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Interactive comment on "Quantifying irreversible movement in steep fractured bedrock permafrost at Matterhorn (CH)" *by* Samuel Weber et al.

Anonymous Referee #2

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GENERAL COMMENTS

The authors propose an empirical/statistical model aimed at separating reversible components of fracture deformation, due to thermo-elastic strains in alpine high-elevation permafrost environments, and the irreversible (plastic) component due to other processes. The topic is interesting and of interest for The Cryosphere. The work is based on a very interesting 7-year time series of fracture displacements recorded at several locations at the Matterhorn (Switzerland) by a monitoring network set up by Hasler et al (2012). Nevertheless, my review pointed out a number of serious scientific issues, which are listed in the following general comments and in the following "Detailed comments" section. I suggest that these points must be carefully addressed before the manuscript can be published in a high-level journal as The Cryosphere.

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Discussion paper

1) The abstract is quite too long and should be more focused and to-the-point;

2) English could be generally improved using shorter and more focused sentences;

3) The “mechanical conceptual model” (Section 2) is characterized by some weaknesses and is not used later in the paper, which then focus on an empirical/statistical model. The authors seem to want to add a "rock mechanics taste" to the work, but tend to mix some different concepts and quantities and use terms as "fracture dynamics" which sound ambiguous to people from the geological and engineering rock mechanics communities (see detailed comments);

4) The empirical/statistical model, making the core of the work, is biased by strong assumptions leading to somehow obvious results and poor predictive capability (see different detailed comments below). Actually, it is difficult for me to see either the scientific advance or the practical contribution of this work. In fact, the statistical model aims at discriminating thermo-mechanical elastic displacements, which are indeed small and of the same order of magnitude of possible precursors of rock slope instability (the latter can also follow very different patterns). This seems to suggest that the reliability of the method is low for small irreversible displacements and useless when irreversible displacements become larger. Finally, irreversible displacements are not investigated themselves thus the method cannot be used to predict rock slope failures (as promised in the abstract)

5) The most interesting contribution seen here is monitoring, providing a continuous, 7-years long time series of displacements. Nevertheless, this contribution originates from the previous work by Hasler et al 2012.

DETAILED COMMENTS

Page 1, line 4: (and elsewhere in the manuscript): "fracture dynamics" is a confusing term to members of the rock mechanics communities (both geoscience and engineering): in fact the term "dynamics" usually refer to fracture mechanics (micro- or mesomodes of failure and related mechanical models and parameters; see e.g. Paterson & Wong) under dynamic loading conditions. Instead, the author simply refer to the temporal pattern of movements along or perpendicular to fractures. Why not use a simple term as "fracture kinematics"?

Page 1, line 9: "gravity-driven slope failure": Rock slope failure? Landslide?

Page 1, lines 12-13: "enables a local assessment of rock wall stability": actually, the presented work just aims at depurate a time series of displacement along fractures from the elastic thermal component. No analysis of the spatial-temporal patterns, mechanisms and triggers of irreversible displacements is proposed, thus I do not understand how rock wall stability is dealt with here.

Page 2, line 4: "frozen rock masses": the authors focus on rock masses with ice-filled discontinuities and exclude ice-free frozen rocks, where a thermal elastic strains indeed occur. This is ok, but I suggest that this should be declared clearly as an assumption at the beginning of the analysis, also suggesting the expected differences in the behaviors of ice-filled and ice-free rock masses with respect to slope instability. This would be very useful to non-permafrost-experts involved in the analysis of slope instability at high altitudes.

Page 2, line 21: "Intact high prosity rocks": and what about low porosity rocks, which form most of the Alps?

Page 3, line 25 "thermo-elastic induced strains": the conceptual model of the authors is based on a balance of driving and resisting forces. Strains are not forces, but are related to forces by a specified rheology and geometry (i.e. Stress distribution). Balancing the contribution of strains is formally incorrect, although this has no consequence on the analysis because the mechanical model is actually not used in the following (but it is another weakness of this work; see General Comments)

Page 3, line 27: "creep and fracture of ice": here the authors include among resisting forces some processes and quantities that are not forces. Creep is a time dependent deformation of materials, including a large variety of physical processes at micro to macro scales. Fracture is brittle failure of solids. I understand that ice deformation and failure reduces stresses through plastic work, but again it is formally not correct to include these processes as forces.

Page 3, line 27: "fracture infill": strength of fracture infill?

Page 3, line 31: "reversible and irreversible": elastic and plastic?

