Dear Editor, dear referees,

We would like to thank the two anonymous referees for their careful and meticolous reviews, pointing out parts that lacked clarity, weaknesses, redundancies, but at the same time helpfully suggesting ways to overcome the observed deficiencies. The reviews motivated us to substantially revise the manuscript. All reviewer comments were carefully considered and most of them taken into account. Our reply to the individual points in the two reviews (both those accounted for and those where we did not fully agree) are detailed out below. We believe that the manuscript has - thanks to the reviews - undergone a substantial improvement and hope that it is now fitted to be published in The cryosphere.

Sincerely yours,

for the author team: Regula Frauenfelder

Reply to comments made by Anonymous Referee #1 (doi:10.5194/tc-2016-223-RC1)

We thank Anonymous Referee #1 for its critical review and suggestions for improvement. Referee comments indicated as "**RC**:" and the author's reply as "***AR***:".

RC: Rockslides in deglaciated terrain can be caused and triggered by a lot of different processes such as debuttressing, sheeting joint development, seismicity, hydrostatic pressures and also permafrost (McColl, 2012). The authors use a single hypothesis approach to conclude that the rockslide was triggered by permafrost degradation. The effect that ice is present in the landslide scarp and warming occurred indicate that permafrost could be involved. Other potential scenarios such as hydrostatic pressure increase due to snowmelt or static fatigue are also possible and should be discussed.
***AR:** The authors acknowledge that an extensive discussion of other potential triggering scenarios is missing. We consider other possible triggering processes as unlikely: debuttressing is not likely since no pre-failure action was identified or noticed; sheeting joint development typically follows the topography, the failure surface was, however, a wedge into the slope face; seismicity: our study area is in a low seismic hazard zone, and there were no corresponding seismic events recorded. Hydrostatic pressure increase due to snowmelt or static fatigue could have played a role. This aspect is now brought into the discussion.*

RC: Furthermore, the effect of permafrost on rock stability or permafrost degradation on rock instability is insufficiently understood. According to the authors, the shear plane is located in depths up to 40 m, therefore, the scenario that fracture ice failed as described by Davies et al. (2001) as a trigger of the rockslide is unlikely. Krautblatter et al. (2013) demonstrated that fracture ice cannot influence stability in depths more than 20 m due to the overburden pressure of the rock mass. They provide two other processes that control rock stability: (1) intact rock bridges and (2) rock-rock-contacts. Permafrost increases the uniaxial and tensile strength of these rock bridges and rock-rock-contacts.
A warming of permafrost would decrease both tensile and uniaxial strength and could trigger rockslope failure (Krautblatter et al., 2013). For a detailed discussion on the interaction between thermal and mechanical processes see Draebing et al. (2014). These authors also provide information on the seasonal

timing of rockslope failures and long-term development of rock stability which you should incorporate in your discussion.

***AR:** We agree with this comment and have, therefore, added the main findings (relevant for this paper) of the studies by Krautblatter et al. (2013) and Draebing et al. (2014) into the discussion.*

RC: The mechanical aspect of this paper is poorly addressed, process understanding does not reflect current knowledge, methods are not well introduced and results are not critically discussed.

***AR:** The paper has been updated to include reference to state-of-practice papers that correspond to the methods conducted in this research. The discontinuity orientation extraction and mapping was completed using standard techniques (according references are now given) for data of this type. It is not suitable for automated feature mapping. The criticality of the discontinuity orientations is not the subject of this paper, nor is it essential in the authors' opinion. Numerous authors have widely published on this topic, we are simply using the extracted information.*

RC: Landslide terms should be used for clarification.

***AR:** The authors have updated the text describing the landslide as a rock avalanche based on the terminology proposed by Hungr et al. (2014).*

RC: The authors derived three post-failure TLS scans but they did not discuss how they derived the estimated volume of 500,000 m3. One TLS scan should be suited for the estimation; the follow-up TLS can provide information on subsequent rockfalls which is not the objective of this paper. In addition, the processing should be described in more detail.

***AR:** This is now addressed in the text and references have been added. We agree that the monitoring of subsequent rockfall was not the objective of this paper. However, it was an objective of the monitoring program and therefore we suggest to leave the according information (i.e. that the site was scanned in three year) in the paper.*

RC: The authors describe problems with data holes during the processing of the DEM. Can you provide an error estimation and information about resolution and accuracy of your DEM? This is important to estimate the quality of your fracture mapping which is insufficiently described. Fracture determination is an complicated task according to Abellan et al. (2014) and should be described in full detail.

