Response to reviewer comment 1 (RC1):

David Mair[1], Alessandro Lechmann[1], Romain Delunel[1], Serdar Yeşilyurt[1,4], Dmitry Tikhomirov[1,2], Christof Vockenhuber[3], Marcus Christl[3], Naki Akçar[1], and Fritz Schlunegger[1]

[1]Institute of Geological Sciences, University of Bern, Bern, 3012, Switzerland
[2]Department of Geography, University of Zurich, Zurich, 8057, Switzerland
[3]Laboratory of Ion Beam Physics, ETH Zurich, Zurich, 8093, Switzerland
[4]Department of Geography, Ankara University, Ankara, 06100, Turkey

Correspondence to: David Mair (david.mair@geo.unibe.ch)

General response

We thank the anonymous Referee for the constructive, insightful and detailed comments, which we consider very helpful to increase the quality and focus of the manuscript. The reviewer raises 6 major points, which we will address in the same order:

1) *The manuscript is poorly structured. The authors use a range of unclear terms such as headwall, flank, side, footwall, foothill that make it difficult to understand the text. […]*

We restructure the paper following the reviewer's recommendation. The major changes include a better-structured and more detailed introduction of the topic. This also includes a more careful use of the terms the reviewer has outlined. We thus will change, update and improve the terminology following the reviewer's recommendations (see also line by line responses).

[…] The introduction section lacks of clear objectives or aims of the study. […]

We acknowledge that the aims of the study needed a clarification, which we will be done in the revised manuscript (please see lines 62 to 68).

[…] This section is mixed with results. […]

We change this particular paragraph of the introduction, and we make sure not include any results so that introduction, methods, results and discussion are now better separated from each other, which admittedly increases the transparency.

[…] The glacial history of the Eiger is missing in the study site section, however, glacial history is necessary to understand the maximum age of CN samples and the time scales that are integrated in the denudation rates. […]

This is improved. We expand the introduction to include the glacial history (line 74 ff.).

[…] The method section is incomplete and lacks of conducted geotechnical measurements. The reconstructed temperature time series is difficult to understand and used input parameters are insufficiently introduced.

We update the method section accordingly and introduce the input parameters more carefully . We also clarify and justify the selection of the time series of temperature data that we employ in our paper, which will additionally be featured in the new Fig. 2.

Unfortunately, no geotechnical measurements are available for the sample sites due to the poor accessibility of the sites. The Eiger north face is too steep to be accessible for non-professional alpinists; therefore, it was not possible to collect bedrock samples and to conduct geotechnical analyses on them. However, geotechnical parameters would rather quantify the short-term bedrock conditions and thus be only of limited use for our understanding of the long-term average denudation pattern.

2) This paper uses five denudation rates, one derived from a new10Be measurement (EW-1) and four already published in a previous study (Mair et al.,2019). The one measurement, the method and the results are described in very detail, however, the resulting denudation rate is very similar to already published EW-2, which is only located 41 m above EW-1. Rock temperature is adapted by altitudinal temperature lapse rates and the close altitudinal location of EW-1 and EW-2 results in the use of the same frost cracking model. The title and the objectives suggest that frost cracking is the main topic of this paper, however, more than half of the length of this paper focuses on one 10Be sample that at the end produce similar results that the previous study. I recommend to omit this sample and the cosmogenic nuclide technique from the method and result sections and just use your published data from Mair et al. (2019) for your analysis of frost cracking results. This would significantly reduce the manuscript length and the author can address comments 3 and 4 in more detail.

We note here, that the 10Be-based denudation rate estimates is based on one depth profile that includes the ^{10}Be concentrations from 5 samples and not from one alone as inferred above. The decay of the ^{10}Be concentrations with depth thus records a long-term memory of exposure and denudation, which makes this methodology very powerful (please see also Mair et al., 2019). However, we acknowledge that the part of the paper where we describe the application of the cosmogenic nuclide technique is too long for the manuscript. Contrastingly, available space is too limited to fully describe the method, as reviewer 2 points out (see also response to RC2). Therefore, we follow the reviewer's recommendation and shift the methodological description and results of the ^{10}Be analysis from the main manuscript to the new Appendix A. We see this as the best compromise because we think that the measured data should be available to the reader and the public. We seize this opportunity to clarify and expand the methods part where we fully describe the way of how we use the cosmogenic nuclide technology (as suggested by reviewer 2; see also Response to RC2).

