Dear editor,

We have carefully addressed all mentioned comments and have improved significantly the structure and the language of the manuscript according to the comments. All detailed changes are listed hereafter.

Philipp Mamot and co-authors

Response by the author to comments of Referee 2

RC = Referee comment

AC = Author comment

Line 13:

RC: It is not quite clear how you calculate the strain rates. Normally for shear tests the shear strain is calculated using the sample height divided by the shear deformation. As you are interested in the deformation of the ice, it would make sense to use the thickness of the ice infill (approx. 3mm) as height. From Fig. S3 the rate of shear deformation seems to be approx. 0.01mm/s. I cannot see how you get to a strain rate of 10^-3/s.

AC: The strain rate is important to make sure that ice fracturing rather than creep is the dominant deformation mode, as 10^{-3} s^{-1} is the strain rate threshold for ice fracturing. Thus, the minimum test strain rate is 1.58 * 10^{-3} s^{-1}, which combines the minimum accounted deformation speed (0.005 mm/s) and the maximum ice layer thickness of 4 mm; this is how we get to the strain rate of 10^{-3} s^{-1}. The shear rate is hereby calculated as the shear velocity [mm/s] divided by the height of the ice layer [mm]. As indicated in the manuscript (line 206) the ice layer thickness varied from 2-4 mm. The mean strain rate averaged for all sample runs is 4.8 ± 1.4 ∗ 10^{-3} s^{-1}.

Correspondingly, we adjusted the mean strain rate in line 231 in the revised manuscript and added some sentences for explanation:

*"The mean horizontal displacement rate was 0.7 ± 0.1 mm/min, corresponding to a mean strain rate of 4.8 ± 1.4 ∗ 10-3 s-1. The minimum test strain rate was 1.58 * 10-3 s-1, which combines the minimum accounted deformation speed (0.005 mm/s) and the maximum ice layer thickness of 4 mm. The shear rate was hereby calculated as the shear velocity [mm/s] divided by the height of the ice layer [mm]. This strain rate guaranteed the dominant deformation mode to be ice fracturing instead of creep, as 10-3 s-1 is the strain rate threshold for ice fracturing (Arenson and Springman, 2005a; Sanderson, 1988). As the shear and compressive strength of*

pure and dirty ice increase with the strain rate (Arenson et al., 2007; Sanderson, 1988; Schulson and Duval, 2009; Yasufuku et al., 2003), variations in the shear rate were kept as low as possible (with ± 0.1 mm/min) and had no measureable influence on the shear strength at failure (Fig. S1b).”

and in line 627:

“…and (iv) with high strain rates ($4.8 \pm 1.4 * 10^{-3}$ s^{-1}) coinciding with the accelerating final failure stage.”

Line 37:

RC: Rock-ice interlocking would only be expected for rough contact surfaces. The surfaces in your experiments are very smooth, so it is very unlikely that you will be able to see interlocking.

AC: We agree with the fact that our experiments simulate the adhesion of rock-ice interfaces rather than rock-ice interlocking due to the smooth shear surfaces. However, we think it to be important to deliver the full information as the introduction part is somehow a state of the art.

In order to consider the referee´s comment, we added a sentence in chapter 4.1, line 450 (revised manuscript), as follows:

“In this study, the bonding of the rock-ice interface is mostly established by adhesion whereas rock-ice interlocking is less important due to the small surface roughness of the rock samples.”

Line 44:

RC: Normally 'discontinuities' is the more general term.

AC: We agree with the referee that “discontinuity” is the more general term than “joints” or “fractures”. Obviously the wording “joints and fractures are used as general terms” was wrong and led to a misunderstanding. Actually, we aimed to say that the long term “discontinuity” will be substituted by the much shorter words “fractures” and “joints” within this manuscript.

So we modified the sentence as follows (line 43 in the revised manuscript):

“In this manuscript, both, joints and fractures are used to substitute the more general term “rock discontinuities”.

Line 47:

RC: Asked about the relevance of fracture toughness of rock bridges in the respective context

AC: The fracture toughness of rock bridges is one component of four which form the shear resistance along discontinuities in a permafrost-affected rock slope after Krautblatter et al. (2013). We wanted to present all components in terms of completeness and in a next step to show on which components we focus in the presented study. That is why we would like to keep the “fracture toughness of rock bridges” in the list of relevant mechanical parameters.

Consequently, we did not cancel this parameter from the list.

Line 90:

RC: Asked to comment on what is commercial ice.

AC: Commercial ice is artificially produced in the laboratory. Butkovich (1954b) describes it with the following words “…virtually free of air bubbles and the grains were prismatic in shape with dimensions of 5 to 20 mm by 40 to 70 mm.” (Hobbs, 1974, page 329)

We renamed the description from “commercial” to “artificial” (line 95 in the revised manuscript)

Line 119:

RC: Proposed to exchange the term *“on the laboratory rock sample scale”* with “on rock samples in the laboratory”

AC: Changed as proposed (line 116 in the revised manuscript)

Line 119:

RC: Asked what is meant by “scaling properties of fracturing dynamics”

AC: We meant the scaling properties of fracture dynamics in the domains of size, space and time. Correspondingly, we added the missing information to the text (line 117 in the revised manuscript):

“Scaling properties of fracturing dynamics in the domains of size, space and time have usually been observed during mechanical loading (Alava et al., 2006).”

