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Discussions

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

Anonymous Referee #3

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Ewald et al. present results of 2.5 year on-site monitoring of crack dynamics in an unstable alpine rockwall underlain by permafrost, including a valuable data set that indicates interesting correlations between 2D crack deformation and thermal conditions down to 5 m depth. Since on-site observations at high-risk rockwalls are extremely limited, the data can potentially contribute to geomorphology and engineering geology in cold regions.

However, the paper includes a number of fundamental problems mainly arising from the lack of originality, missing detailed information on monitoring, inadequate modelling, disregard of heat conduction and unclear interpretations. Observed large vertical movements unaccompanied by horizontal movements during zero-curtain periods are interesting, but interpretation seems unsuccessful. Also, the absence of irreversible

crack opening does not prove the contribution of the observed movements to the instability of the monitored block as preparatory processes. The modelling is confusing, since it does not successfully separate thermo-elastic components from other (e.g. cryogenic) components (cf. Weber et al., 2017) and, furthermore, it discusses temporal variability of a constant (thermal expansion coefficient of rock). As a result, plausible conclusions have not yet been reached. In conclusion, the paper is not acceptable in its present form, but it may become acceptable when carefully and thoroughly revised.

The major issues are listed below: the third one is most serious.

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?

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.

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 -10°C 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 in summer than measured deformation (Fig.7a), which indicates the invalidity of the assumption. Perhaps a different coefficient should be used for data above 0°C?

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?

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.

Specific comments (Page/Line) P2/L17: I suspect that Wegmann and Gudmundsen (1999) do not attribute seasonal crack movements to thermo-mechanical process but to seasonal freezing-thawing (volumetric expansion or ice segregation). P3/L5: …immediately upslope of the… P4/L7: Add information on the data logger. P4/L11: Why are resolutions different between two crackmeters? P4/L12: Show the location of thermistor (for CTT) in Fig.1 (or 2). P4/L16: Show the location of the borehole in Fig.1. P4/L30: The definitions of deltaCD and deltaCTT are unclear. What is the 10-day mean crack deformation: the difference between the maximum width and minimum width in 10 days, or anything else? DeltaCTT appears to represent a change in crack-top temperature, but indeed it is defined by the 10-day mean CTT value (not a change but a single temperature). P5/L20: Use consistent units: …depth of 2 m, 3 m and 5 m… P5/L22: Define RT300. P5/L25: 'with no obvious correlation to snow cover thickness': But does the snow cover thickness represent the value at the observed crack? P7/L15: I recommend to draw a graph showing the derivation of alpha values. Fig.1: Do the red face and blue face in (d) represent K1 and K2, respectively? Fig.3: How are the zero-curtain periods determined? – derived from CTT values in Fig.4? Fig.4: Why did data gap occur in spring 2017 – not mentioned in the caption/text.

All of the references cited here are listed in the original text.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-42/tc-2019-42-RC3-supplement.pdf

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