Reply to comments made by Anonymous Referee #2 (doi:10.5194/tc-2016-136-RC2).
We thank Anonymous Referee #2 for its review and suggestions for improvement. Referee comments indicated as “RC:”, author reply as “AR:”. Only sections requiring a reply are reproduced.

GENERAL COMMENTS
RC: 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.

RC: GENERAL COMMENT 1. The abstract is quite too long and should be more focused and to-the-point;
AR: We shortened the abstract and focused more on the main results of our analysis. See page 1.

RC: GENERAL COMMENT 2. English could be generally improved using shorter and more focused sentences;
AR: We addressed this comment. The re-submitted manuscript was revised by a native speaker.

RC: GENERAL COMMENT 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);
AR: We agree, the initial conceptual model was not consistent, contained some weaknesses and lacked clear link to the main work undertaken in this study. We replaced the conceptual model by a schematic visualization and a description of kinematics in steep fractured bedrock permafrost and the related main acting mechanisms influenced by varying environmental forcing. This part leads now more clearly to the research questions and includes the assumption for the developed linear regression model. To be more precise, we now use the term “fracture kinematics” and “fracture displacements” instead of “fracture dynamics”.

RC: GENERAL COMMENT 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).
AR: We agree that the original manuscript was not clear enough on the aim and main focus. This point helped to improve the manuscript. The main results stay the same, which indicates that the previous assumptions were not invalid. We agree, the applied model is based on assumption and has limitations, but the main target is to separate reversible thermo-mechanically induced (elastic) displacements from the residual irreversible (plastic) displacements and not to predict. This model is rather a tool for fracture kinematics analysis than for prediction of rock slope failure. The focus and aim of this manuscripts are now clarified and assumptions and limitations are discussed in more detail in the revised manuscript and is investigated by a separate correlation analysis.
The scientific contribution of this manuscript is to distinguish phases as well as the timing in

relation to potentially acting processes. Timing of irreversible kinematics in relation to environmental forcing is crucial for investigating and identifying the acting mechanisms and to assess rock slope stability. The results clearly show, that thermo-mechanically induced strain dominates in winter. Further, the irreversible displacements are investigated in relation of environmental forcing using the available data. This allows some inferences on potential causing mechanisms. But as referee 2 rightly points out, we can not investigate the actual causing process in detail (this point has been clarified in the manuscript).

RC: GENERAL COMMENT 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.
AR: The data in this manuscript is based on the initial experimental and installation setup by Hasler et al. (2012). But the analysis of Hasler et al. (2012) was based on a very short time series (5 locations under 2 years and 5 locations under one year). Due to the limited duration of the data set, Hasler et al. (2012) provided only a qualitative analysis. Here, we present a much extended data set of 7 consecutive years of most sensors. Further, with this data set we undertake a much more detailed and quantitative analysis. All data used in this paper is openly available.

RC: 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 meso-modes 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"?
AR: We appreciate this advice. We replaced "fracture dynamics" by "fracture kinematics" or “fracture displacements” everywhere in the manuscript.

RC: Page 1, line 9: "gravity-driven slope failure": Rock slope failure? Landslide?
AR: “gravity-driven slope failure” has been removed by shortening the abstract.

RC: 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.
AR: We agree with the referee that our investigations are not focused on stability. However, the analysis includes measurements with a high temporal resolution at multiple locations with different characteristics as exposition or slope. This gives an idea of the spatial variability, but no common pattern could be detected. As irreversible kinematics can lead to instabilities, the temporal evolution of the irreversibility provides a first indication for stability assessments. We adjusted the text accordingly.

RC: 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.
AR: In our interpretation, the adjective “frozen” refers to the aggregate state of potentially available water in a rock mass. In permafrost regions, three layers are expected. In the top layer (active layer), ice can occur seasonally if water is available. At the permafrost table (boundary between active layer and permafrost body), the percolating water freezes and stays perennially. The ice content in the permafrost body mainly depends on the water availability during permafrost aggradation. We fully agree that there are differences in the behavior of ice-filled and ice-free rock masses with respect to slope instability. But it is difficult to quantify the occurrence of ice in fractures, as the visible part of the fracture lays in the active layer and is ice-free in summer. Visual observations during field visits in winter support the seasonal availability of ice in some fractures.