Lines 147-149:

RC: I am fully aware that using synthetic materials such as concrete is not ideal, but I can't quite follow your logic here: You state that the friction of ice on granite is different to glass or metals. However, you assume that the rock type is not important, even though different minerals, such as silicates and calcite have different strength, hardness and chemical bonding, which all may affect the rock-ice friction. – with respect to the sentence “*We assume that the shear strength of rock-ice interfaces is mostly affected by temperature, normal stress and joint surface roughness while the rock type is less important.”*

AC: We shifted the respective paragraph to section 4.1 (line 452) because it makes more sense to discuss the topic there.

We agree that the strength and type of the constitutive minerals could also affect the shear strength of an ice-filled rock joint. As no publication on this topic has been published yet, our assumption relies on observations during our experiments and on publications on specific and relevant characteristics of the different rock types. Hence, we listed a few arguments supporting the opposite opinion:

"So far the failure of ice-filled permafrost rock joints has been studied using concrete as a rock analogue (Davies et al., 2000; Günzel, 2008). For the first time, we use rock to closely reproduce real conditions along rock joints. Synthetic materials possibly deviate from shear strength values representative for rock joints in the field. For instance, ice sliding on granite shows a friction coefficient μ approx. 0.5 higher than ice sliding on glass or metals, all having a similar surface asperity roughness. The higher friction of the granite-ice interfaces is due to a higher effective adhesion (Barnes et al., 1971). We assume that the shear strength of rock-ice interfaces is mostly affected by temperature, normal stress, strain rate and joint surface roughness. However, the applied constant strain rate as well as the standardised preparation of a uniform joint surface roughness prohibit any potential effects on the shear strength. We postulate that the influence of the rock type on the shear strength is less important:

(i) The thermal conductivity of the rock may affect the shear strength by facilitated melting along heat-insulating surfaces causing a decrease in friction (Barnes et al., 1971). The thermal conductivity of rocks varies in a range of 0.3–5.4 W m-1 K-1 (Clauser and Huenges, 1993; Schön, 2015). A metal like brass, with a much higher thermal conductivity of 100 W m-1 K-1, would lead to a warming at the interface 14.5 °C lower than granite (Barnes et al., 1971). Due to the relatively small range of thermal conductivities for different rock types we do not expect a rock type-dependent effect on the shear strength.

(ii) The porosity of the rock and the type of constitutive minerals may play a role for the growth of ice crystals along the rock-ice interface which in turn affects the shear strength. The strain rate and the compressive strength of ice crystals are significantly higher for those oriented parallel to the applied stress than for those oriented randomly (Hobbs, 1974; Paterson, 1994). However, we assume any potential rock type-dependent orientations of ice crystals to be deleted before shearing starts due to the applied uniaxial compression during initial consolidation.

(iii) The strength of the constitutive minerals of the rock surfaces may control the friction of the rock-ice contact. However, we assume the strength of minerals to play a minor role for the shear strength, because at the rock-ice interface the ice will fail before the rock material and ice strength will control the failure process. In our tests we could not observe particles breaking off the rock surfaces. Elastic moduli of most rock minerals (bulk modulus k: 17–176 GPa; shear modulus μ: 9–95 GPa) are 2–20 times higher than of ice (k: 8.9 GPa; μ: 3.5 GPa) (Schulson and Duval, 2009; Schön, 2015). The small applied roughness of the shear surfaces additionally prevented any relevant impact of differing mineral strengths.

(iv) Rock minerals heat up differently and can generate thermal stresses (Gómez-Heras et al., 2006) in the intact rock and along discontinuities causing thermal fatigue and a reduction in the shear resistance (Dräbing et al., 2017). However, we do not expect an impact on the shear strength as repetitive temperature cycles with high magnitude and frequency are required for thermal stress fatigue (Hall and Thorn, 2014). During our tests, temperatures were kept constant.

Thus, the tests using limestone represent rock-ice fracturing along joints of all rock types. To use rock instead of other materials probably has a greater effect on the shear strength than different rock types. Still, a potential shear strength dependence on different rock types has to be proven in additional experiments."

We added the new references "Schön, 2015" and "Gómez-Heras et al., 2006" to the reference list (section 11).

Lines 153-155:

RC: Asked for supporting evidence for our statement: "To use rock instead of other materials probably has a greater effect on the shear strength than different rock types."

AC: We shifted the respective statement to the first part of the paragraph and then listed a few arguments including references to support the statement (see above, response to lines 147-149).

Lines 192-193:

RC: Asked for a reference related to the standardized roughness recommendation by the ISRM

AC: We included two references as proposed (line 181 in the revised manuscript):

"Following the ISRM recommendations for standardised tests (Ulusay, 2015) and Coulson (1979, in Cruden and Hu, 1988), the roughness of the specimen's surfaces was produced using abrasive grinding powder.."

Additionally, we added the references to Cruden and Hu (1988) and Ulusay (2015) to the list in section 11.

Line 195:

RC: This is a very smooth surface; the amplitude and wavelength of natural joints is much larger than 0.185mm. As you state above, the surface roughness does have an effect, such as rock-ice interlocking (which was also mentioned earlier). You may also have to consider localised pressure melting at asperities. – with reference to the used diameter of the abrasive grains of 0.185 mm.

AC: We are in agreement with the referee about the influence of surface roughness on the shear strength and failure behaviour. However, we decided to account for both requirements equally: on the one hand a good sample-reproducibility in terms of uniform test conditions (without spending too much time and costs) and on the other hand a simulation of rock surfaces as close as possible to conditions in the field. In future we consider to perform more tests on varying surface roughness, but this is beyond the scope of this first study.

Therefore, we would like to stick to the roughness amplitude of 0.185 mm (line 181 in the revised manuscript).