3) The authors reconstructed a rock temperature series based on rock temperature logger data by Gruber et al. (2004) and PERMOS data from the years 2001-2014. The authors should produce a figure showing the original data and the generated time series they use as input data. They use only 7 complete years from the data to generate the time series for their sample locations. Which years are used are unknown and it remains unknown how representative the time series is. […]

We acknowledge the need for a more detailed description of the temperature dataset that we use in this paper. We design a new plot (new Fig. 2) where we illustrate the temperature data that we use to estimate the temperature quantities and the resulting input curves for the frost cracking modelling. We further clarify the source and the nature of the data (e.g., years for which data is available) more carefully along with a better justification. All information is there, and we acknowledge that we could have done a better job explaining the material that we use for our paper.

[…] The rock temperature data could be compared to a longer air temperature time series. There will be a thermal offset, however, this offset should be similar for all the years. Furthermore, the authors shift the temperature data to back to LIA and Medieval climatic optimum based on published temperature offsets. This can be suited for EW-3 and EM-samples, however, EW-2 is exposed to atmospheric conditions more than 1.73 ± 0.26 ky (Mair et al., 2019). […]

We are not aware of air temperature data for the studied sites, therefore suitable records would be close-by weather stations. However, we refrain from comparing temperature data with such records since we expect a strong influence of local microclimatic conditions, on rock temperatures (Noetzli et al., 2019). Therefore, we consider such a comparison not helpful for understanding the local temperature regime. Nevertheless, we compare temperature offsets with the climate conditions during the Roman warm period and during the migration period when paleoclimate was cooler. This is based on a different record (Büntgen et al., 2011), which we use to enclose the exposure of EW-02.

[…] Differences in temperatures between logger locations are explained by an insolation model and there is no information how this model is derived in the entire manuscript.

We describe, along with references, the maximum insolation GIS tool that we employ to estimate the maximum annual insolation in section 2.

4) The authors used the frost cracking model by Andersen et al, 2015. They use a rock porosity of 2 % and provide no basis why they use this value. […]

We use a rock porosity value of 2% for the local limestone because it is in excellent agreement with the porosity of 1.8 ± 0.5 that we measured for a previous article (Mair et al., 2019; Supplementary Notes S4) and because it is the default value of Andersen et al. (2015), therefore allowing a better comparison to other studies. We clarify this point in the revised manuscript.

[…] The model requires more than 15 more variables such as flow restrictions, conductivities, heat capacities and so on that are not introduced in the method section. Therefore, it is impossible to understand the model set up. […]

We introduce the variables of the model in the method section. However, we refrain from discussing all of them in detail, as this would be a repetition of the work of Andersen et al. (2015).

[…] There are different limestones at Eiger, which could results in differences of variables such as conductivities of rock. Different conductivities can result in different model results. The authors should test the sensitivity of their model in terms of their chosen input parameters. In addition, they use a fixed frost cracking window (FCW) of -8 to -3_C. Andersen et al. (2015) already demonstrated the consequences of different FCWs in their study and the authors should address this in the discussion. […]

We update our paper accordingly and complement our results with model runs for different conductivities and different FCWs, and we discuss how the results depend on the selection of the input parameters.

[…] FCWs are lithology and strength dependent which is currently reflected by the model by Rempel et al. (2016) and the lab study by Draebing and Krautblatter (2019). […]

We use the most commonly referenced approach due to a lack of empirical data for our setting. Andersen et al. (2015) already evaluated different windows for their model and find similar patterns of FCI intensity in response to different MAT windows. They found that despite differences in the absolute FCI values the relative pattern remains the same. Thus, we expect a similar behaviour for our setting (see also comment above). Nevertheless, we test the effects of different FCW in additional model runs and evaluate potential effects for our setting. We have will thus update our paper accordingly.

