Dear Editor,

I have reviewed the paper *Quantifying irreversible movement in steep fracture bedrock permafrost at Matterhorn (CH)* with great interest. The authors propose a statistical model, with which reversible and irreversible fracture dislocations can be decomposed. It allows inferring possible driving mechanisms from the timing of irreversible movements and may perform well as predictive modelling tool. The paper is well written and in a mature state so that I believe it can be published after some revisions and clarifications. Spell check and language check would be very helpful (I suggested some corrections, but may not have captured all errors not being a native speaker myself).

All my major and minor comments are described in the following:

Page 1 Line 13; Comma after 'Here, ...'

Line 15: 'variable rates'

Line 16: Space after '...year.'

Line 19: remove 'such'. This statement (also occurring elsewhere several times) needs to be reconsidered. What do you mean with 'water'? Water pressure? I think it is far-fetched to say that thawing or the presence of water lowers cohesion and/or friction? There might be alternative mechanisms: increased water pressure would lower the effective stress along fracture but leave the strength (i.e. cohesion and friction) untouched. However, I doubt that significant water pressure can build up in such a heavily fracture and steep, ridge-shaped topography. I would agree that thawing of ice in fractures may have an effect on strength. But how? Reducing cohesion? tensile strength? Friction? All of them? What if ice melts in a fracture that has previously been ice-filled so that the blocks were separated? If the ice melts the blocks would get into contact again and hence friction would actually be higher than with presence of ice. I suggest refining/rewording the statement to describe a mechanism that is better funded.

Line 22: '... deformation cannot be explained by a single process even at close-by locations' (check word order)

Page 2, Line 8: 'Assuming that warming ...'

Line 13: Improved monitoring strategies and hazard assessment for frozen ...'

Line 22: remove 'hereby developed'. Is it known what components change the most to increase 'shear resistance'? Cohesion or friction?

Line 28 and elsewhere: I find the term deformation for discontinuities or fractures confusing or problematic. I associate 'deformation' in rock mechanical contexts with a continuum, so a deforming fracture would be one that changes for instance shape from being planar to being curved. You are referring to movement of one side of the fracture with respect to the other one, while the fracture itself remains undeformed. I suggest using to use the term 'dislocation' for fractures (i.e. infinite deformation along a nominally flat fracture with very small aperture), and leave the term deformation for intact rock.

Line 32: remove widespread or replace by widely. Here is another instance of the term 'fracture deformation'.

Page 3, Line 1: 'unbalance'

Line 23: ‘sketched’ not sketched out.

Line 31: ‘the observed motion’

Page 4, Line 12 – 14: The sentence is somewhat trivial as nobody expects that this equation can readily be applied. I suggest omitting.

Generally Section 2 could be shortened and written in a slightly more concise manner.

Line 22: ‘stresses’ not ‘pressures’. Not sure that is necessarily has to lead to a ‘stress reduction’. I would replace this by ‘deformation/dislocation’.

Line 28: ‘is’ not ‘get’. The sentence is not generally true. In first order, fracturing of cohesive rock bridges only stress intensity. How does the temperature dependence come in? Through presence of ice/water? The mechanism has to be explained in greater detail.

Page 6: Line 10 ‘It depends, among other factors,...’

Line 8: ‘water’ not ‘hydro’ (also in process D4 in Figure 1), hydrostatic pressure (‘hydropressure’ is not a common term).

Line 15: ‘ ...change the resisting forces defined by cohesion and friction ... ’.

Line 16: ‘e.g. from dry to wet’ (Generally, it would be good check the manuscript for colloquial expressions).

Page 7: Line 8. Move sentence ‘Figure 3 gives ... ‘ before the sentence on the measurements locations ‘Fracture deformation perpendicular’ ....

Also: it is not clear how dislocation parallel to a fracture is measured. I assume via extensometers spanned across fracture is an oblique manner. If that is the case, then these sensors would also measure a perpendicular component, and the parallel component has to be computed using the sensor perpendicular to the fracture. A sketch and explanation would help.

Page 8, Section 4.1: How large are the gaps? Do they occur often?

Page 9: Line 4/5: Although a smoothed temperature may resemble temperature time series a greater depth, there are phase shifts of temperature cycles towards depth. I would omit that part of the sentence.

Line 5: ‘are’ not ‘get’.

Line 6: training window: this needs to be explained better what you mean with it.