Lines 196-197:

RC: I do not agree with this statement. Porosity and chemistry of the rock material are likely to have an effect. – related to the sentence *"The uniform joint surface roughness prohibits any potential effect of differing rock types on the shear strength results."*

AC: We agree with the referee´s comment as joint surface roughness does have an effect on the shear strength but it is independent of the rock type. We also agree with the referee´s opinion that some other characteristics (for example porosity or mineral composition) varying with the rock type may affect the shear strength. To account for this, we listed several characteristics and discussed them (see above, response to lines 147-149).

Consequently, we modified the sentence as follows (line 185 in the revised manuscript):

"..The uniform joint surface roughness prohibits any potential effect on the shear strength results.."

Lines 209-211:

RC: The constant-strain results for 'sandwich' samples and concrete-ice samples do differ significantly. The samples with 25mm infill do not show brittle failure. – with respect to the sentence *"In other shear experiments, the shear strength of sandwich samples with a 1 mm thick ice layer did also not differ significantly from the results of concrete-ice specimens representing a 25 mm thick ice infilling (Günzel, 2008)."*

AC: We checked the publication of Günzel (2008) and have to admit that shear strengths of concrete-ice samples and sandwich samples, tested at constant strain, do differ from each other. However, constant stress experiments show similar shear strength results for both sample types (page 585). These include brittle failure as the dominant deformation type: "Constant-stress experiments with both sample types show that here the predominant type of deformation is breaking the connection between the ice and the concrete." (page 586) We hope that we did not misunderstand this.

Consequently, we would like to stick to the reference to Günzel (2008) and specify the information on the tests as follows (line 197 in the revised manuscript):

"..In other shear tests with constant stress, the shear strength of sandwich samples with a 1 mm thick ice layer did also not differ significantly from the results of concrete-ice specimens representing a 25 mm thick ice infilling (Günzel, 2008)…"

Line 228:

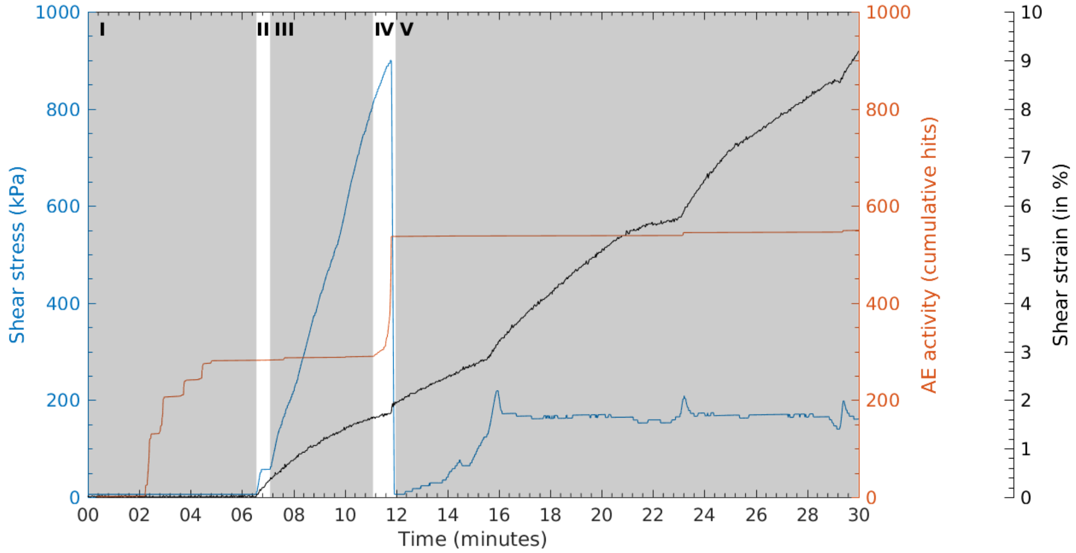
RC: Proposed to exchange the word "campaign" with "series"

AC: Changed as suggested

Line 277/Fig. 4:

RC: Axis labels for both y-axes are missing. As the blue y-axis goes up to 1.0, it is not clear if the shear stress and deformation is normalised in any way?