***AR:** The holes in the DEM do not have any impact on the accuracy of the structural measurements, these aspects are not correlated. The accuracy of the measurements from LiDAR data is well documented, and references have been added. There is no need for this to be explained in the text. Abellan et al. (2014) – which Lato is a co-author – is a review paper written for general audiences. The details of converting vector orientations to dip and dip direction requires careful calculation, but in our view, our paper is not the place for it to be discussed as the methodology has been widely published over the past ten years.*

RC: The authors conclude that the bedding surface is steeper that the friction angle which is an important information. Unfortunately, the presentation of this important information is insufficient in the figures and result section.

***AR:** This is briefly, but clearly described in Section 3.1 and on Figure 5. The focus of this paper is not the extraction of discontinuity measurements. The authors feel that the descriptions and figures provided are sufficient given the context and focus of this paper. There are numerous references supplied that outline the methodologies of extracting this kind of information from LiDAR data, is the reader is inclined to read more they can refer to the added references.*

RC: Furthermore, the authors should discuss critically the influence of permafrost on increasing the internal angle of friction as described by Krautblatter et al. (2013), thus, this is the link to the thermal regime you monitored and modelled.
***AR:** The main findings (relevant for this paper) of the study by Krautblatter et al. (2013) are now added.*

RC: To estimate the thermal influence, the authors monitored near-surface rock and soil temperatures and used a 2D model to model rock temperature. For this purpose, they used three different temperature loggers. Resolution and accuracy of the logger types should be introduced and the influence on temperature records quantified.
***AR:** We introduced information about resolution and accuracy for the loggers in the text: "The absolute accuracy for all three logger types is ± 0.1 °C. The resolution of M-Log5W, UTL-3 and UTL-1 is 0.01, 0.02 and 0.27 °C respectively. The M-Log5W loggers and the UTL-3 loggers were programmed to measure every 30 minutes, while the UTL-1 loggers (having smaller memory capacities than the other two employed logger types) measured every two hours. Based on experiences from long-term permafrost monitoring programs in Norway using such loggers (e.g., Isaksen et al. 2011; Gisnås et al. 2016) we claim that the bias on the accuracy of the temperature measurements introduced with the given setups is negligible."*

RC: The location of loggers is introduced, however, further information on altitude, aspect, distance to rock ledges and slope angle is required to estimate the influence of topography and snow cover. Up to now, there is temperature information on rockslopes in Norway measured by 3 data loggers by Hipp et al. (2014). The information of this manuscript can provide new interesting data on rock temperatures in this environment. Hipp et al. (2014) showed that near-surface rock temperature distribution is different to the European Alps. The authors should discuss in more detail this difference and potential causes. Please include the influence of snow cover on the thermal and mechanical regime in steep rockwalls as recently addressed by Haberkorn et al. (2015a; 2015b) or Draebing et al. (2016).
***AR:** We included a new table (Table 2) with elevation, aspect and snow conditions for the logger sites. A comparison of aspect dependency at our site as compared to sites in the European Alps has been added to our discussion. We also added some text related to the influence of snow cover on the thermal and mechanical regime in rockwalls, referring to important recent literature in the field (e.g., Haberkorn et al., 2015a, b; Magnin et al., 2015).*

RC: In the next step, the authors used this information to model ground temperatures. They used the CryoGrid2 model which provides a resolution of 1 km2 (Westermann et al., 2013). Consequently, the Polvartinden rockslide is presented by two pixels. Subsurface material is derived from a geological map and is till and colluvium according to the authors. Therefore, CryoGrid 2 is not suited to model rockwall temperatures, thus, resolution is too coarse and subsurface material different than bedrock.
***AR:** We agree that CryoGrid2 may not be the best choice to model rock wall temperatures. However, as written in the Methods section the larger area around the failure zone, outside the steepest slope, is characterized by a combination of small vertical rock outcrops and undulating slopes with an established soil and snow cover. Consequently, temperature loggers were installed in both types of terrain, i.e., in both vertical rock outcrops and within soil material of the more gentle slopes. To be representative for the latter type of terrain it was desirable to look further into the temperature development in the ground in areas close to the release area with a more established snow and soil cover. To this aim, the CryoGrid2 model (cf. Westermann et al., 2013) was used. In our opinion the model is very well suited to study long-*

term changes in ground temperatures representative for the same elevation as the failure zone for sites with more gentle slopes and a developed soil cover and where snow accumulates.