RC: Page 2, line 21: "Intact high prosity rocks": and what about low porosity rocks, which form most of the Alps?
AR: We fully agree on this point, also the Matterhorn consists of low porosity rock. Unfortunately, there are limited studies investigating low porosity rocks. The same mechanism is also expected to act in rock masses with flaws in rock. We addressed this point by adding the following sentence to the manuscript: "Based on numerical simulations, ice segregation can even occur in low porosity rocks in an estimated temperature range from −4 to −15° C (Walder and Hallet, 1985)." See page 4, lines 25-26.

RC: 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)
AR: We agree that there was a confusing use of language/terminology in this section. We replaced the conceptual model by a schematic visualization and a description of kinematics in steep fractured bedrock permafrost and the related main acting mechanisms influenced by varying environmental forcing. This new approach built the basis for the linear regression model and the hypothesis. Based on the 7 year time series, we analyzed and discussed the influence of environmental forcing on the acting mechanisms.

RC: 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.
AR: We agree, a detailed answer is given in the previous point and the text has been revised accordingly.

RC: Page 3, line 27: "fracture infill": strength of fracture infill?
AR: Fracture infill is interpreted as a mechanism that blocks the fracture and prevents a closing of the fracture, unless there are other mechanisms which reduce the amount of infill.

RC: Page 3, line 31: "reversible and irreversible": elastic and plastic?
AR: Reversible kinematics refers to thermally-induced strain, while irreversible describes the residual kinematics. Thus, the reversible part is elastic strain, but the irreversible part can also include creep and rupture beside plastic strain. We addressed this comment by modifying the manuscript: "... The observed fracture kinematics usually consists of a reversible (elastic) and irreversible (plastic, creep and rupture) component. ..." See page 3, lines 3-4.

RC: 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?
AR: We think there is a misunderstanding in scale and temperature here. The laboratory experiment of Wolters (1969) showed a linear temperature-strain relation for the temperature range from -20 to +80° C, which covers the temperature range measured at Matterhorn. Several studies in permafrost bedrock with different measurement setups (e.g. Wegmann and Gudmundsson, 1999; Matsuoka, 2001; Matsuoka and Murton, 2008; Nordvik et al., 2010) reported a simple

correlation between fracture kinematics and (rock-) temperature at different time scales from diurnal to annual. The field site Matterhorn consists of fractures with and without ice, but the stress induced by ice pressure might be limited due to the high degree of fracturing. For our model describing the reversible fracture kinematics, we assumed a linear relationship between thermo-elastic strains in rock and temperature (we modified and clarified this point in the manuscript). It is clear that reversible kinematics can not be split up in different processes, but high coefficients of determination resulting from the regression analysis indicate that it works.

RC: 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.
AR: We agree that these statements are not explicitly validated due to limited data describing environmental conditions and no reliable data providing information about the state of ice infill in fractures is available. The paper was refocused and the hypotheses were removed, as they mainly supposed the same as the research questions.

RC: Page 6, line 29: "heterogeneous": in which sense?
AR: We addressed this point by rephrasing this sentence: “This field site consists of spatially heterogeneous steep fractured bedrock with partially debris covered ledges.” See page 6, line 19.

RC: Page 6, line 30: rainfall, cold winter temperature, exposure etc.: please provide quantitative values/ranges typical of the studied environments.
AR: Unfortunately, we have limited weather data for this field site and no representative weather station of the Swiss Meteo Station Network, which is close to the field site and in a similar elevation. But we inserted the MAAT and maximum wind speed locally measured in the years 2011-2012 (see page 6, lines 19ff). We added three pictures distributed over a year (taken in the morn on 01 Jan 2015, 03 Apr 2015, 01 Jul 2015 and 01 Oct 2015) to illustrate the variability of snow deposition (see Figure 4).

RC: 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?
AR: The depth of rock temperature measurements (0.1, 0.35, 0.6 and 0.85 m) are given by the installation of Hasler et al. 2012. The extended Table 1 on page 9 gives an overview of the available temperature in rock and fracture at different depths. A selection of the rock temperature time series are shown in Figure 6 (at the end of this reply a similar figure with temperature gradients calculated by ($T_{0.85\,m} - T_{0.1\,m}$)/0.75 m is shown). For the new analysis, temperature measurements in fractures at different depth are included. Applying a best fit analysis using all available rock and fracture temperatures, we determined the most representative temperature measurement (which are in most cases at 0.85 m depth) for modeling the reversible thermo-mechanically induced fracture kinematics. The optimized trainings windows are shown in Table 2 on page 13.