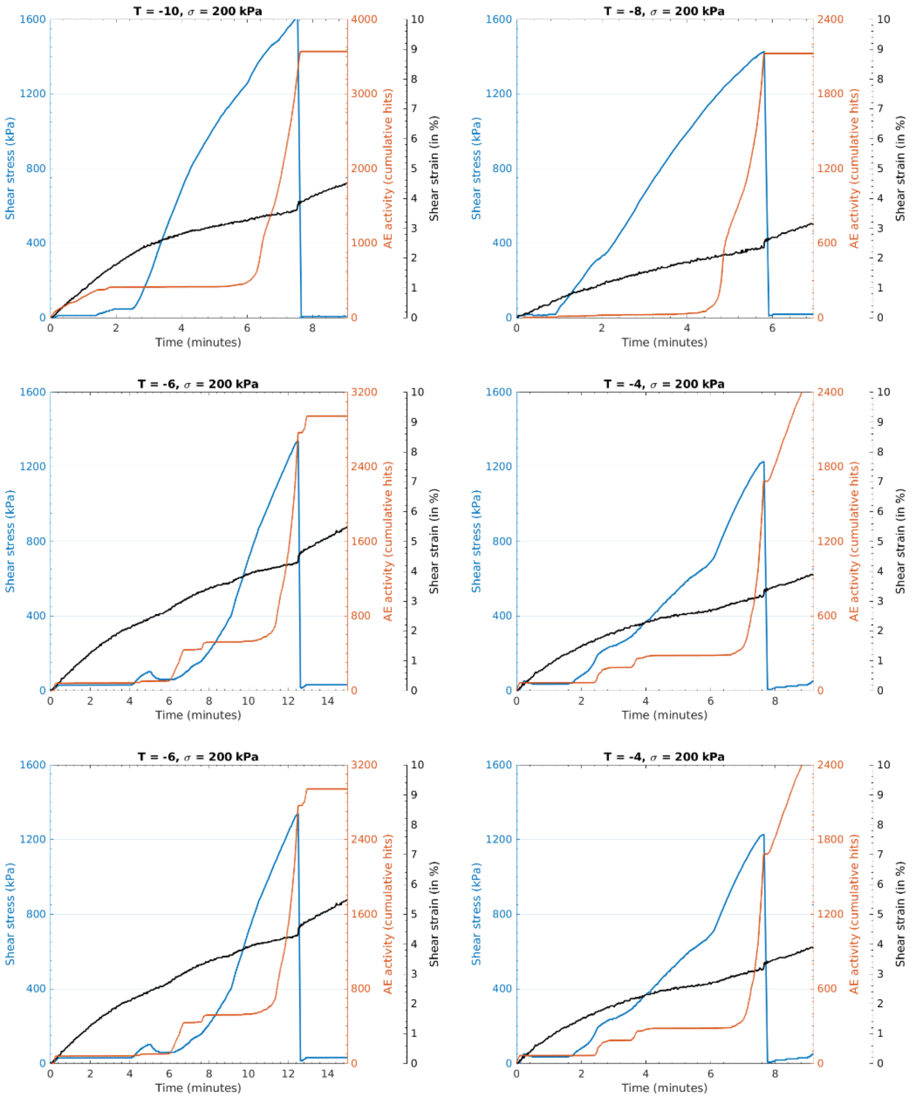
AC: We addressed this comment and modified Fig. 4 (line 269 in the revised manuscript):



Moreover, we modified the first sentence of the caption (line 270 in the revised manuscript):

"Typical curves of shear stress, shear strain and acoustic activity for a shear test at T = -3 °C and σ = 200 kPa."

In the supplementary material we modified Fig. S3 (line 69) in the same way as Fig. 4.:



Line 283:

RC: Proposed to add the word “before” in the phrase *“the experiment starts with a consolidation of approximately five minutes before applying the specified normal load.”*

AC: The specified normal load is applied in the first stage of consolidation. That is why we do not want to add the word “before” as it suggests the normal load to be applied after consolidation (line 276 in the revised manuscript).

Line 292:

RC: Asked if ice healing really gives cracks – related to the phrase *“…the post-failure shear stress exhibits several small peaks, which are often accompanied by a minor increase in AE hits and audible cracking (ice healing).”*

AC: Sorry for this misunderstanding by putting "ice healing" in brackets and not adding any comments on this. When mentioning "ice healing" we primarily referred to the peaks in shear stress which are supposed to represent healed ice that breaks. More than 90 % of the tests with observed post-failure-peaks revealed failures within the ice or a mixture of failures in the ice and along rock-ice contacts. Thus, the peaks of shear stress could also be caused by ice-ice interlocking and subsequent failure when stresses exceed a threshold.

We changed the last sentence of the paragraph and added three ones for explanation (line 283 in the revised manuscript):

"In a few experiments (such as in Fig. 4), the post-failure shear stress exhibits several small peaks, which are often accompanied by a minor increase in AE hits, audible cracking and a pronounced rise in shear strain. The peaks in shear stress may refer to healed ice that breaks when stresses exceed a certain threshold. Further, more than 90 % of the samples with observed post-failure-peaks also failed within the ice or both in the ice and along the rock-ice interface. Hence, these peaks may also be caused by ice-ice interlocking leading to the observed decrease in shear strain (Fig. 4). Subsequent failure occurs when the shear stresses overcome the ice strength."

We also corrected the sentence in lines 280 in terms of the description of the shear strain behaviour at failure:

"Correspondingly, the ice infill between the rock cylinders fails and shear deformation reaches one of its maximum values."

Line 294/Fig. 5a:

RC: Why does recording start at different times? What is time zero? Start of shear? Please give some explanation. – related to Fig. 5a

AC: In Fig. 5a and Fig. 5c, time zero corresponds to the shear start of the experiments. Curves starting at x > 0 represent tests where the first AE signals were recorded a certain time after shear start.

We added the missing explanation to the caption of Fig. 5 (line 291 in the revised manuscript):

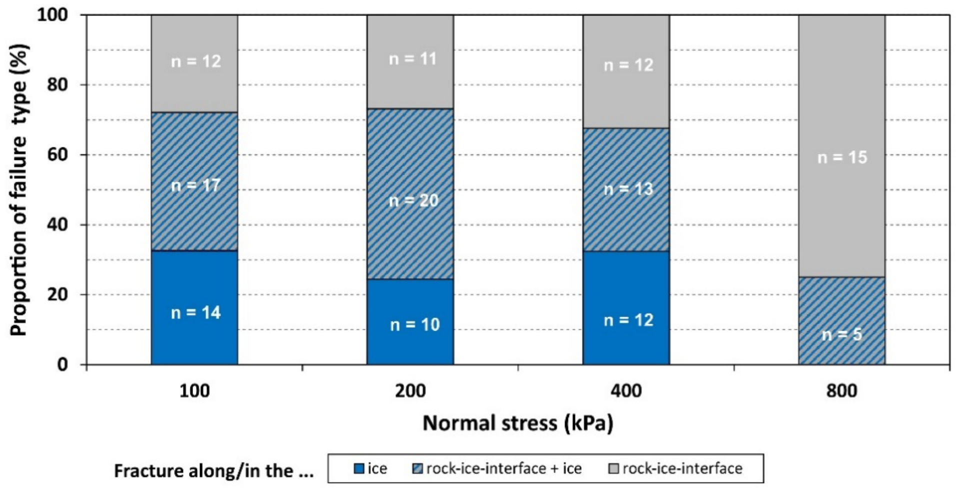
"Figure 5: Time series of (a) cumulative AE hits and (c) normalised cumulative AE hits with 400 kPa normal stress and at -4° C. Time zero corresponds to the shear start of the experiments (spec1-4). Curves starting at x > 0 represent tests where first AE signals were recorded a certain time after shear start. The temperature-dependent time between onset of AE increase and rupture is displayed in seconds (b) and normalised (d)…"

Line 327:

RC: Asked if stress had an effect on the failure type too

AC: To include our observations on the relationship between normal stress and failure type we changed the title of section 3.2 into "*Observed failure type and its dependence on temperature and normal stress*".