[…] In addition, the model assumes water availability in rock when temperatures are above 0°C. […]

We address this point by clearly stating the model assumption in the method section and by discussing potential effects in the discussion section. However, we note that the main reason for the assumption is that water availability is governed by the thermal conditions, where the thermal gradient has to be positive. Temperatures do not necessarily need to exceed 0°C; a reservoir for liquid water, however, is required (see also related in-line responses).

[…] The length of rock the water needs to travel to the freezing front is penalized following Anderson et al. (2013). The authors should discuss the penalization thus water flow can be increased by fractures and therefore increase the FCI. […]

We provide now an expanded discussion on these mechanisms within the method section and discuss its potential effects in the discussion section.

[…] The assumption of water availability decreases frost cracking in permafrost, which is the major argument of the authors for the difference in the denudation rates between North and South and upper and lower locations. However, this assumption is contrary to the findings of Murton et al. (2006) that find higher frost cracking in permafrost due to water release of the active layer during thawing and refreezing of water at the permafrost table. Physical frost cracking models by Walder and Hallet (1985) and Rempel et al. (2016) would show contrary results thus these models integrate mechanical parameters such as ice pressure and rock strength. The authors should be more careful in their discussion and discuss the influence of model assumptions on their results. […]

We recognize that there is a misconception of one of our main arguments here. We infer that permafrost might reduce water availability from below for the scenario where (i) the surface is frozen, (ii) no significant thawing occurs at the permafrost table and (iii) no regolith reservoir for water is present (Andersen et al. 2015). In these scenarios water would need to reach the freezing front from below, which would limit the permafrost conditions (Andersen et al. 2015), while in general permafrost occurrence would promote the occurrence of cracking, as the reviewer points out. We acknowledge that this argument needs a clarification, which we present through expanding the method section to better document, and justify the model's assumptions and through amending the discussion section to reflect these issues (see also corresponding in-line responses).

5-6) The denudation rates reflect different time scales ranging from 0.29 ± 0.05 to 1.73 ± 0.26 ky (Mair et al., 2019). These are quite large differences where climatic conditions and therefore frost cracking will change. The scaling issue is not addressed at all by the authors. […]

We seize the opportunity to address this point more clearly in the method and discussion sections. We do find comparatively little change in climatic conditions throughout the last millennium in near sedimentary records. We add additional temperature data set for the Roman climatic optimum conditions from Büntgen et al. (2011). These data indicate that the temperature changes over time are smaller than between the studied sites.

[…] Other studies observed a paraglacial adjustment of rockwalls and increased denudation rates directly after deglaciation or with a response time up to millennia after deglaciation (Grämiger et al., 2017).

Different glacial history between North and South rockwall could result in differential paraglacial adjustment between North and South rockwall and different denudation rates. […]

Following the works cited in the manuscript, it is reasonable to assume that in the NW the last glaciation occurred during the alpine LGM, while in the SE the rock faces were covered by ice during the Younger Dryas. Our denudation rates, however, are valid for times < 2 ka and thus for a shorter period. Therefore, the time in-between is most likely too long (> 9 kyr) for a paraglacial adjustment to be considered. The numerical models and field evidence predict that the main damages of rock faces occurred during deglaciation (Grämiger et al., 2017). Furthermore, the response time of stress release through sheeting depends to the rock quality (McColl 2012). In highly fractured rock, as is case for the limestone at Mt Eiger, stress release should occur shortly after or during the deglaciation (McColl 2012). Finally, the likeliness of sheeting joints to form also depends on the pre-existing fracture density, where a high fracture density (as is the case at Mt. Eiger) accommodates stresses during glaciation and deglaciation quite fast, which therefore hinders sheeting joints to form (McColl 2012). However, a reconfiguration of paraglacial stress might have been an important factor during the deglaciation and sometime thereafter. Thus, we follow the reviewer's recommendations and add the corresponding argument to the discussion section.