RC: 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?
AR: These images are mainly used for inspection of the instrumentation, but also provide information about the snow deposition. Currently, we do not derive displacements. This would be the scope of an other project.

Page 7, line 13: "aggregated": cumulated or averaged?
AR: The data was aggregated by averaging.

RC: Page 8, line 4: "Staub et al": manuscripts in review are not citable.
AR: This publication is accepted now and published as early view article.

RC: Page 8, line 9: "Used temperature ....at 85cm depth": why?
AR: This point was addressed in detail in the author response to the referee comment RC Page 7, line 5.

RC: 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.
AR: We addressed this point and explained the linear regression model in more detail. We added an additional correlation analysis for defining the trainings phase and a table with the statistical performance (Table 2, page 13). In principle, LRM can be applied the same way to shear and normal fracture kinematics, but is much more sensitive to the geometric mesoscale arrangement of the fracture. Assuming for instance the rock masses aside the fracture have the same size and thermal condition, the thermo-mechanically induced strain is also the same and no kinematics along fracture is measured. For one location (*mh08*), we added in the supplements a figure illustrating the modeled reversible, thermo-mechanically induced kinematics (Figure 13, page 22).

RC: 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.
AR: We agree that smoothing over 28 days may attenuate variations on short timescales. We adapted the irreversibility index, run the index function (Equations 3 + 4 on page 10) with a sliding windows of 21 days and do not explicitly smooth the data any further. Anyway, the irreversibility index aims at detecting periods, when the irreversible fracture kinematics dominates. On the one hand, it helps to interpret potential forcing and on the other hand, it should enable to assess the stability and not to predict rock slope instabilities.

RC: 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.
AR: We rephrased this sentence and do not refer to a process anymore. The referee is right, we can not separate the population of reversible vs. irreversible kinematics during the training phase. We assumed that the irreversible kinematics is negligible during the trainings phase, which is confirmed by the coefficient of determination given by the regression analysis (see Table 2, page 13).

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.
AR: The LRM+ model was removed. See comment above.

RC: 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.
AR: This figure was removed according to the explanation in RC: P10, lines 21. Instead, we analyzed the whole time series, focusing on the irreversible fracture kinematics after removing the reversible part from the raw data.

RC: 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.
AR: We think this statement is still valuable and very well supported by data. Individual fractures seem to respond quite differently. Multiple spatially distributed locations with different characteristics as exposition or slope, including fractured rock masses, give an idea of spatial variability. Single measurement points enable to investigate the kinematics at small scale, while an array of measurement points can help to assess the stability of the instrumented area.

RC: 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?)
AR: We appreciate this note. We clarified this in the caption of Figure 7: "Black lines indicate the linear regression function determined by the regression analysis (see Table 2)." Table 2 provides the regression parameters (selected temperature, trainings phase, parameters intercept and slope of regression function, correlation coefficient and coefficient of determination).