We have tried to make these points clearer in the current version of the manuscript, for example by explaining them better in the method chapter and by highlighting them in the results section and in the discussion.

RC: Models such as CryoGrid2D used by Myhra et al. (2016) or modeling approaches by Noetzli et al. (2007) are better suited. The latter approach is also used in this paper but input data, model parameters such as chosen thermal conductivity, ice content and porosity, or data processing is not introduced as well as resulting effects on modelling results are not quantified or discussed.

***AR:** As written in section 2.3.2 the applied model by Noetzli et al. (2007) was used to get insights into the present subsurface temperature field of Polvartinden and is valid for areas that are assumed not to be influenced by a snow cover, i.e., the steep rock-faces of Polvartinden. In our paper we present subsurface properties in Table 1 that were obtained from representative sites nearby (cf. Lilleøren et al., 2012). The model applied has been widely presented and discussed in earlier literature, which we refer to in the text. We do not see the need to go into detail about data processing, etc. of this model in this paper.*

RC: Comments and suggestions in the supplement.
***AR:** We were impressed by the level of detail and meticulousness of the comments and suggestions in the supplement. Some of them are answered by our replies to the review comments (above and below), all others were carefully considered and amendments made were we agreed with the comments/suggestions.*

Reply to comments made by Anonymous Referee #2 (doi:10.5194/tc-2016-223-RC2)

We thank Anonymous Referee #2 for its critical review and suggestions for improvement. Referee comments indicated as "**RC**:", author reply as "***AR***:".

RC: 1. The manuscript is lengthy and reads like a report rather than a scientific paper. Methods and results are widely inter-mixed.
***AR:** We do not agree with this comment and reviewer #1 did not comment something in this direction. We have, therefore, not altered the structure of the manuscript. If a change of structure is suggested by the editor, we will of course do that.*

RC: 2. Observations: There has been employed 14 loggers around the mountain, in the end 9 of them were used. However, for the reader it is difficult to see where the loggers are placed, in relation to possible snow cover and topographic aspect. You should give a table of logger description, inkl. elevation, aspect etc. Fig. 3b is not useful within this respect; please give a map rather than an image.
***AR:** We included a new table (Table 2) with elevation, aspect and snow conditions for the logger sites. Figure 3b has been updated, now featuring a map as base layer instead of an aerial photo.*

RC: 3. Setting and geomechnical mapping: A setting chapter is lacking, the info is part of the introduction. For readers not particular well-known in the area, I would suggest to provide general geophysiographic setting of the area, including general climate parameters and the regional distribution of permafrost. I do not see the point of the kinematic analysis here. You could simply describe that in the setting chapter as background for the site.

***AR:** See above. We have not changed the structure of the paper at this stage.*
The kinematic analysis was an objective of the overall monitoring program and is important in order to increase our understanding of the possible failure mechanisms that led to the Signaldalen rock avalanche. We believe that the now improved discussion of the mechanical and thermal controls on rock stability (triggered by the comments by reviewer #1) justifies the inclusion of the kinematic results.

RC: 4. CG2 model: A major part of your conclusions are based on the results of the CG2 modelling. First of all, the description of the model, its principles etc must be given even if details are explained in another publication. The same is valid for the reasoning of the parameter choice.

***AR**: We agree that the description of the model, its principles and the reasoning of the parameter choice must be given, so we have added according information to section 2.3.1.*

RC: Last not least, snow is of course here a problem. What snow cover have you assumed? Are there any observations? I understand that the forcing is based on gridded data? What is the relation between the gridded data and e.g. a long met series from one of the met stations nearby?

***AR**: We added the following text in 2.3.1. to make this clearer: "The model is forced by operational gridded (1 x 1 km) air temperature (Mohr, 2009; Tveito et al., 2000) and snow-depth (Engeset et al., 2004; Saloranta, 2012). Snow cover data is based on the seNorge snow model that uses gridded observations of daily temperature and precipitation as its input forcing, and simulates, among others, snow water equivalent (SWE), snow depth (SD), and the snow bulk density (ρ) (Saloranta, 2012). The gridded air temperature for our site is mainly driven by the nearby Skibotn and Rihpojavri weather stations which were validated against our local measurements (see section 3.2). The gridded precipitation for our site is mainly driven by observed precipitation at Skibotn weather station. Since we have no observations of snow cover and owing to the large spatiotemporal variability of snow conditions in our alpine study area, the snow simulations from the seNorge model provide probably the best estimate of the spatial average 1 x 1 km snow conditions for our site (cf. Saloranta, 2012)."*

In addition, according to Westermann et al. (2013) the thermal conductivity of the snow is the largest source of uncertainty in CG2, thus a low (LC) and a high conductivity (HC) scenario run of CG2 are used as a confining range for the true conditions.