[…] The authors also use the APIM model to analyze the effect of permafrost. The APIM models permafrost on a regional scale of the European Alps and logger data used by the authors in this study demonstrate that the APIM model fails to model permafrost distribution on the South rockwall. Model results from APIM cannot be used on smaller scale and the use is contradictive to the logger data due to scale issues. In addition the APIM suggest a current permafrost distribution (for a period around 2012) due to the used data input (logger data and rock glacier inventories) and provide no insights into past permafrost distribution. […]

We concede that the APIM model resolution is not high enough to reliably predict the occurrence of permafrost. We therefore eliminate the corresponding sentences from the manuscript. Instead, we discuss possible permafrost occurrence on the basis of the temperature data that we use in our paper

[…] In summary, the authors focus too much on frost weathering, discuss a bit thermal stresses and permafrost, however, completely ignore alternative explanation of the observed denudation rates. McColl and Draebing (2019) recently reviewed rock slope adjustment and describe how paraglacial processes, permafrost and weathering processes jointly influence rock slope stability. Therefore, I recommend to discuss the denudation rates more openly and not only focused on frost weathering. […]

We follow the reviewer’s recommendation and restructure the discussion accordingly. We now discuss the potential influence of alternative paraglacial processes on the local denudation rates.

Line by line responses

2: *What are headwalls? Steep rockwalls or rockslopes? You use rockwall, headwall, face, flank and side. I recommend to stick to a clear geomorphic term such as rockwall or rockslope.*

We now use ‘rockwall’ instead of ‘headwall’ and ‘flank’, and we use ‘face’ instead of ‘side’ for clarity.

9: *Maybe use headwalls to clarify it.*

Done.

10: *Rockfalls are preconditioned by fractures which can be also from tectonic origin. Thermo-cryogenic processes prepare and can also trigger rockfalls (cf. McColl, 2012 and McColl and Draebing, 2017).*

We rephrase the text; the point is now clarified in the introduction.

11: *What controls and conditions do you mean? Controls by fractures and influence by thermo-cryogenic processes. Please clarify.*

Clarified.

12: *What you mean with debated? What are the positions of this debate?*

Poorly phrased, now improved to better explain the original idea.

12: *What do you mean with new? You present results from your measurements and compare them to published denudation rates?*

“new and published” removed to better comply with the new structure of the manuscript.

13: *reconstructed temperature conditions*

Suggestion is followed.

15: Suggestion is followed.

16: *I never heard the term footwall before. Better use "foot of the rockslope or rockwall".*

Corrected

19: *Better use the term rockslope, rockwall or rock face. Otherwise, what is the difference between a flank and a face?*

Recommendation followed to avoid unclear terminology.

19: *"and resulting"*

Implemented.

34: *Hallet et al. tested Berea Sandstone and Murton et al. Tuffeau Limestone which are both abundant in the Arctic or South UK and possess porosities between 20 and 40% which are not existing*

