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Interactive comment on "Ground thermal and geomechanical conditions in a permafrost-affected high-latitude rockslide site (Polvartinden, Northern Norway)" *by* Regula Frauenfelder et al.

Anonymous Referee #1

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This paper aims to reconstruct the recent and past thermal regime of a rockslide-affected permafrost slope in Northern Norway to estimate the role of permafrost degradation on landslides. Landslides in high-arctic areas are an important process and present hazards to communities. The role of permafrost in preparing and triggering these landslides is currently poorly understood, therefore, this study addresses a relevant scientific question within the scope of the Journal "The Cryosphere". To address the research questions, an interesting combination of monitoring both the thermal and mechanical regime is applied. State-of-the-art thermal modelling is used to gain insights on the current thermal regime and the past regime that potentially influenced

the rockslide event. Unfortunately, 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.

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. 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.

The mechanical part of the paper is poorly addressed and landslide terms should be used for clarification. The authors derived three post-failure TLS scans but they did not discuss how they derived the estimated volume of 500,000 m^3. 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. 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. 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. 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.

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. 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).

In the next step, the authors used this information to model ground temperatures. They used the CryoGrid2 model which provides a resolution of 1 km^2 (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. 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.

To analyze the pre-failure rock temperature, the authors reconstruct past air temperatures (1958 to today) derived from meteo data from two meteo stations. This reconstruction should be described in more detail, thus, it is difficult to follow the approach and estimate model quality. Furthermore, permafrost is influenced by transient effects such as temperatures in the Pleistocene or Holocene (Noetzli and Gruber, 2009), however, such historicity effects are not discussed by the authors but should be addressed in a revision of the manuscript.

Despite these shortcomings, this manuscript could be suited for The Cryosphere after major revisions. The authors should significantly improve the structure and the mechanical part of the manuscript. Please use topic sentences to introduce paragraphs and landslide terms for mechanical descriptions. Furthermore, sharpen your objectives and discuss these critically. For detailed comments, see attached pdf.

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Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-223/tc-2016-223-RC1-supplement.pdf

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