RC: A major problem is of course the lack of validation of the model. I understand that there is no borehole at the study site for ground temperature validation. But you could check the modelled ground surface temperatures against some of your loggers? As the snow cover and subsurface parameters are very uncertain and not validated, and the model is certainly based on heat conduction, the model mirrors of course the air temperature forcing. So, it is not an independent support of the findings from the long-term air temperature analysis, which should be discussed somehow.

***AR**: We now checked the modelled ground surface temperatures (for 0.1m depth) against our soil temperature loggers for the first year of measurements when CG2 data were available and added a new figure (Fig. 8) visualizing the results. The following text (going with the new Fig. 8) has been added:*

"A one-year comparison of observed daily SST at sites with an established soil and snow cover and the modelled SST from the CG2 model is shown in Figure 8. As seen in Figure 8a the SST observed in the valley floor is in good agreement (R^2 = 0.86 to 0.88) with the CG2 model results for a grid cell with approximately the same elevation as the valley floor. The Signaldalen valley is dominated by mountain birch forest and is characterized by an open forest layer with heather and lichen species dominating the forest floor layer. This type of forest causes snow to accumulate and insulates the ground against strong cooling (cf. Isaksen et al., 2008; Farbrot et al., 2013), an effect which can be seen in our CG2 model results. In the mountains (Fig. 8b) there is a somewhat weaker correlation (R^2 = 0.69 to 0.82) between observed and modelled data. The MASST are about 1 to 1.5°C lower than in the CG2 model for the corresponding grid cell with similar elevation. The warm bias of the simulations during winter is mainly explained by the different snow conditions assumed in the model: while our observational sites in the mountains are affected by considerably strong snow cover variations induced by wind drift, the snow model used as input in the CG2-model generally overestimates snow depths in high mountain areas (cf. Saloranta, 2012). This is also in line with results of an equilibrium permafrost model used by Gisnås et al. (2013) who found a better agreement between the CG2-model and their validation data when the snow depth in the snow model data was reduced by 30 % for areas above the tree line. During summer, on the other hand, our observed SST in the mountains are well reproduced by the CG2 simulations."

RC: 5. 3D model: With the use of the 3D model I had some problems. The authors acknowledge that there are large uncertainties about the aspect-dependency of ground surface temperatures. The basics of the aspect dependency are related to the measurement array, which partly was influenced by snow cover etc. The author found an aspect dependency of c. 1◦C, which might be something between 150 and 200 m in elevation given certain lapse rates. They show fig. 11a, with a polynomial fit to 8 points, which is not statistically sound (e.g. what are the p-values for this fit).

***AR:** We agree that a polynomial fit to eight points may not be statistically sound. However, our curve fitting must be seen as an attempt to capture the main patterns in our data, rather than to deep-dive into questions about the probability for a given statistical model. Our results should be seen as tentative estimates (as already emphasized in our discussion). The authors feel that the figure, descriptions and discussions provided are reasonable and in line with previous studies using similar approaches, also suffering of a limited number of data points (e.g. Hipp et al. (2014) and Gruber et al. (2004) used 5 and 14 rock-wall temperature data loggers in their studies, respectively).*

RC: These data were used to force the 3D model, and the authors choose to show a slice from a north-south oriented section. There are several problems here: (1) The isolines become more or less horizontal, so the plot does not give much new information in relation to the analysis of the data loggers or the 1D model. (2) The uncertainty is very high here, which the authors also mention, so I wonder if there is any justification of this analysis, beside showing a nice figure, and that such analysis are possible? (3) What was the initialization used? I do not understand page 7, line 15 etc. Does the model now show more or less the same as the 1D model, and is its use scientifically justified here? Especially in the light that also snow cover was neglected. Ok maybe for vertical rock walls, but is this a good approximation for your study site?

