Report #1

Submitted on 01 Sep 2017
Anonymous Referee #3

Anonymous during peer-review: Yes No
Anonymous in acknowledgements of published article: Yes No

Recommendation to the Editor

1) Originality (Novelty) Within the scope of The Cryosphere, does the manuscript represent substantial progress beyond current scientific understanding (new insight, concepts, methods, or data)?	Excellent **Good** Fair Poor
2) Scientific Quality (Rigour) (A) Is the purpose of the work clearly articulated, reflected in an adequate methodology, and its achievement compellingly underpinned by the evidence presented? (B) Are the applied methods and techniques valid and suitable? (C) Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?	Excellent **Good** Fair Poor
3) Significance (Impact) Does the manuscript contribute to changing our scientific understanding of a subject substantially or to introducing new practical applications of broad relevance?	Excellent **Good** Fair Poor
4) Presentation Quality Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?	**Excellent** Good Fair Poor

For final publication, the manuscript should be

accepted as is

accepted subject to **technical corrections**

accepted subject to minor revisions

reconsidered after **major revisions**

I would like to review the revised paper

I would NOT be willing to review the revised paper

rejected

Please note that this rating only refers to this version of the manuscript!

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Comments to the paper:
Ground thermal and geomechanical conditions in a permafrost-affected high-latitude rock

avalanche site (Polvartinden, northern Norway)

by Regula Frauenfelder, Ketil Isaksen, Jeannette Noetzli, Matthew J. Lato

This manuscript describes a rock fall event happened in 2008 at Polvartinden, Northern Norway. The authors estimate the volume of the rock fall and they investigate possible permafrost conditions at the detachment site of the rock avalanche. They use methods like ground based laser scanning to establish a digital terrain model and ground temperature measurements to determine the ground thermal regime. The measurements are also used to drive different models such CryoGrid2 (by Westermann 2013) and a 3D-heat conduction model (by Noetzli 2007).

General comments:
****

This study is elaborated very carefully using a minimum amount of measurements. Although having not a lot of direct measurements available, I believe that the authors got most out of the data using them in a very smart combination with other existing data (like meteorological information of metno) and numerical models (like CryoGrid2).

In general, the paper is written very concise and clear. I have only some minor points, where I suggest some changes in the paper. Most of them are directly related to the section below ‘specific comments’.
I suggest to introduce a short chapter ‘Site Description’, where the authors should collect all information from the Introduction and Method section related to the site description. This would make the structure of the paper more clear.

Specific comments:
****

Abstract:
Page 1, Line 24: change this by these
AR: Changed according to reviewer suggestion.

Introduction:
Page 2, Line 4: it would be interesting to now at which depth the ice vertical to the surface BEFORE the rock fall was situated. This should be easy knowing the surface before and after the rock fall. It can tell us may be also something about the heat and water fluxes. Was the ice visible in Figure 2 not dated? If yes, this information would be of outstanding interest.
AR: Unfortunately, we don’t have an accurate pre-failure surface (apart from a nation-wide 15 m digital elevation model). From our interpretation of photographs and LiDAR intensity values, we can approximate that the biggest visible patches of ice were located ca. 20-30 m below the pre-failure surface (this information has now been incorporated in the text). This is only a rough estimate, however. Also, the photographs did not allow to map all the ice that was visible during site-inspection (personal communication by K. Brattlien, NGI who was in the helicopter on June 26th 2008 taking the pictures).
The ice visible in Figure 2 was not dated.

Page 3, Line 3: what means rock avalanche deformation … Do you mean here rock avalanche formation?
AR: We agree that the word "deformation" can be deleted here. We changed the sentence to the following: "…on the interactions between permafrost and a potential rock avalanche site at …"

Page 3, Line 18: I was wondering if a 10 m active layer corresponds with the observation of the ice in the rock avalanche detachment zone. Therefore, it is very interesting to know the rock depth above the ice at the detachment zone before the rock avalanche happened.
AR: Please see also our answer above. What we DO know quite accurately, is that the two biggest patches of visible ice were located at ca. 10 m and ca. 20 m, respectively, above the basal failure surface. We do not know, however, the depth of smaller visible ice patches that were observed during the helicopter reconnaissance tour. A ≥10 m active layer thickness would correspond to active layer depths observed in permafrost bedrock sites which are under degradation in northern Norway (cf. Farbrot et al. 2013; Gisnås et al. 2016).