Then we added a new figure (Fig. S6) to the supplementary material (line 97):



Within the supplement: We added a caption and a few sentences for explanation (line 98 and following):

"Figure S6: "Proportions and absolute numbers of failure types for normal stresses from 100 to 800 kPa and temperatures from -10 to -0.5 °C."

A new paragraph (lines 100 and following) was added:

"A relationship between failure type and normal stress could not be identified for stress levels 100–400 kPa (Fig. S6). However, at normal stresses of 800 kPa fracturing in the ice does not occur at all whereas fracturing of rock-ice contacts dominates with 75 %. This overrepresentation of failures along the rock-ice interface may be caused due to the absence of tests at 800 kPa and temperatures below -5 °C where (at ≤ 400 kPa) much higher proportions of failures within the ice were observed (Fig. 6)."

Further, the old numbers of the figures in the supplementary material were changed and we included the figure in the "legend" at the first page (line 18):

"(vii) the relationship between failure type and normal stress level (Fig. S6),"

In the main manuscript (line 325): In order to account for the referee`s comment we added the sentence "*A relationship between failure type and normal stress could not be identified (Fig. S6)."*

Further, the old numbers of the figures in the manuscript were changed (lines 328, 360).

Line 345:

RC: I am not quite sure about that. The y-axes have a very different scale to the x-axes. If you plot the data in graphs with equal scale for both axes you will see that the correlation is very weak. – with respect to Fig. 7 and our description of the results: *"The low values of R^2 are partially an effect of the tests clustering around the four predefined stress levels."*

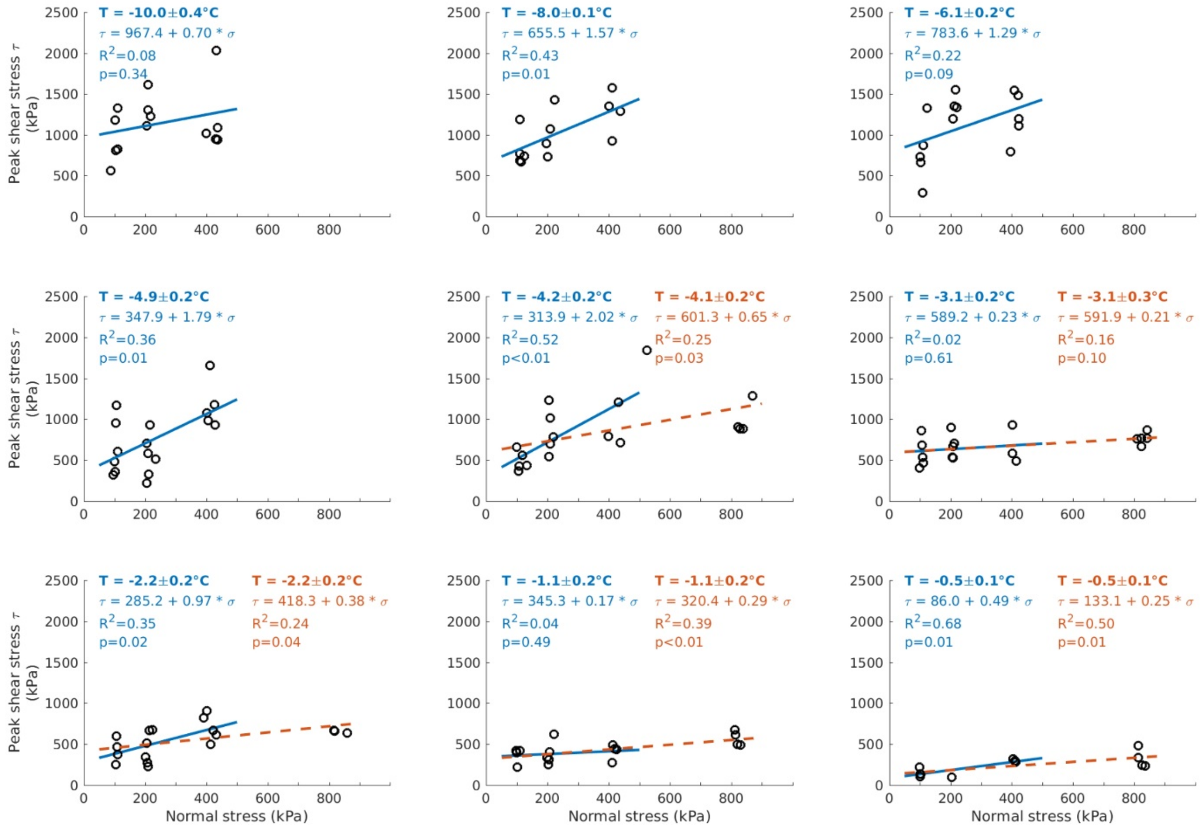
AC: We agree with the referee that the clustering of the data points around the three or four predefined normal stress levels does not have an effect on the values of R^2. When adding more data points at other normal stress levels than 100, 200, 400 or 800 kPa and with similar standard deviations, the values of R^2 do not increase.

