The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-42-AC3, 2019

The Cryosphere
Discussions
Open Access
EGU

Interactive comment on "Fracture dynamics in an unstable, deglaciating headwall, Kitzsteinhorn, Austria" *by* A. Ewald et al.

A. Ewald et al.

andreas.ewald@stud.sbg.ac.at

Received and published: 18 November 2019

We thank Anonymous Referee #3 for his/her detailed, insightful and constructive comments. The expressed criticism is substantial and points to a lack of novelty and originality in the submitted manuscript. We therefore consider a complete revision of our manuscript. The intended new manuscript will no longer focus on recent deglaciation, instead we will explore the relation between fracture kinematics and active layer dynamics in steep, frozen rockwalls. For this purpose we will now concentrate on regression analyses between data from the described crackmeter station and an adjacent permafrost borehole. We consider the immediate vicinity of a deep borehole and a crackmeter station a novel measurement setup that has the potential to advance the current knowledge on kinematics in steep bedrock permafrost. To increase the signifi-

Printer-friendly version

Discussion paper

cance of the new analysis we will expand the time series to four years (2016-2019) as opposed to 2.5 years in the current manuscript. To model thermo-elastic deformation we will resort to the linear regression model published by Weber et al. (2017), and will no longer derive the thermo-elastic deformation component from cracktop temperatures below -10 °C, which has been criticized by all reviewers. To identify potential discrepancies between measured and modeled fracture kinematics we intend to implement state-of-the-art ice segregation models driven by borehole temperature data. Below, we respond to the major concerns/points of criticism of Anonymous Referee #2 only. Facing significant changes of the revised manuscript we assume that most minor comments will no longer apply, therefore we will not comment on here. More general comments of this section will be considered in the revision process.

REVIEWER COMMENT: MAJOR POINTS

1. What is the novelty of this paper? Whereas the paper presents data from a single crack for 2.5 years, Hasler et al. (2012) and Weber et al. (2017) have already presented data on 2D deformation of several cracks, suggesting several types of triggers. Draebing et al. (2017) presented data on horizontal deformation, temperature and water level of three cracks facing different aspects, discussing detailed thermo-hydrological conditions of the cracks. The analysis in this paper mainly follows Draebing et al. without any advance/improvement. Overall, what are the strong points of this paper? Perhaps the borehole temperatures may help discussion of the correlation between thermal condition and crack deformation at depth?

REPLY: Excellent point. We consider a revision of the present manuscript that will focus on the relationship between fracture deformation and borehole temperature data. Furthermore, we intend to use the borehole data to run a frost-cracking model.

REVIEWER COMMENT: 2. The methodology should be more clearly illustrated. The photographs (Fig.1e,f) only display protectors, but do not show the installation of crackmeters. I suspect that the crackmeters were installed like Fig.R1 (see supplement). If it is correct, the two components, CDH and CDV, have to be interrelated. In addition, information on CTT and boreholes are lacking. Is CTT measured at the entrance of the crack or in the casing of crackmeter? Please show the location of the borehole in Fig.1 – on the rockface or on the top station? Detailed illustration of these features is necessary for plausible interpretations.

REPLY: We will provide an improved illustration that accounts for your suggestions.

REVIEWER COMMENT: 3. Data analysis is unreasonable or questionable in the following points. First, Equation 1 assumes that the whole lengths of two blocks (B1 and B2 in Fig.R2) enclosing the observed crack contribute to the horizontal deformation (CDH) of the crack, but in reality, only half of both blocks contribute to it while the other halves contribute to the next cracks (see Fig.R2). Thus, I believe that the contributing length is L rather than 2L. Similarly, CDV is affected by outward (or inward) movements of the both blocks, instead of the authors' approximation that CDV is taken to represent the deformation of one single block (the last sentence of page 4). In this case, when the rockwall is heated, both B1 and B2 blocks may expand outward (see Fig.R2), cancelling the movement of each block; as a result, CDV represents the deformation derived from the difference in the heights of the two blocks. Second, Fig.8 shows temporal changes in the thermal expansion coefficient, but this is unrealistic. The coefficient must be constant for each rock (also cannot be negative), so the assumption in the modelling is likely to be wrong. Third, I suspect that the model applies the parameters derived from on-site monitoring to the results of on-site monitoring. The argument seems to constitute circular reasoning, and the agreement between the monitoring and model should be a natural consequence? Fourth, the modelling assumes 'fracture deformation at CTT below -10C is governed exclusively by thermo-mechanical controls', but deformation at positive CTT (e.g. values during the midsummer) are also largely governed by thermo-mechanical control (except for some events associated with rainwater) so that it can also be used 'to unravel thermo-mechanical from cryogenic fracture deformation'. In fact, however, modelled deformation shows five times wider daily fluctuations

Printer-friendly version

Discussion paper

in summer than measured deformation (Fig.7a), which indicates the invalidity of the assumption. Perhaps a different coefficient should be used for data above 0C?

REPLY: These comments will no longer apply since we will use a different modelling approach. We consider the comment on L for the CDH model. Concerning CDV we acknowledge the comment and agree that this measurement is critical and hard to explain. Also with respect to the available data and large gaps in our measurements, we promise to be more careful when interpreting the CDV results.

REVIEWER COMMENT: 4. Three kinds of thermal windows (TE, FT, IS) are proposed in Fig.8, but I wonder whether the analysis is appropriate or not. This is because FT and IS should be defined by the temperature at a depth where deformation actually occurs, instead of the surface (crack-top) temperature. When heat conduction (i.e. time lag in cooling/warming) is taken into account, the former temperature could be significantly higher than the latter during intensive cooling. Are there any supporting data or reason for substituting the former for the latter?

REPLY: This is a great point. In the intended new manuscript we will refrain from using cracktop temperature as an exclusive indicator of FT and IS. We will instead analyze borehole data (maximum depth 30 m) and will furthermore use the linear regression model introduced by Weber et al. (2017) to model thermo-elastic (reversible) deformation.

REVIEWER COMMENT: 5. The observed vertical expansion during the spring zero-curtain period is very interesting, but needs more careful interpretation, preferably with illustration. The authors attributed the increase in CDV to refreezing of rain-/melt-water. This is a possible explanation for 'horizontal' expansion, but why CDH did not increase. The vertical movement might be explained by frost heaving of the upslope block (B1) at the bottom, followed by settlement of B1 upon thawing (see Fig.R3), but is it realistic? However, this interpretation still cannot explain the absence of change in CDH. Reconsideration is necessary.

REPLY: We will provide another figure that better explains our intentions. During the described period we found an increase in CDV that was not accompanied by a change in CDH. We therefore concluded that the increase in CDV can only be explained by expansion in the uppermost decimeters of the upper block - which are not covered by the horizontal crackmeter (CDH).

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-42, 2019.