Page 3, Lines 23-26: I would rewrite the sentences accordingly: For the same mountain Myhra et al. (2015) modelled, a bedrock warming at 20 and 50 m depth of about 1.0 and 0.5°C, respectively, from the end of the LIA and using a present lower limit of permafrost of about 650 m asl. A new Nordic permafrost map based on modelled results confirm ongoing degradation of mountain permafrost in the fjord areas in Troms (Gisnås et al., 2016).
AR: Changed according to reviewer suggestion.

Page 3, Line 29: … subsequent modelling of the temperature regime …
AR: Changed according to reviewer suggestion.

Page 3, Lines 32-33: You certainly discuss the influence of solar radiation very good in the discussion section. However, as you state it here in the Introduction as a main goal of the paper, you definitively do not contribute a lot with measurements or modelling to better understanding of solar radiation. Therefore, I would skip this sentence. Although, I do full agree with your comments about this topic in the Discussion chapter.
AR: We boldly claim that our results from Signaldalen contribute to a new understanding of the influence of aspects in northern areas (in fact, North of the polar article), since there are very limited publications available on this topic from Northern Norway/Northern Scandinavia, and the Arctic in general. Therefore, we choose to keep the sentence here but change it to: "Further, our results contribute to the understanding of the influence of solar radiation on near surface temperatures in different aspects of steep mountain walls at high-latitudes."

Between Introduction and Methods chapter, please insert a chapter Site Description.
AR: We agree with this comment and have now introduced a "Site Description" chapter, where we shortly comment on regions tectonics and main bedrock geology, and the glacial history of the area. In addition, we collected the relevant passages originally located within the Introduction and Method chapters and included them into this new chapter.

Methods:

Page 4, Line 5: Insert: … subsequent ground temperature modelling …

AR: Changed according to reviewer suggestion.

Discussion:

Page 13, Line 11: Please mention also study of Heggem et al. 2005 showing the influence of of radiation on permafrost in Southern Norway from west to east. Heggem, E. S. F., Juliussen, H., and Etzelmüller, B. (2005). "Mountain permafrost in Central-Eastern Norway." Norwegian Journal of Geography 59(2): 94-108.

AR: We do not agree that the study by Heggem et al. (2005) is fitting into this part of the discussion. However, in order to acknowledge this study, we have now added it as a reference to the Introduction, to where earlier studies on the permafrost distribution in Norway are mentioned: "Early studies by, e.g., King (1986), Ødegård et al. (1996), Etzelmüller et al. (2003) and Heggem et al. (2005) have shown that permafrost is discontinuous in the higher mountains of central southern and eastern Norway."

Figures:

Page 29, Figure 6: please change colours of the lines fitting them better thematically to the different loggers put at rock and soil sites.

AR: We have now revised the figure so that the differences between rock sites (thick lines) and soil sites (thin lines) become clearer. We want to keep the colour consistency, however, both in terms of the colours used in Figure 6 matching the same logger used in Figure 8 and Figure 9, and – and even more important – to signal "warm" and "cold" colours in accordance with the widely used colour schemes appropriate for Scientific Data Graphics for temperature data, with a subjective interpretation of blue = cold, and red = hot.

Report #2

Submitted on 12 Oct 2017
Referee #4: Michael Krautblatter, m.krautblatter@tum.de

Anonymous during peer-review: Yes **No**
Anonymous in acknowledgements of published article: Yes **No**

Recommendation to the Editor

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Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Dear Dr. Frauenfelder,

I have carefully read your paper also in comparison to the first submitted version and I have found a large number of issues that I would have commented in the earlier version now carefully revised in the present Version 4. Earlier reviews have commented a lot on the thermal permafrost content in this paper, so I focussed a little bit more on the geological and slope instability content of this paper.

I made comments on a number of issues relating to
>the introduction of the structural and geological controls of the rock slope failure
>the mechanical explanation of the failure
>the terminology
>the structural interpretation
>and the way permafrost could have played a role.
However, even if there is quite a number of these comments, they are all minor revisions in my view. I hope they will help to make some statements including the key statement of a potentially permafrost-affected rock slope failure clearer. I also added some comments on thermal conditions in steep structured rock faces.