Consequently, we deleted the respective sentence *"The low values of R² are partially an effect of the tests clustering around the four predefined stress levels."* (line 340 in the revised manuscript).

Line 349/Fig. 7:

RC: Agreed with Arenson´s comment as the first referee who wrote "Combine a) with b) as illustrated" – see Arenson´s comments

AC: We modified Fig. 7 as suggested (line 341 in the revised manuscript):



As a consequence, we had to adjust the corresponding caption of Figure 7 as follows (line 342 in the revised manuscript):

"Measured shear stress at failure as a function of normal stress for temperatures between -10 and -0.5 °C. The temperature values are corrected by the means (including standard deviation) of the respective temperature level. Normal stress levels of 100, 200 and 400 kPa were applied for the whole temperature range. 800 kPa was additionally tested between -4 and -0.5 °C. Blue solid lines = calculated regression lines for normal stresses 100-400 kPa. Orange dashed lines = calculated regression lines for 100-800 kPa. Regression lines were used to derive temperature-specific cohesion and friction values. p = Probability of rejecting the null hypothesis (H0) although it is true."

Further, we had to change references to the respective figure from "Fig. 7a" or "Fig. 7b" to "Fig. 7", in lines 338 and 580.

Line 351:

RC: I don't quite understand how the temperature data are corrected and indeed why they need to be corrected. – related to the Fig. caption *“The temperature values are corrected by the means (including standard deviation) of the respective temperature level.”*

AC: We did not want to use the values of the temperature classes as they do not represent the real temperatures of the samples close to the moment of failure. Therefore, we used the measured rock temperatures of all experiments belonging to a specific temperature class and calculated the mean and corresponding standard deviation.

We changed the respective sentence in the caption of Fig. 7 for a more detailed and understandable explanation (line 342 in the revised manuscript):

“Figure 7: Measured shear stress at failure as a function of normal stress for temperatures between -10 and -0.5 °C. The temperature values are the means (including standard deviations) of the measured rock sample temperatures (close to the moment of failure) of all experiments belonging to a specific temperature level. Normal stress levels of 100, 200 and 400 kPa were applied for the whole temperature range.”

Line 360:

RC: Recommends to use total normal stress and not effective normal stress

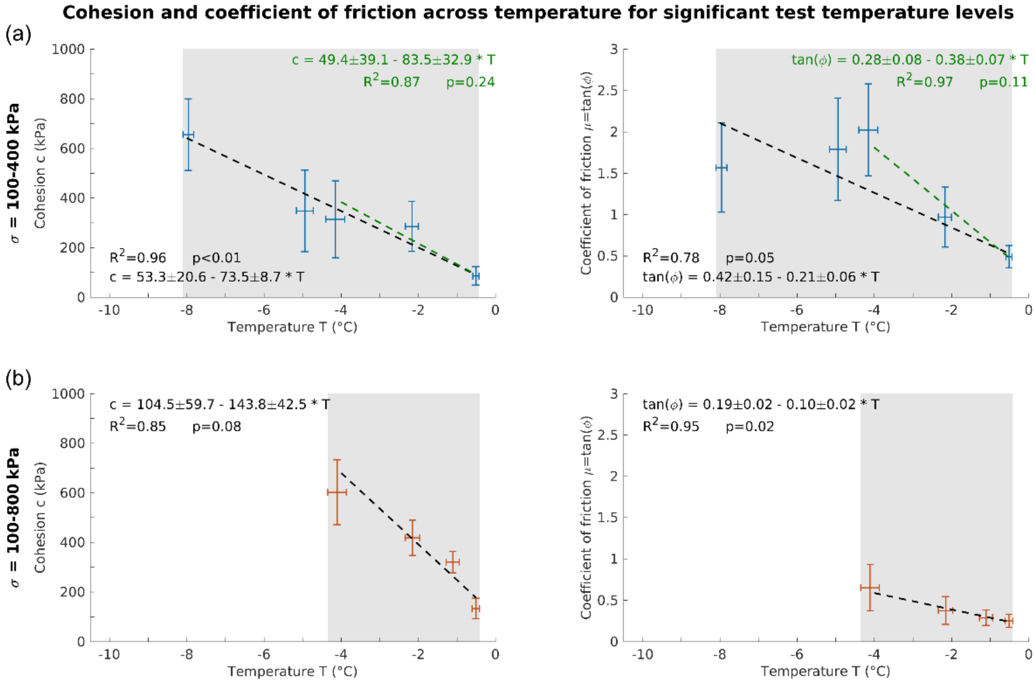
AC: Changed as recommended (line 356 in the revised manuscript).

Line 376/Fig. 8:

RC: I agree with Arenson's comments on the linear relationships; I assume that zero degrees still has ice content (and water)? Then cohesion should be zero. No ice and rock-rock contact at zero degrees would be an entirely different situation. – referred to Fig. 8

AC: Considering the temperatures tested in our experiments, we do not know how cohesion and friction behave between -0.5 and 0 °C. Correspondingly, the curves were not calculated/displayed for temperatures warmer than -0.5 °C. If we assume to have no cohesion at the melting point (where we fully agree with the referee), exponential curves are supposed to be very sensitive in the range of -0.5 to 0 °C. That is why we still keep to the linear curves.