Page 3, line 33 and Page 4, line 1: I am not convinced about the physical consistency of the "temperature-fracture deformation relationships". It is well known from a huge laboratory rock mechanics literature that the rheology (stress-strain relationships, brittle vs ductile behavior) of rocks depends on temperature. Thus, it would not be possible in principle to define unique temperature-strain relationships, especially when dealing with creep, which is non-linear and time dependent even at constant temperature. I understand that authors just refer to individual existing fracture deformations and guess that they assume linear elastic-perfectly plastic rheology in the considered temperature range. Nevertheless, the authors should clearly state and support their assumptions and related limitations: are they sure that stress-strain-temperature relationships for ice filled fractures (and even more for fractured rock masses!) are as simple as they state? Are they able to provide experimental data or literature to support that?

Page 6, line 23: "these statements are validated": in the following, the authors switch from a conceptual mechanical model to a simplified statistical one to discriminate reversible and irreversible movements along monitored fractures. Nevertheless, the postulated origins of irreversible movements, I.e. "Cryogenic" in winter and "hydro" in summer, although reasonable, are not validated by data and analysis. No information is provided about the state of ice filling in fractures, and there is no correlation between hydrological parameters (e.g. Rainfall) and irreversible movements.

Page 6, line 29: "heterogeneous": in which sense?

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Page 6, line 30: rainfall, cold winter temperature, exposure etc.: please provide quantitative values/ranges typical of the studied environments.

Page 7, line 5: could the authors explain why they measured temperature down to 85cm and not deeper? This also applies elsewhere in the manuscript. Which are the other measuring depths, and why temperature profiles are not used / presented?

Page 7, lines 5-6: "high resolution images": what are these used for, also considering that pixel resolution is of the same order of magnitude of the fracture displacements recorded in seven years?

Page 7, line 13: "aggregated": cumulated or averaged?

Page 8, line 4: "Staub et al": manuscripts in review are not citable.

Page 8, line 9: "Used temperature ....at 85cm depth": why?

Page 9, lines 3-10: the statistical linear model for the reversible deformations is poorly explained and supported: does it apply at the same way to shear and normal fracture displacements? How is the data population related to reversible movements separated from the irreversible movements occurring in winter for fitting purposes? Which are the best-fit statistical parameters of the model and related measures of statistical performance? These are not reported and the reader is forced to believe that the model is robust. This is a major scientific weakness of the work and the authors should work more on this.

Page 9, line 14: 28 days window length: one month is a long smoothing period, could the authors explain why they used such a long time interval? In general, one could expect that excessive smoothing may "kill" some important signals on shorter timescales.

Page 10, line 11: “due to creeping”: this part is obscure and, again, I cannot understand how the authors are able to separate the population of reversible vs. irreversible winter deformations.

Page 10, line 21: “in winter. . ...we assume that deformation by the thermos-mechanical induced strain dominated”: this indeed remains a strong assumption, possibly significantly biasing the model. The authors should try to support this better.

Page 11, Figure 4 (and related text): the piecewise linear regression model sounds over-simplified and biased by different strong assumptions including the following: 1) winter deformation is always reversible (or, at least, the same reversible deformation fitted in an early “training period” occur every winter – this may be not true as the rock mass accumulates damage); 2) the beginning of the “creeping” phases can be predefined and is the same every year; 3) displacement time series in the creeping phase are linear. I suggest that these assumptions pose too many constraints on the model and hampers its application to prediction/forecasting, except in very simple cases.

Page 11, line 23: “. . .a field site can not be described by a single measurement location. . ..”: this seems quite obvious, and things are even worse when dealing with rock masses instead of individual fractures.

Page 12, lines 9-10: “indicated by a black line in Figure 6”: this is unclear or incorrect. The black lines seem linear regression functions, not their coefficients (which are never reported in the paper; instead, the authors should provide tables of best-fitting function parameters and regression quality statistics or indices to demonstrate the performance of their statistical model). Moreover, it is difficult to understand why the black lines are plotted at these positions (why don’t they intersect the x-axis in zero? What is actually fited?)

Page 13, lines 4-5: “note that. . ...deformation”: incomplete statement.

Page 13, lines 5-6: “reduced data input”: the authors’ approach is to fit very limited time windows and then extrapolate the results. But in this way, they are not able to obtain a model fitting the entire dataset, which is particularly important to empirically fit time-dependent movements (creep). Also, in this way the potential of the beautiful 7-year presented monitoring series is not exploited.

Page 14, line 1: “this likely indicates thawing related processes”: this is obviously reasonable, but but still unsupported by specific analyses. “assuming that water is available. . ...deformation”: same comment.

Page 17, lines 4-5: “one single. . .. . .fracture deformation”: the result is reasonable in some specific conditions (individual fracture displacements vs. rock mass, low strain, low damage, simple failure kinematics causing block movements), but is biased by the strong assumptions on which the model is based (what is reversible or irreversible deformation?)

Page 18, section 6.2: a qualitative analysis of raw data would have brought the same observations / conclusions, suggesting that the data (following the work of Hasler et al 2012) are very interesting, but the proposed model does not bring significant contributions or advantages (especially in a predictive perspective)

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