Including these comments, I can see that this paper is a valuable contribution to better understanding preparatory conditions of rock slope failures in permafrost-affected rock walls in a setting that is quite different from previously reported Alpine conditions.

Thus, I suggest to publish this paper subsequent to a number of minor revisions listed below

Good luck with the revision
Michael Krautblatter

P2, L25: >I think you should include a statement that the evidence of permafrost-related destabilisation is not only derived (abductively) from observation, where the relative importance of permafrost degradation remains difficult to prove, but also supported by (inductive/deductive) mechanical studies that explain the underlying physics and processes of destabilisation. Without mechanical papers by Mellor, Davies, Guenzel, Dviwedi, Arenson, this statement could not be validated physically.
AR: We fully agree with this comment and have added a corresponding sentence, including references to relevant studies, such as the ones mentioned by the reviewer above, and additional ones that are of relevance in this context.

P3, L1: “Large-scale rock-slope failures pose a significant geohazard in the fjords and valleys of western and northern Norway (Blikra et al., 2006).”
>I think you should update and specify this comment since, especially as Norway has a very elaborated ranking of rock slope failure hazards now.
AR: We fully agree with this comment and added the following text: "The fjords and valleys are confined by steep mountain slopes, which leads to a limitation of areas where human infrastructure can be built; this results often in the infrastructure being located close to the steep mountain sides. Large-scale rock-slope failures therefore pose a significant geohazard in the fjords and valleys of western and northern Norway (Blikra et al., 2006), both due to the potential of a direct hit in case of a slope failure, but also due to the potential of rock slope failures generating tsunamis in the fjords and damming up rivers in the valley floor, with subsequent risk of major floodings. Mass movements generated from such slopes and their secondary effects have caused hundreds of fatalities during the last

500 years in Norway. Understanding of the destabilization processes and the classification of their risk has therefore become a major field of research (Hermanns et al., 2012; 2013; 2016)."

P5, L9: “Low resolution data was collected to enable the visualization and modelling of a large area of the mountainside. Higher resolution data was collected at specific zones near the failure zone and its surrounding areas to enable a geomechanical interpretation.”
>Specify the resolution of LiDAR especially for the term “Higher resolution data”, since this affects the geomechanical interpretation that can be achieved.
AR: The higher resolution data was collected at a resolution of 0.20 m point spacing (~25 pts/m^2). Given the scanning distance, this was the highest reasonable resolution of data we could collect at the time with the available LiDAR technology. A paragraph explaining this is now included in the text: "Higher resolution data was collected at specific zones near the failure zone and its surrounding areas to enable a geomechanical interpretation. This data has a resolution of approximately 0.20 m point spacing (or 25 pts/m^2). Given the scanning distance, this was the highest reasonable resolution of data we could collect at the time with the available LiDAR technology."

P5, L13: In the previous version you speak about “The extremely irregular surface as well as large amounts of snow and ice present resulted in the development of a mesh with numerous larger data holes”
>Specify the range of dimensions of these ”numerous” holes, since they can camouflage/affect the geomechanical interpretation.
AR: Contrary to earlier versions of this manuscript, we have phrased the text more carefully (less sloppy), which leads to a more accurate description of the size and relevance of the holes, setting their size into perspective to the total size of the model: "Occluded zones on the rock face were limited to surfaces parallel to the scanner orientation and regions with ice cover. The size of these regions varied from 1 m^2 to 10 m^2. These regions do not negatively impact the geomechanical interpretation since they represent less than 1% of the total model area."
With this description, we hope that it will become more evident to the reader, that the holes do not camouflage the interpretation.

