

*P3 L25: Permafrost is suspected to warm at a rate of 0.04_C per decade at which depth ? Since when ? Does is not depend also on the ground ice content and the temperature of the permafrost (the closer is the temperature to the melting point, the larger is the latent heat consumption and the smaller is the warming rate) ?*

Details about location and time period added. Section 2.1:

*"The only deep permafrost borehole in the region, located in the Ecrins massif in temperate permafrost (-1.3°C) with low ice content, showed a temperature increase rate of 0.04°C perdecade between 2010 and 2014 (Schoeneich et al., 2012), similarly to many sites in Switzerland where data series are longer(PERMOS , 2016) ."*

*P3 L26: Increased rock glacier velocities since the 1990s : provide a reference (Laurichard?)*

Yes, reference added.

*P3 L26: The increase of rock glacier velocity and some destabilization phenomena (and not their destabilization): : :*

Agree, corrected

*P3 L27: Was really the Berard a rock glacier and not "simply" a landslide (of frozen shale and coarser debris)?*

In the Berard site it was observed creeping and massive ice of (probably) periglacial genesis, features that brought Bodin et al (2016) to define it as rock glacier. We would like to stick to the definition proposed by previous authors. Nevertheless, we may not fully understand your concerns about the definition of the Berard site and its implications with this study. Assuming that you are concerned by the exceptionality of the event, we changed the sentence in section 2.1:

*"In 2006 the Berard rock glacier collapsed causing a landslide of 250 000 m3, a very exceptional event that was possibly linked to the rare characteristics of this site, e.g. uncommonly fine grained debris (Bodin et al., 2016)"*

Also, in Figure 8, the Berard example has been replaced with a "more conventional" example of destabilization (Iseran destabilized rock glacier).

*P3 L28: Did not start the destabilization of Pierre Brune rock glacier much earlier than 1990 (see Figure 2), what is not in accordance with the sentence L26.*

Pierre Brune was showing a crevasse since the 70s. Nevertheless, surface velocity were very low until the 90s. The crisis occurred mainly at the end of the 90s (velocities up to 5 m/s) and currently ongoing.

*P3 L29: It cannot be spoken about the detachment of the active layer of the: : : Lou rock glacier, causing a debris flow. So far I know, there was a thunderstorm, which caused the debris flow mobilizing the active layer of the: : : Lou rock glacier. The permafrost table probably limited the torrential regressive erosion and consecutively the total volume of mobilized sediments.*

Yes, the Lou frontal slides were recognized to be concentrated flow phenomena (Kummert et al, 2017) only after the submission of this study. We agree that we cannot talk about permafrost degradation/rock glacier destabilization as a trigger for that event.

*P4 L4-6: What is the accuracy (limit of detection) of the multi-temporal orthoimagery ? Was for instance a rock glacier moving 10 cm/y detectable as active ? How many of the 2100 rock glaciers not classified as active: : : could be active, to say moving more than a (few) cm/y ? This may also have an importance for the model.*

This part was removed as it was actually already treated in Marcer et al (2017). To answer you question I quote that study: *"Also, due to the relatively short time span of 8–15 years covered by the aerial imagery, the movement of rock glaciers creeping at small velocities (~0.1–0.2 m/y) may have remained undetected."*

*P4 L13: A debris flow gully is not a rock glacier surface disturbance. It cannot be used as an indicator for rock glacier destabilization: : : but only for rock glacier motion (and the availability of water) in very specific topographical settings. Rock glaciers classified as destabilized on the single basis of the occurrence of a debris flow gully at their front are not and must be disregarded when building up the model.*

Agree. This was a misjudgement due to the (over)interpretation of the Lou event. The debris flow gullies are now disregarded in this study and the destabilization rating and model computation updated consequently.

*P4 L20-21: 2 m x 2 m is quite coarse. What is accuracy (limit of detection) in a decade (2000-04 to 2012-13) ?*

Wrong value, it is actually 1 x 1 m (or finer according to location). Apart this, section removed.

*P5 L3 : : : : to a possible shift: : :*

Corrected

*P5 L15: : : : Grosse Grabe and Gänder: : :*

Corrected

*P7 L12-14: Rock glacier destabilization was observed to occur : : : at the lower limit of the permafrost zone. Is it really so ? Or what do the authors precisely mean ? Lambiel and Reynard 2001 is not here an adequate reference.*

Inadequate sentence. This may not be true as only few studies actually report this information (e.g. Scotti et al, 2016; Bodin et al, 2016, personal knowledge on Pierre Brune and Roc Noir rock glaciers). This is now omitted (as the inadequate reference).

*P7 L24-25: It could be worth to explain in a few words (if possible) how the PFI index is determined. Values between >0 and <1 represent the uncertainty domain of the PFI model ? Is this correct ?*

It is now added that the PFI is based on rock glacier inventory. The PFI varies between 0 (climate favourable to the existence of a relict rock glacier) and 1 (climate favourable to the existence of a rooting zone of an active rock glacier). Nevertheless, we now avoid going into the details of the meaning of the indexes, as it can result too confusing and complex as explanations. PTP description is now more qualitative and only the PTP index is explained.