In order to stress the fact that the equations for cohesion and friction are only valid for temperatures between -0.5 and -8 °C, we added grey areas displaying the valid temperature ranges (see below, line 371 in the revised manuscript):



Due to this change in the figure, we had to add further explanation in the caption of Fig. 8 (line 377 in the revised manuscript):

"…The grey areas represent the valid temperature range for the respective parameter."

We agree that no cohesion will be present at 0 °C and when the ice layer has melted. However, as the formula is just valid for temperatures from -8 to -0.5 °C (see sentence below Eq. (6)), cohesion values at a temperature of 0 °C are not supposed to be calculated with the formula. Tests at temperatures warmer than -0.5°C have not been performed as we assume the ice layer to be squeezed out of the rock cylinders leading to shearing along rock-rock contacts. In this case another surface material is involved in the shearing and may lead to different results. This can be observed in Davies et al. (2000, 2001) when comparing ice-concrete samples with concrete-concrete samples.

Hence, we would like to stick to the linear functions and added an explanation to prevent further misunderstandings (line 399 in the revised manuscript)

"We did not perform tests at temperatures warmer than -0.5 °C because we assume the ice to melt or be squeezed out of the rock cylinders which leads to shearing along rock-rock contacts. Cohesion will be absent at the melting point and shear strength values will rise. This is shown by the tests of Davies et al. (2001) where the shear strengths of ice-concrete samples closely approach the concrete-concrete sample line."

Line 431:

RC: Asked if 23 * 10^3 m^3 is meant referring to the expression "23.000 m^3"

AC: We changed the writing style of this rock failure volume and some more (see below) to a uniform writing style used in the whole manuscript:

- Line 69: ."..*97 % of them had volumes* $\leq 3 \times 10^4$ *m³ which correspond to the relevant..*"
- Line 71: "..*94 % of the failures had volumes* $\leq 2.3 \times 10^4$ *m³ which also refer..*"
- Line 428: *"…dominates rock failure volumes of* $\leq 2.3 \times 10^4$ *m³ where all ice..*"
- Line 626: "..*Our tests apply to failures of permafrost rock slopes (i) with volumes of* $\leq 2.3 \times 10^4$ *m³, (ii) with ice..*"

Line 434:

RC: Suggested to exchange "anyhow" by "however"

AC: Changed as suggested

Line 447:

RC: Pointed to her comment in the abstract (line 13).

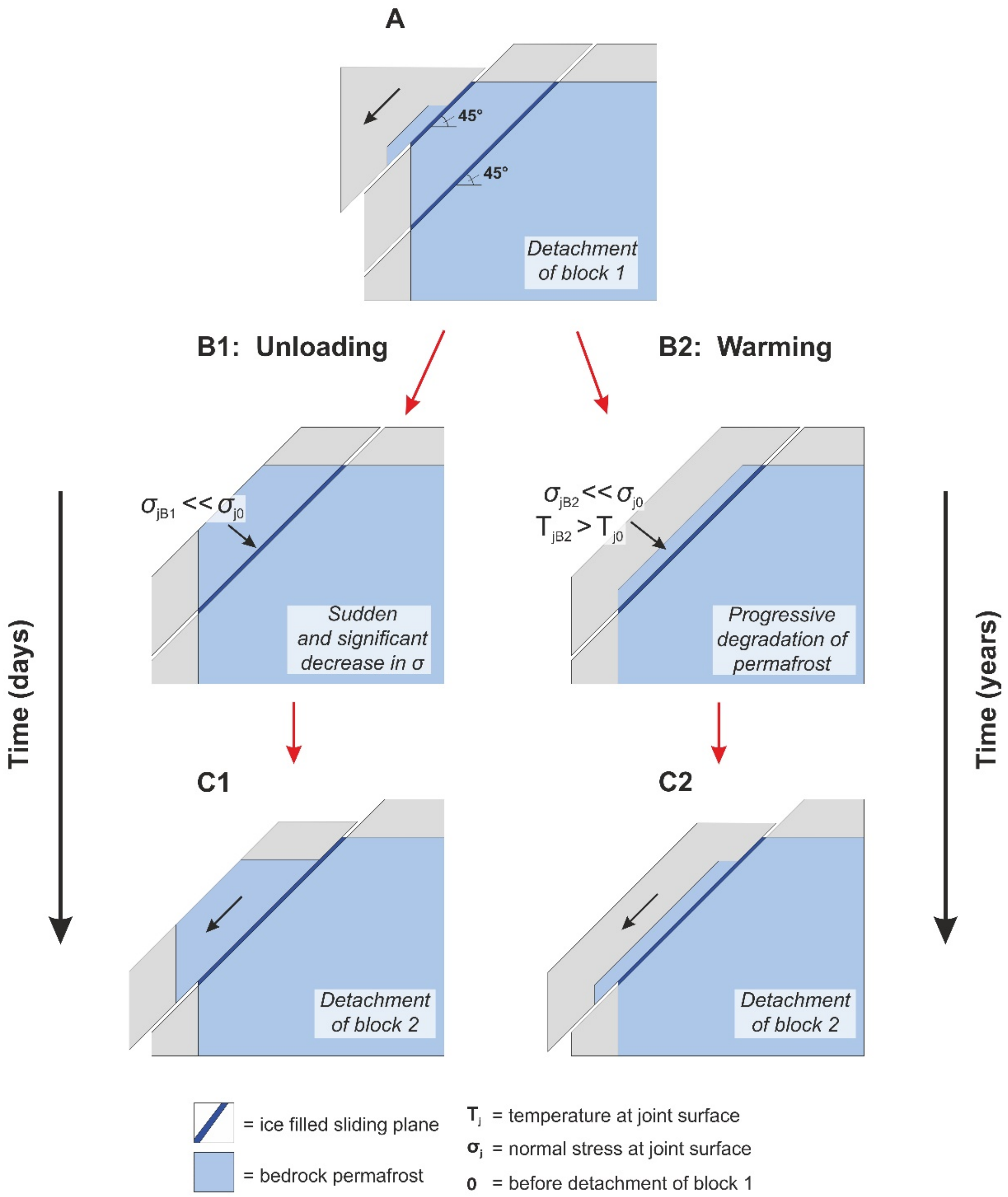
AC: Referring to our response to line 13 (see above) we would like to let the strain rate of 10^{-3} s^{-1} unchanged.