rockwalls. Better cite Draebing & Krautblatter (2019) who tested recently frost cracking on samples from Alpine rockwalls or Murton et al. (2016) which used Wetterstein Limestone.
We follow the suggestion and cite both publications now.
38: *Have a look at Draebing & Krautblatter (2019). They compared the efficacy of volumetric expansion and ice segregation.*
This work is now referenced and its findings are presented in the introduction.
39: *These studies by Matsuoka refer more to volumetric expansion.*
True, not cited at this point any more.
40: *The efficacy of which processes? Thermal processes are higher near the surface when diurnal temperature variations are occuring (cf. Collins & Stock, 2016 or Draebing et al., 2017) and propagate to greater rock depth when they occur seasonally (cf. Gischig et al. 2011 a, b).*
I would suggest to refer to frost cracking processes only and they are governed by diurnal processes for volumetric expansion (Cf. Matsuoka, 2008) and seasonal for ice segregation (Anderson, 1998) and temperature gradients.
We follow the reviewer's suggestion and adapt the statements to be more precise.
45: *Draebing and Krautblatter (2019) simulated the influence of water in their ice segregation tests and show how water is driven to a frozen crack.*
Suggested work is now referenced at this point.
47: *also Draebing and Krautblatter (2019)*
Now referenced.
52: *also Draebing and Krautblatter (2019)*
Now referenced.
64: *microclimatic conditions.*
Changed.
64: *Try to keep terms for rockwall small. You use rockwall, face, flank and side. I recommend to stick to a clear geomorphic term such as rockwall or rockslope.*
We follow the recommendation (see also response to major points).
65: *This section presents results. In the end of the introduction you should present the aims of your study. Please clarify what you are doing without presenting results.*
You could write: Our study aims 1) to quantify rockwall denudation in different rockwall locations experiencing diffarnt climatic conditions using CN, 2) model frost cracking and 3) compare denudation rates with potential preparing and triggering factors.
Or something similar. Than it is clear what you will present in this manuscript.
We rewrite the paragraph accordingly.
65: *What is a foothill? You mean foot of the rockslope or rockwall? I am not sure that you took samples at the foot of the rockwall. These location I would expect in Interlaken not at 2500 m altitude. I recommend to rename the location and define the term before you use it to clarify it for the reader.*
We follow the recommendation (see corresponding comments above).
67: *Contains results*.
Rewritten (see comment above).
75: *Can you present the glacial history of the Eiger. This is necessary to understand if there is a glacial history at your sampling location. Have they been covered by ice? Since when the locations are ice-free? In addition, a glacial history is necessary to understand potential paraglacial processes (cf. McColl 2012 or McColl and Draebing, 2019).*
We follow the suggestion and briefly discuss the glacial history at this point.
76: *strikethrough*

Changed.

76: *Oversteepened by what? Glacier erosion?*

Changed to steep.

78: *Can you be more quantitative and calculate a slope angle range based on your DEM.*

We expand on this issue and now provide slope distributions in Fig. 1b. The results are now incorporated in the text.

79: *Please be more quantitative and provide a slope angle or slope angle range.*

We follow the recommendation (see also previous comment).

80: *what are "active glaciers". Depending on definition glaciers need to have moving ice to be glaciers. Do you mean by small "cirque glaciers"? Be more precise.*

Clarified.

82: *five but four are identical with this study.*

Five were sampled, but only four could be interpreted. This is now clarified.

93: *Nice way to say it. In other words you add one 10Be profile to your already published results. You can shorten this section significantly.*

We shorten this section by moving some information to the new Appendix and by streamlining the remaining text.

101: *How do you know this? If there are small rock ledges a significant snow cover can accumulate (cf. Haberkorn et al. 2015 or Draebing et al. 2017a).*

There is a misconception – 'snow cover significant for TCN analysis' was the intended statement. As we shorten the text, we remove the misleading statement.

106: *Fig. 2 shows that EW-2 and EW-3 are located in rockwalls where the model shows. permafrost in nearly all conditions. However, there is no difference to EM1 and EM2. There is maybe a very slight decrease in permafrost probability but this difference is too low to come to the conclusion that the rockwall is "less likely" affected by permafrost. The aim of the APIM is to model permafrost on a regional scale (European Alps). It provides the probability of permafrost and should not be over-interpreted on mountain scale. The resolution is pretty coarse. If high-resolution models are available such as Noetzli et al., (2007), you can draw conclusion on differences in permafrost distribution but you cannot do this based on a coarse regional permafrost model which only provides very slight differences between your measurement locations.*

You should focus on the PERMOS data and interpret the MAT in your results. Positive MAT at EM-sampling sites indicate non-permafrost conditions, however, the regional model by Boeckli et al. (2012) shows permafrost occurrence in arrange somewhere between nearly and mostly cold conditions. The APIM contradicts the temperature data and I would suggest to omit the model and focus on the PEMOS data thus the model is not accurate enough for your scale.

We concede that the results of the APIM model should not be interpreted in this context. Thus, we follow the recommendation and focus now on the PERMOS data. We seize this opportunity to display the reconstructed MAT data in new Fig. 2 (see also response to major point 3).

109: *Thes are results of this study. You should move the data to the result section, describe the mapping approach in the method section and compare it with the general geological data (all references incl. Mair et al. 2018) in the discussuion section.*

We follow the recommendation.