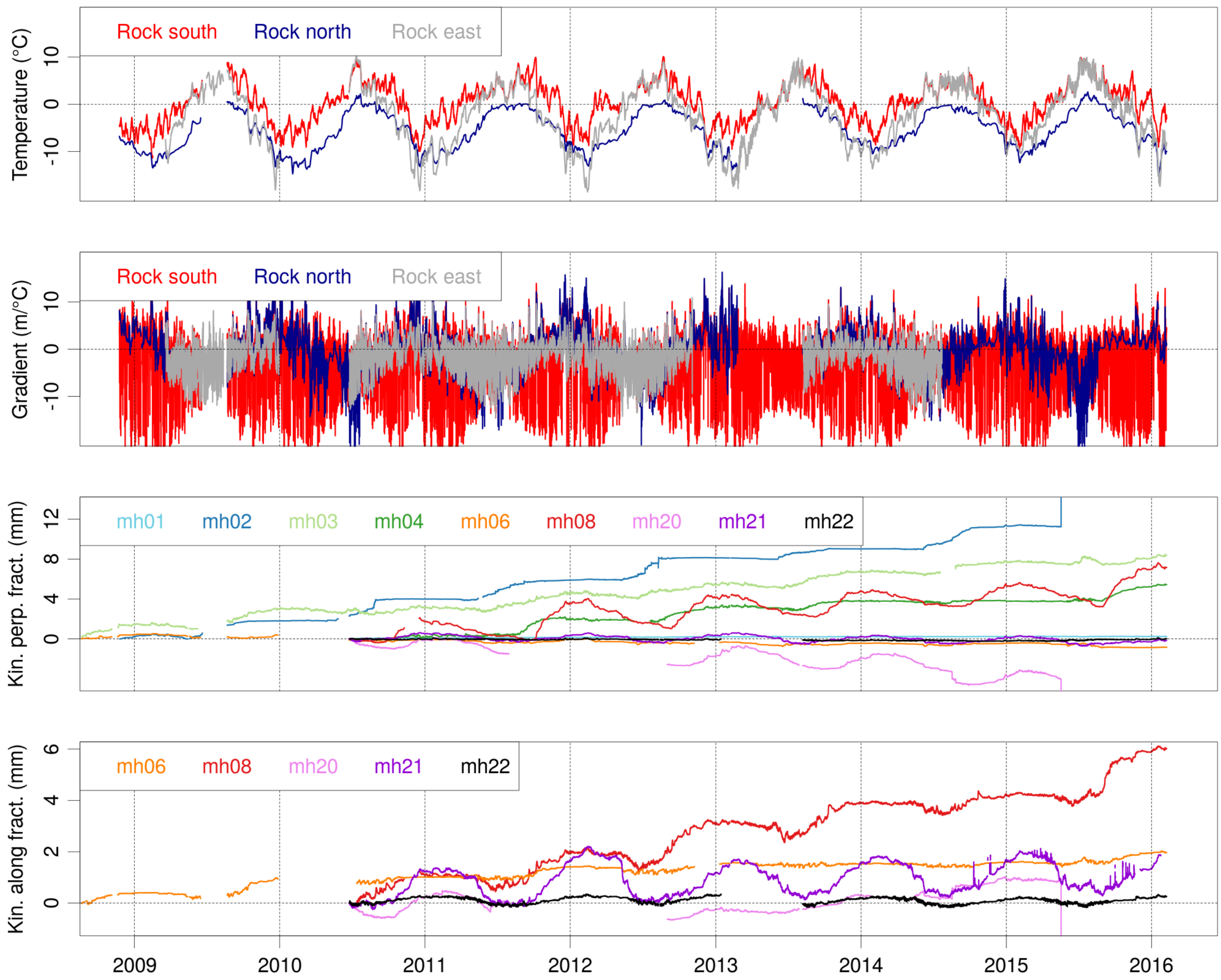
RC: Page 13, lines 4-5: "note that. . ...deformation": incomplete statement.
AR: The LRM+ model was removed (see previous comments).

RC: 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.
AR: This section was removed and the full 7-year monitoring series without reduction is now discussed/explored in more detail. However, we end up with similar results showing that fracture kinematics at most locations consists of reversible thermo-mechanically induced strain, creep phase during thawing period and fracture opening in autumn when temperatures drop below 0° C.

RC: 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.
AR: We don t really understand this comment: We specifically build an index to analyze our data, and could eventually link its variations to environmental conditions. Moreover, we specifically mention this as a possible interpretation.

RC: 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?)
AR: See comments above.

RC: 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)
AR: We disagree on this point. With a qualitative analysis, it is very difficult to assess the relative contribution of reversible versus irreversible displacement and in particular the timing/evolution of irreversible displacement. This timing is however crucial in relation to the environmental forcing (melt, freezing, precipitation, …) and hence relating it to potential responsible processes.
This work provides a new quantitative analysis based on a significantly longer time series (7 years vs. 2 years). Furthermore, the developed irreversible index may be a useful measure for evaluating on rock wall stability.


Rock south
Rock north
Rock east
Temperature (°C)
10
0
-10
Gradient (m/°C)
mh01
mh02
mh03
mh04
mh06
mh08
mh20
mh21
mh22
Kin. perp. fract. (mm)
12
8
4
Kin. along fract. (mm)
6
2
2009
2010
2011
2012
2013
2014
2015
2016