Line 494:

RC: This model may be a little too simplified. As you have cohesion in your model the reduction of mass above the sliding plane will increase slope stability. (For pure frictional sliding the slope stability would be independent of the normal stress.) I recommend that you do some model calculations with commercially available software. – with respect to the concept in Fig. 10.

AC: We would like to avoid any model calculations with commercially available software because this is beyond the scope of this paper. However, we agree with the referee that the presented model is too simple.

Hence, we modified Fig. 10 (line 518 in the revised manuscript) and added more detailed information about what we originally meant (including a new Fig. 11, line 530 in the revised manuscript):



Line 523 in the revised manuscript:

"This study shows that both warming and unloading of ice-filled rock joints, lead to a significant drop in shear resistance, which may cause a self-enforced rock slope failure propagation. The progressive degradation of bedrock permafrost and ice in rock joints may control the cohesion and friction angle of the joints. However, as soon as a first slab has detached from the rock slope (A in Fig. 10), further slabs below can become unstable and finally detach as the shear strength along the ice-filled failure plane is affected by progressive warming (i.e. permafrost degradation, B2 in Fig. 10), but even faster by sudden unloading (B1 in Fig. 10). The latter is represented by a significant drop in normal stress in the Mohr-Coulomb failure criterion (Fig. 11).

Temperature class [°C]	Coefficient of friction [μ]	Cohesion [kPa]	Normal stress-dependent friction at σ_1 =			
			100 kPa	200 kPa	400 kPa	800 kPa
$\lvert\tau\rvert = \sigma * (0.42 - 0.21 * T) + (53.3 - 73.5 * T)$						
			Unloading reduces stability of underlying frozen rock mass		Critical stability loss of underlying frozen rock mass	
-10	2.52	788	252	504	1008	2016
-8	2.10	641	210	420	840	1680
-6	1.68	494	168	336	672	1344
-5	1.47	421	147	294	588	1176
-4	1.26	347	126	252	504	1008
-3	1.05	274	105	210	420	840
			Unloading increases stability of underlying frozen rock mass			
-2	0.84	200	84	168	336	672
-1	0.63	127	63	126	252	504
-0.5	0.53	90	53	105	210	420

Figure 11: Three scenarios of rock slope stability after the sudden unloading of an underlying frozen rock mass with 45 ° fractures. Green boxes: Increasing stability of underlying frozen rock mass. Unloading increases shear resistance relative to shear forces in 45 ° fractures. Orange boxes: Reducing stability. Unloading reduces shear resistance (friction) relative to shear stress in 45 ° fractures. Red boxes: Critical stability loss. Friction loss along underlying 45 ° fractures even exceeds ice cohesion.

We consider three scenarios of rock slope stability depending on the reduction in normal stress and its relationship to cohesion along a specific shear plane with an inclination of 45 °. Correspondingly, all changes in shear forces equal the changes in the shear resistance $\Delta\tau = \Delta\sigma$. However, instability may increase when the loss in friction surpasses the loss of applied shear forces: $\Delta\sigma * \tan\varphi > \Delta\tau$. This may happen in an underlying frozen rock mass upon unloading when μ or $(0.42 - 0.21 * T) > 1$. The cohesion may possibly compensate the decrease in friction when $c > \Delta\sigma * \tan\varphi$. In this case the shear plane could become more unstable without complete failure. This is valid for temperatures between -10 and -3 °C and normal stresses 100–200 kPa (orange boxes in Fig. 11). When the loss of friction in an underlying frozen rock mass upon unloading is much bigger than the reduction in the shear forces $\Delta\sigma * \tan\varphi \gg \Delta\tau$ and even exceeds the ice cohesion $c < \Delta\sigma * \tan\varphi$, then failure along the shear plane is strongly promoted. This is valid for temperatures between -10 and -0.5 °C and normal stresses 400–800 kPa (red boxes in Fig. 11). Rock slope stability in an underlying frozen rock mass upon unloading is only weakly affected if the reduction in friction is smaller than the reduction in the shear force: $\Delta\sigma * \tan\varphi + c < \Delta\tau$. This is valid for temperatures between -2 and -0.5 °C and normal stresses 100–200 kPa (green boxes in Fig. 11). In summary, the red scenario in Fig. 11 indicates a high likelihood of subsequent failures by unloading the underlying frozen rock mass, the orange scenario exposes a moderate likelihood and the green scenario shows a small likelihood. Unloading may lead to instability or even failure when the shear planes (i) are affected by a high reduction in normal stress exceeding the ice cohesion and/or (ii) are below -2 °C due to a friction coefficient higher than 1."

Lines 529-530:

RC: Recommended to change the structure of the sentence from *“Due to the fact that at higher stresses significantly exceeding 400 kPa brittle fracturing can be suppressed in favour of creep behavior, we also conducted experiments at 800 kPa.”* into “We also conducted experiments at 800 kPa because at higher stresses significantly exceeding 400 kPa brittle fracturing can be suppressed in favour of creep behavior.”

AC: We deleted the sentence because it transports redundant information (line 579 in the revised manuscript).