*P7 L30ff : The new PFI map is a shift of about 300 m of the permafrost lower limit (?), making that all PFI values within this shifting range are now set to 0, whereas in the 300 m above some are reduced to values between >0 and <1 ? Is it right ? Highest PTP values are found close to the upper boundary of the 300 m shifting range or slightly above it, no ?*

No, the highest PTP values are found where PFI was equal 1 during the LIA and equal to zero in the present climatic conditions. In other words, we expect thaw where there was permafrost during the LIA and now it is not supposed to hold in the current climate. This confusion is probably due to the fact that the section was poorly explained, involving too many indexes and complexity. As explained above, section has been described more qualitatively, hopefully making it more clear. 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 eviedence and it represents thepermafrost 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."*

*P9 L12: Pixels of 2x2m or 0.5x0.5m ? How to get 0.3 m/year accuracy in the first 2000-2004 to 2008-2009 time window with 2x2m pixels ?*

Same error as above, it actually is 1 x 1m.

*P9 L13: Undisturbed (instead of stable ?) active rock glaciers: : :*

We prefer keeping the stability scale as we are talking about rock glaciers destabilization and not only of presence/absence of surface disturbances.

*P9 L14-18: The two sentences are somewhat contradictive.*
Section deleted.

*P10 L1 : The negative correlation of PISR with the destabilization probability is somewhat surprising. Is this not due to the fact that rock glaciers are (much) less frequent on southern expositions due to mountains that are not high enough to allow the occurrence of rock glaciers in such an aspect ?*
We are not sure if we can offer a convincing explanation for this phenomenon at this point. In the Discussion (section 4.3) we point the reader to the importance of water in causing the destabilization of rock glaciers (Ikeda et al., 2008).

Concerning the second part of the reviewer's comment it is important to distinguish between (1) the probability of a specific location presenting a destabilized rock glacier, and (2) the probability that a given rock glacier shows signs of destabilization. Clearly, the reviewer refers to the probability of type (1), which partly relates to rock glacier occurrence per se. This paper, however, only addresses probability type (2), which is conditional on the occurrence of a rock glacier, and therefore unrelated to the question where rock glaciers are more frequent. In other words, yes we have more active rock glaciers on northern expositions than southern, but still the probability of having destabilization is proportionally higher in northern expositions than in southern expositions.

*P11 L29: : : : reaching much more than 5-10 m/y in extreme cases of destabilization (at least seasonally) (e.g. Grabengufer – Delaloye et al. 2013, Ádjet – Eriksen et al. 2018 GRL DOI: 10.1029/2018GL077605), Jegi – Ghirlanda et al. 2016 https://media.gfzpotsdam.de/bib/ICOP/ICOP_2016_Book_of_Abstracts.pdf p.36-38, etc.)*
Thank you for the reference, now integrated in the study.

*P11 L32: See also Lambiel et al. 2008 9ICOP Proceedings*
Sorry but we could not find the reference you proposed.

*P11 L33: : : : because of the high rate of sediment supply in a subjacent gully (if occurring) that may be prone to debris flow events (e.g. Kummert et al. 2017 PPP)*
True (although section removed)

*P12 L6 (and previous): What is a hazardous rock glacier? This is mostly a question of connectivity toward very steep slopes or torrential gullies and transfer rate of sediments (e.g. Kummert et al. 2017 PPP), but for sure not a question of destabilization. Most of the destabilized rock glaciers are far from being hazardous (for human beings and infrastructures) ! But active "stable" rock glaciers may be.*
Yes, it is now made clearer through the manuscript that hazard is discriminated by connectivity (see comment above)

*P12 L11-13: According to my comment on P7 L30ff, it would be very interesting to explore more deeply the relationships between PTP and active rock glaciers. PFI being basically based on the front position of active rock glaciers, one can assume that migrating PFI 300 m upward would makes that the highest PTP values to be found much higher on rock glaciers: : : that is more likely were cracks and crevasses are located. I am wondering to what extent is this DEFROST-PTP correlation physically significant or just fortunately caused by the common morphology of rock glaciers in the French Alps ?*
This question is very similar to P10 L1 and we address it in the same way. As you say, it is true that, due to the method used to produce the PTP map, many active rock glaciers present a high PTP index. Nevertheless, the proportion of destabilization VS stability is still higher for higher PTP than for lower PTP. Significance of the PTP as predictor in the model indicates that there is indeed a significant correlation between destabilization and this predictor.

*P12 L15ff: The comparison to the active layer detachment in the Canadian Arctic appears not to be so adequate because we are comparing two completely different phenomena/processes : shallow*

*infiltration of water in unfrozen ground versus a deep creeping process. Moreover, the snow melt period is occurring later on northern slopes, but it starts also later. Is it so much longer ?*
The comparison is now avoided. This is a good question and we cannot provide an answer. We are suggesting that between north exposed slopes and south exposed slopes there is a strong variability in snow cover duration. Considering the impact of snow cover on permafrost, we suggest to investigate that phenomena to explain the fact that most of the destabilisation occurs at low solar radiation.