P5, L19 “The volume of the 2008 rock avalanche was computed using the 2012 TLS data by delineating a pre-failure surface based on adjacent slope topography bounding the scarp. The differential volume between the 2012 TLS data and the pre-failure surface was computed using standard 3-dimensional techniques, as presented, e.g., by Lato et al. (2014)..”
>It is quite common that there is not sufficiently resolved 3D DEM before the failure, but please explain from where you took your constraints to interpolate the prior surface. Looking at Figure 4 it seems that the reconstruction of a former surface not straightforward.
AR: The construction of the pre-failure surface was constrained by the interpretation of photographs and intensity values in the LiDAR dataset. Freshly exposed rock surfaces, due to the failure, were easily identified and used as the boundary. Due to the consistent slope morphology, we claim that we were able to estimate the slide topography with sufficient accuracy. We added an explanatory sentence: "The extents of the pre-failure surface was delineated based on the visual contrast between the appearance of the pre-failure and post-failure rock mass."

P6, L2 “Where possible, a vertical distance of several meters to the flat terrain was chosen, however, this was not possible at all of the sites, which has some implications on the interpretation of the results (cf. chapter 4).”
>Please specify for which sensors this applies and whether these logger have been affected by snow accretion
>The specification for rock wall loggers also relies on a minimum steepness of rock faces (often >60°) where snow does not accumulate on the rock face – have you checked the steepness?
AR: The steepness of the logger installations was checked during site selection and lies within the requirements for rock wall loggers (> 60°). As discussed in Chapter 4, all rock wall loggers were installed in small vertical rock outcrops, typically 3-5 m high (rather than in large rock walls). At such settings the snow close and/or atop these cliffs can attenuate normal winter cooling and, thus, affect the results during the winter months. Loggers R03, R04 and R12 were snow covered during winter and, therefore, data was removed for snow-influenced months (cf. Figure caption for Figure 11).

Figure 4: I would not use the term “rock avalanche” used for highly mobilised (low fahrboschung/ energy line angles <15°, which your event does not have) high-magnitude (>1 mio. m^3) events with– rock slope failure is a more appropriate term.
AR: In the first version of the manuscript, we used the term "rock slide" to describe the event, a term which Reviewer #1 requested to be updated to common landslide terminology. We have then carefully consulted several semantic studies, of which the paper by Hungr et al. (2014) who published an update on the "Varnes classification of landslide types" seems to be one of the most widely accepted ones (cited 425 times in its 4 years of existence). According to this classification, the Signaldalen event can be described as an event that transformed into a rock avalanche (this can, e.g., be deduced from its flow-like features that are visible in the deposit area). The Hungr et al. definition of rock avalanches does not say anything about mobility/fahrboschung: “Rock/ice avalanche: Extremely rapid, massive, flow-like motion of fragmented rock from a large rock slide or rock fall.”; although it does go on to say that “large” rock avalanches tend to achieve high mobility. Still, there are plenty of events called "rock avalanches" in the pre-Hungr et al. classical literature (Scheidegger, 1973; Li, 1983; etc.) with travel angles higher than 15 degrees.
The Hungr et al. (2014) system leaves it open to use a composite term like “rock slide – rock avalanche”, however, in our view, this just adds an unnecessary semantic complication to the paper. We therefore decide to stick to the "rock avalanche" term for the Signaldalen event. We hope the editor can accept our argumentation leading to this decision.

- Scheidegger, A. E., 1973. On the prediction of reach and velocity of catastrophic landslides. Rock Mechanics,5, 231–236
- Li Tianchi 1983. A mathematical model for predicting the extent of a major rockfall. Zeitschrift für Geomorfologie N.F., 27, 473-482

Figure 5: “Figure 5. Kinematic analysis of the bedding planes for the sliding failure. The green line represents the orientation of the natural slope surface before failure, the white circle represents an estimated friction cone of 30° and the green cone represents the sliding daylight window for the associated pre-failure surface.”
>This Figure needs a more elaborated explanation
>It is difficult to read it without having any kind of geomechanical background, which