113: *therein*

Changed.

130: *Be more precise and include a subsection on your mapping approach. Describe also how you analyse your data, which software you use to produce the stereonets.*

We follow the recommendation and provide the requested information in the new section 3.1.

191: *You mean you need a time series of rock temperature data to run your model. You use data from Permos (2019) which is based on loggers installed oirginally by Gruber et al. (2004b). Please rephrase and simplify your text.*

Simplified.

203: *Which years you used? Be more precise. Can you please add the data to this paper in form of a figure. Please highlight the logger locations in Figure 1.*

We display the used temperature data now in new Fig. 2. and provide the complete data series in the supplement.

206: *Why this lapse rate? Is the PERMOS data supporting this lapse rate?*

We provide references for this lapse rate in the revised manuscript. The PERMOS data allow no estimation of a lapse rate due to the differences in the microclimatic conditions between NW and SE rockwall.

207: *Can You provide a figure of your modelled rock surface temperature that you used as input for your frost cracking. Please clarify which years are the basis of these modelled rock surface temperature data. Do you omit extreme warm years such as 2003?*

We provide the data series in the supplement and display the years we have considered and the modelled temperature curve that we have used in new Fig. 2.

214: *You assume that there is no response time between climatic warming of air temperature and rock surface temperature. This is a fair assumption but you should highlight it.*

Highlighted.

217: *This model is not a mechanistic model. It incorporates no information on rock properties such as the model of Walder and Hallet (1985). Better use numerical model.*

Changed.

219: *Please simplify the sentence.*

Simplified.

224: *This is highly questionable and a pure assumption. The model by Walder and Hallet (1985) shows that the frost cracking window depends on lithology. A recent study by Draebing and Krautblatter (2019) show that there can be significant differences. You should highlight that this is an assumption based on current knowledge and other studies exist which show alternative frost cracking windows.*

We thank for the comment and follow the recommendation.

229: *This penalty function is suggested by Anderson et al. (2013) and there is no data supporting it. Please highlight that this is an assumption due to lack of available studies that could provide data.*

We follow the recommendation.

232: *Why 2 %? Do you have rock property data that confirm this range. By using your model you assume that heat transport only occurs by conduction, that no fractures are in your rockwalls which produce anisotropy and are preferred path of water and advective heat transport. Please add these assumptions.*

We now reference porosity calculations from density measurements to support the inferred value of 2%, and we discuss the mentioned assumptions now in the text.

257: *strikethrough*

Changed.

336: *Why is this so? Can there be a paraglacial signal such as sheeting joints (cf. McColl 2012) or can you exclude these?*

Indeed, we now discuss this possibility. We consider sheeting joints in response to deglaciation as unlikely to explain the differences in the denudation pattern because: 1) Suitable rockwall parallel joints are only present in the NW (C2; former Fig. 5), where they show a spacing of m to tens of meters. 2) The last possible glaciation was the LGM deglaciation period in the NW and the Younger Dryas in the SW, which would imply a response time for sheeting joints to form of 9 ka or more. 3) Furthermore, the response time of stress release through sheeting is related to the rock quality (McColl 2012). In highly fractured rock, as is the case for the limestone at Mt. Eiger, stress release should occur shortly after or during the deglaciation (McColl 2012). The general likeliness of sheeting joints to form also depends on the pre-existing fracture density, where a high fracture density (as is the case at Mt. Eiger) better accommodates stresses during glaciation and deglaciation, which in turn hinders sheeting joints to form (McColl 2012). However, reconfiguration of paraglacial stress might have been an important factor during the deglaciation and sometime thereafter.

365: *Same scaling problem mentioned above. You should stick to the PERMOS and Gruber data and omit a regional model that is too coarse to show actual permafrost. In addition, the model by Boeckli et al. (2012) is based on current temperature conditions and give no indications if there was permafrost in the past at the Eiger.*

We follow the recommendation and change the sentence accordingly (see response to related comment above).