*P13 L2 : What is this special thermal regime of rock glaciers ?*
*P13 L3 : Why is active layer thickening causing rock glacier destabilization ? I do not clearly understand what is meant.*
*P13 L6: Debris flows need debris and water. How to use their occurrence for validating the DEFROST susceptibility is so far obscure to me.*
Inappropriate section, deleted in agreement to a focus to rock glacier destabilization only.

*P13 L8-12: And if we look toward the future (to say again +1.5_C), what will remain "sustainable" ?*
We cannot provide a correct answer to this question in the context of rock glacier destabilization.

Concerning, the PTP which can be extrapolated using future climatic scenarios, an increase of + 1.5 with respect to present levels (i.e. + 3 since the preindustrial), will result in a shift of ~500m upslope of the lower limits of the permafrost zone.

*Figure 5 : Only about 25 rock glaciers are moving faster that 2 m/y in the most recent period (5% of the active ones), and not all are considered as potentially destabilized. Is this finally much or not ? About half of the potentially destabilized rock glacier (cat. 3) are moving less than 2 m/y ? I am wondering here if the criteria to define a destabilization phenomenon are all pertinent (see also my comment on Tables 1 and 3). How many rock glaciers are considered in this figure (it looks that there is only a reduced number of cat. 0 and 1) ? This could be indicated.*
Considering that this figure (and relative section) has been removed, please find our answer to the issue of criteria of destabilization at "Table 3" comment.

*Figure 6 : Destabilization rating dots are almost not visible on the map.*
New version of the figure proposed

*Figure 7: PISR : I am wondering if there is not also an effect of illumination, that may make much easier to detect crevasses and cracks on a north slope (better contrast) than on an over-illuminated southern slope (less contrast) ?*
We checked if there was a systematic issue with illumination and we did not find any. It is true that strong illumination makes surfaces featureless. However, this issue exists in all aspects and does not concerns all the orthoimages.

*Table 1 : As already said, I do not consider a debris flow gully as a sign of rock glacier destabilization. The "rugged topography" proposed by Roer et al. (2008) was related to crevasses and scarps and is not synonym of the "crack cluster" described here.*
Yes it has been now removed. Thank you for the "rugged topography" clarification.

*Table 3 : I am very impressed by the high number of rock glaciers displaying cracks and crack clusters. Is it due to a specific lithology ? Is it finally really a sign of destabilization ? Are all rapidly moving rock glaciers (> 2 m/y) exposing scarps and/or crevasses ? Or not ? It may be helpful to organize the table by importance of the specific disturbances as destabilization signs : crevasse(s), scarp(s), cracks cluster, crack(s). Omit gully.*
Very good point, the manuscript has been majorly changed following this. Cracks and crack clusters are surface disturbances that could be observed on the field in two cases of known destabilisation, therefore we consider them as a destabilisation evidence. Still, we agree there is a lot of uncertainty

about this features and their significance as they are very common. We acknowledge this in the text now and potentially destabilised rock glaciers (cat 3) were separated into two different categories according to the surface disturbances they were showing. Rock glaciers showing scarps and crevasses as major evidence of destabilisation were classified into the cat 3a, while in 3b were classified rock glaciers showing crack clusters. Section 2.2.1

*"Potentially destabilized rock glaciers were ultimately classified into two different categories according to the type of surface disturbances observed. Most of the destabilization cases observed by previous studies described rock glaciers characterized by surface disturbances that may reach several meters of depth, i.e. crevasses and scarps, and therefore suggested to split the permafrost body. These surface disturbances can be observed in coarse grained (i.e. blocky, sensu Ikeda and Matsuoka (2006)) rock glaciers. Nevertheless, in the French Alps many active rock glaciers are fine grained and some destabilization cases, e.g the Lou (Schoeneich et al., 2017) and Iseran (Serrano , 2017) rock glaciers, were observed to be characterized by the presence of cracks only. These surface disturbances are shallower than crevasses and scarps and therefore suggested to affect only the upper layer of the rock glacier. As these observations were relatively recent, at present there is still not enough knowledge concerning the significance of these shallow cracks in the context of rock glaciers destabilization. We therefore decided to separate rock glaciers showing shallow surface disturbances from rock glaciers showing deep surfaces disturbances into two5distinct classes in order to make the reader aware of this gap in knowledge"*

To answer your questions regarding rock glacier velocity (although not in the text anymore): not all rapidly moving rock glacier show crack or crevasses and not all cat 3 rock glaciers showing crack or crevasses are rapidly moving. Rock glaciers showing only crack clusters may be rapidly moving (one site was observed on orthoimages to move at more than 6 m/y over 5 years, while the Lou currently moves at more than 3.5 m/y). This is nevertheless a very interesting subject, that will hopefully be developed more into details in a future study.