***AR**: Permafrost is a subsurface phenomenon mainly driven by the state and changes of the surface temperatures. We do not claim to provide a real model of the subsurface temperature field (which would require many more data points as well as a statistically sound analyses of the factors influencing the spatial distribution), but rather to schematically illustrate the general pattern of the subsurface temperatures in a mountain such as the Polarvatiden and to what extent it is still influenced by smaller differences between in MAGST at the different mountain sides. Our main point with using this model was a rough visualization of the permafrost body and the general pattern of the subsurface temperature field in the mountain. This cannot be achieved with a 1D-profile. The model illustrates the extent of the permafrost body within the mountain and in the larger area of the starting zone.*

The text for the initialization and the transient model run has been adapted. The model was initialized for the temperature conditions at 1900 (it is assumed that due to the smaller size of the mountain no earlier surface temperature variations are significant for the subsurface thermal regime, cf. also Noetzli and Gruber, 2009). Then a transient simulation was run with the upper boundary condition (that is the distributed MAGST) linearly increasing by 0.55 °C until the current situation.

RC: 6. Conclusions: The conclusions are a bit thin. I agree that you have indications for a thermal trigger. But only state what you can justify with your observations and/or models. I do not understand what the passage of activity in the slide contributes here? You may omit that.
***AR:** We slightly expanded the conclusions, however, we believe that conclusions should be kept short and concise.*
We do not understand the comment about the "passage of activity".

RC: Introduction: see comments above. I suggest to make a setting chapter in addition
***AR:** See above. We have so far not altered the structure of the paper.*

RC: p.4, First three paragraphs: Delete, not necessary with a summary first, and details afterwards. Include in the detailed method description.
***AR:** See above. We have so far not altered the structure of the paper*

RC: p. 5, l. 4-10: This is typical setting, move,
***AR:** See above. We have so far not altered the structure of the paper*

RC: p. 5: l. 11-20: The laser-scan is not necessary here. Figure 4 is not understandable at all, at least not for me, and you can simply write that repeated laser scan analyses did not reveal large movements after the event. In the suggested setting chapter.
***AR:** We agree that the figure was poor and have made an improved, updated version.*

RC: p. 5, l. 25 ff: Give resolution/precision of loggers.
***AR:** This has now been added: "The absolute accuracy for all three logger types is ± 0.1 °C. The resolution of M-Log5W, UTL-3 and UTL-1 is 0.01, 0.02 and 0.27 °C respectively. The M-Log5W loggers and the UTL-3*

loggers were programmed to measure every 30 minutes, while the UTL-1 loggers (having smaller memory capacities than the other two employed logger types) measured every two hours"

RC: p. 6, l. 11-15: Delete whole paragraph, nothing new.
AR: *Agreed. The paragraph has been deleted.*

RC: p. 6/7: About the models, see comments above. Especially p 7, l16 is problematic. How many places in your study area are really not covered by snow?
AR: *See our reply above and new information about snow cover included in the new Table 2.*

RC: p. 8, l. 1-16: I am not an expert her, but what is the point of this in relation to your hypothesis, e.g. triggering may have been related to permafrost thaw?
AR: *Triggered by the comments by reviewer #1 we have considerably increased the discussion of the mechanical and thermal controls on rock stability. We hope that our hypothesis that triggering might have been related to permafrost warming is now more clearly justified.*

RC: p. 8, l 25: What is the rationale behind to use always a 1a-mean?
AR: *For several of the analysis presented annual mean values were calculated to identify variations in mean annual ground surface temperatures and mean air temperature. This ensures easier comparison between the monitoring sites and makes it easier to identify local maxima and minima as well as trends (cf. Isaksen et al., 2011). This argumentation is now included in the revised manuscript.*

RC: p. 8, l 28: Reference to an EGU abstract is not acceptable for international journals as the data cannot be reproduced. So either show the data, or give them in an appendix or make a figure to include here.
AR: *We omitted the references to the EGU abstract.*

RC: p. 9, 1. Paragr. : This is important for your reasoning, so you may show that somehow.
AR: *See above, we included a new figure (Fig. 8) and commented in detail on the findings visualized in Fig. 8.*

RC: p. 10: Why did you choose 10 m depth for the CG2 model?
AR: *Westermann et al. (2013) found 10 m to be an appropriate depth to study long-term changes in ground temperatures but avoiding dominance of near-surface high-frequency temperature variations.*