Here also is my greatest criticism. I’m not sure if the concept of using a training window/learning period is applied in a sensible way. If the goal of the statistical model is to learn something about processes or timing of the dominant process (which I think it is the case here) it would be sufficient to calibrate the model with the entire dataset. If the goal is to demonstrate that the statistical model works as a predictive tool, it should be applied differently: to make predictions one has to use all data recorded up to a certain time, i.e. the model is calibrated against data from the start of the time series to the most recent data, and the training window is growing with time. You could for instance calibrate the model using the first 3 month, 6 month, 1 year, 2 years, etc. to show that it becomes better and better constraint or robust with time. However, choosing a training window in the middle and stating that periods in winter work better is a very arbitrary. I understand that this was done to illustrate the

robustness of the model, but it does not tell anything about its predictive capability nor is it the best calibrated model (which would be one using the entire dataset). I suggest reconsidering this calibration strategy.

Line 14: ‘ the difference between y and y is smoothed with a’

Section 4.4 I’m not sure if these variables do give much insight into the processes. Also the observations in Figure 8 are not very conclusive in terms of correlation between TDD and OFST. The article does not benefit much from it. However, it is up to the author if they leave it in or not.

Page 10, Line 27: ‘There are two options for the end...’, ‘when the rock temperature crosses’

The choice of the end and start of the reversible period sound somewhat arbitrary. It relies on the pre-assumption that the irreversible period only occurs in summer or when temperatures are above -1°. Later this assumption is sold as a result / conclusion drawn from the data. A different strategy would be to let data tell, when to set the start / end of the irreversible period. The irreversibility index offers itself to guide the onset and end of the period.

Replace ‘get’ or ‘got’ by proper passive tense.

Page 11, Line 22-25: ‘the instrumented rock’, ‘the observed fracture deformation’

Line 22: Order of Figures: here Figure 10 follows Figure 5.

Page 12, Line 14: thermo-elastic would be a more appropriate term to talk about a reversible process. (also elsewhere)

Page 13, Line 5: check sentence, there is something wrong here.

Page 14, Line 7: ‘thermo-elastic’ instead of ‘therm-mechanical’

Line 12: ‘distinct’

Page 17, Line 7: ‘melt onset’

Line 5: what is the reason that mh02 does not show any temperature-dependent reversible movement? I think it is remarkable that a fracture does not react on temperature! Do the authors know the reason? I think an explanation would be warranted.

Page 18, Line 6: ‘rates’

Line 15: ‘cannot’ not ‘can not’ also elsewhere.

Line 19: Not only at mh02 and mh21 does OFST and TDD not correlate. How about mh08, or the last point at mh03?. As mentioned earlier I am not too convinced about the value of these observations. Maybe if more explanation/analysis is offered it could be a good contribution to process understanding. However, you do not really elaborate much on the different behaviours.

Line 25: How many days before the rock fall did this increase occur? I think even without the irreversibility index the change in behaviour was readily visible from the fracture opening data. Do you know the volume of the break-off?

Line 32 ‘was not observed to close’

Page 19, Line 20: During additional phases, ...

Line 22: ‘suggesting a decrease of cohesion and friction’ → as mentioned earlier this is a too far-fetched conclusion and is not directly supported by your data. It may also be that during summer, stress redistribute such that strength (i.e. friction and cohesion) is overcome and slip initiates, while friction and cohesion themselves do not change.

If you compare with

Collins, B. D. & Stock, G. M. Nature Geosci. http://dx.doi. org/10.1038/ngeo2686 (2016).

or

Gunzburger, Y., Merrien-Soukatchoff, V. & Guglielmi, Y. Int. J. Rock Mech. Min. Sci. 42, 331–349 (2005).

irreversible fracture opening or slip does not have to be related to a change in strength (or not even to ice, although it may well be the case). To me it is not entirely clear by what mechanism irreversible movements in your case are induced: are the tensile fractures ‘glued’ with ice in winter (in this case it would be a change in tensile strength) or is it ice along sliding planes? Can something be deduced from your data and structural observations (block shapes, fracture orientations?)

Line 24-25: I suggest omitting this sentence. It does not conclude from your observations.

I hope that these comments help for improving the presentation of this very interesting work. If you have questions you would like to discuss in person, you are most welcome to contact me in person by email or phone.

Best regards,

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