should be described in at least in 4-5 lines in the introduction. Type of geology, discontinuity patterns, deglaciation history…
>For the readership of the Cryosphere you should explain your geomechanical analysis in a way they have a chance to understand it
>Please be careful with attributing the failure to a "bedding plane"
> From what I can see in Figure 1, there are persistent, vertical to subvertical, bended joint sets that predefine possible failure planes. These also seem to have at least partially a fine-grained infill.
>Pure or dirty ice infill can add certain limited (2 MPa) tensile strength to the vertical to subvertical, bended joint sets in the rock mass prior to failure and affect the sliding resistance.
>As it is presented now, I cannot really understand the point you want to make with this Figure 5
>I only have Fig. 1 to judge the geology and no background information is provided in your article, but your "complex wedge" is composed of an intersection of at least three sets of discontinuities and I strongly recommend you to give the reader some more background structural information on the rock face which must have certainly been recorded by the NGI.
AR: The main message behind Figure 5 should have been that the basal sliding plane is orientated with respect to the slope and that this is, geomechanically spoken, an unfavourable condition, which could have led to the failure. Since we did not have the possibility to go close-up to the failure face, we do not really have much more information that we could add. The way the figure was represented by us, seems to invite the readers to read too much into the analysis lying behind it and to look for information which we do not have. We have, therefore, decided to remove Figure 5 from the final manuscript, and have reworded the geomechanical explanations.

P7, L8 Values defining the subsurface properties (heat capacity, thermal conductivity, porosity) were obtained from representative sites nearby
>Please state that they (especially porosity) can vary significantly and have quite an influence on the thermal behaviour
AR: We agree with this comment and have modified the corresponding sentence as follows: "Values defining the subsurface properties (heat capacity, thermal conductivity, porosity; cf. Table 1) were obtained from sites nearby (cf. Lilleøren et al., 2012). Especially for porosity, these values can vary significantly and have quite an influence on the thermal behavior. However, due to the similar geological setting and proximity of our site to the sites described in Lilleøren et al. (2012), we assume these values to be fairly representative for our study location."

P7, L2: "Our results are valid for areas that are assumed not to be influenced by a snow cover, i.e., the steep rock-faces of Polvartinden. The main source of uncertainty for the three-dimensional modelling is related to the extrapolation of the MARST"
>Structured rock faces like yours have quite a significant snow cover an this can be a major error source for your model. Please check recent papers by A. Haberkorn.
AR: We agree that structured rock faces may have a significant snow cover and we, therefore, included a new sub-paragraph related to this (including references to studies by Haberkorn et al.) in the third paragraph of the discussion.
However, data from some of our rock wall sites that were used as input in the transient three-dimensional transient heat conduction model suggest a fairly direct link to the

atmospheric conditions. We found, e.g., a very high correlation between our local air temperature measurements in the Signaldalen valley floor and the rock wall loggers R05 ($R2 = 0.99$; Fig. 7c) and R06 ($R2 = 0.98$; Fig. 7d), which are the "warmest" and "coldest" rock wall loggers, respectively, during the four-year measurement period. For the sites R03, R04 and R12, periods during which the loggers obviously had been snow-covered (cf. Table 2) were omitted. 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 cover. Snow cover and snow depth, therefore, varies considerably on these two different types of topographies. Consequently, temperature loggers were installed in both types of terrain, i.e., in both vertical rock outcrops (Fig. 3a; rock surface temperature (RST)) and within the soil material of the more gentle slopes (soil surface temperature (SST)). Together, we believe that the loggers (including the soil loggers) encompass the variation of the snow and surface conditions present around the Signaldalen rock avalanche site.

P8, L4: "The discontinuity ordination data are plotted on a stereonet in Figure 5. 20 The bedding planes have been identified as the failure surface from the terrestrial laser scanning data and from interpretation of the GigaPan photography. The green line in Figure 5 represents the orientation of the natural slope surface, and the green circle represents the corresponding daylight window. The white cone depicts an estimated friction surface of 30°. Poles that are contained outside of the white circle but within the green circle are kinematically unstable and represent potential sliding failure planes (Goodman, 1995). The stereonet demonstrates that the bedding surface orientation is steeper than the estimated 25 friction angle, but shallow enough to daylight with respect to the slope face. The bedding surface meets, therewith, the kinematic requirements of a sliding failure posing a potential rockfall hazard (see e.g., Hasler et al., 2012)."
>I only have Fig. 1 to judge the geology and no background information is provided in your article, but your "complex wedge" is composed of an intersection of at least three sets of discontinuities and I strongly recommend you to give the reader some more background structural information on the rock face which must have certainly been recorded by the NGI.

AR: The information structural information available from the NGI report is now incorporated in the new chapter "Site Description". In addition, please see our answers to the previous reviewer's comment related to this (reviewer's comments on Figure 5).