369: *That's true, however, Murton et al. (2006) demonstrated an increase frost cracking due to permafrost conditions when the active-layer thaws and refreezes at the permafrost table. You cannot exclude this. […]*

We think that this point is due to a misunderstanding (smiliar to the major point 4). We do not want to contradict the findings of Murton et al. (2006); active layer thawing should increase water availability and the FCI. Permafrost might hinder water availability only during times when no thawing occurs. We clarify the argument to avoid confusion.

[…] The frost cracking model you used a priori assumes that water is only available when rock temperature is positive. There is supercooled water that can exist below th freezing point and these assumptions are maybe wrong.

We address this point by clearly listing all model assumptions in the method part. We change the corresponding sentence accordingly (see also major point 4 above).

376: *How you calculate this? It is not described in the method section nor are the results presented in the result section.*

We use the hemispherical viewshed algorithm to calculate maximum annual solar radiation in ESRIS's ArcGis, which was developed by Fu and Rich (2002). We describe this now in the method section, and present the results briefly in the corresponding section.

377: *You should have a look at Draebing and Krautblatter (2019) who quantified the efficacy of frost cracking processes.*

We include the recent findings of Draebing and Krautblatter (2019) in the section.

380: *See also Rode et al. you cited.*

Now also referenced here.

384: *See Draebing and Krautblatter (2019) that quantified stresses.*

We include the recent findings of Draebing and Krautblatter (2019) in the section.

386: *SeeDraebing and Krautblatter (2019) and Walder and Hallet (1985)*

Text amended and works referenced.

388: *Why? This depends on fracture toughness, crack geometry and other lithological properties such as Walder and Hallet (1985) and more recently Rempel et al. (2016) showed.*

Changed, as this admittedly was too simplistic. We expand the literature discussion and now discuss the effect of different windows in our used model.

396: *You don't have regolith cover. Omit this sentence.*
Omitted.

402: *The reason for lower efficacy are the assumptions of water availability only during positive rock temperatures. Water can also be available at negative MATs. Different models such as Walder and Hallet (1985) or Rempel et al. (2016) will result in different results. You should be more careful with your interpretation.*

We concede this argument needs a better clarification (see also responses to major point 4 and comment in line 369). We do not exclude the possibility of water being present at negative MATs as the model also predicts that the FCI on the NW sites increases for colder MATs (see modified former Fig. 6). The model assumes that a liquid water reservoir is essential for an effective ice segregation, which is supported by experimental findings (Walder and Hallet 1985, Matsuoka 2001; and references therein). The model of Rempel et al. (2016) indeed predicts different results, but employs a set of assumptions as well (e.g. cracking is directly correlated to porosity and to constant water availability at the lower boundary). We now discuss that different models would predict different cracking behavior. However, we note that the alternative models would not be able to explain the difference in denudation pattern.

414: *On what basis you draw this conclusion?*
The conclusion is based on the lack of suitable deposits in any geological maps. We clarify this now and refer to some maps.

415: *Why is that so? You referring to a bergsturz event (>1 M m^3).*
We clarify the sentence accordingly.

416: *You can also have paraglacial stress release joints (sheeting joints) that responding to former glaciation and can have large response times (cf. Grämiger et al., 2017).*
Paraglacial stress has indeed the potential to release joints. We address this point now more prominent in the introduction and discussion (see response to major points 5 and 8). We modify the statement accordingly.

417: *What do you mean? If you shift the MAT to adapt to past climates then the mean will always be smaller than the max? Do you mean mean MAT based on current conditions? Please rephrase.*
Rephrased and clarified.

418: *That is based on the model assumptions Lower temperatures will reduce water availability. However, this is contrary to the findings of Murton et al. (2006) which observed increased frost cracking during active-layer thaw.*
We address this argument now clearly in the introduction, method and discussion section (see response to major point 4 and related in-line comments. We rephrase the sentence accordingly.

419: *You modelled these scenarios? It is missing in the method section.*
We do not model the scenarios; we use the result of the modelling study, which we reference (cf. CH2018).

425: *To which graph you are referring to?*
Changed reference to table 1, which is more appropriate.

References

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