

## ***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 #2**

Received and published: 15 February 2017

The study by Frauenfelder et al. describes a rockslide detached on a mountain side in northern Norway. The rock slide was triggered during a period where there were no special rain or snow melt events. Observations just after the event indicates ice lenses in the detachment zone. These observations together established the hypotheses that the rock slide triggering mechanism was related to thawing permafrost. Unlike in the Alps or other high-mountain areas, a relationship between landslides and permafrost thaw has not yet been observed or documented in Scandinavia, even if indicated in seral studies. This study provides evidence for a thermal influence of the triggering mechanism. After the event the authors did investigations at the site, a.o. instrument-

C1

ing the source area of the rockslide with ground surface loggers to investigate thermal regime and aspect dependencies. The authors also provide 1D and 3D modelling attempts to underpin their statements of permafrost thaw being a main factor. They conclude that a continuous warming during the last decades and extra high temperatures prior to the event degraded permafrost in the starting zone and thus may be a major trigger for the rock slide.

The manuscripts and the measurements described therein are well written and documented. The data series provided are of a wider interest as this seems to be the first site in Scandinavia relating a landslide event to permafrost thaw. However, the manuscript has also shortcomings which should be addressed prior to publication.

#### General comments

1. The manuscript is lengthy and reads like a report rather than a scientific paper. Methods and results are widely inter-mixed.
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.
3. Setting and geomechanical 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.
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

C2

even if details are explained in another publication. The same is valid for the reasoning of the parameter choice. 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? 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 sub-surface 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.

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). 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?

C3

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.

Minor comments:

Introduction: see comments above. I suggest to make a setting chapter in addition p.4, First three paragraphs: Delete, not necessary with a summary first, and details afterwards. Include in the detailed method description.

p. 5, l. 4-10: This is typical setting, move,

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.

p. 5, l. 25 ff: Give resolution/precision of loggers.

p. 6, l. 11-15: Delete whole paragraph, nothing new.

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?

p. 8, l. 1-16: I am not an expert here, but what is the point of this in relation to your hypothesis, e.g. triggering may have been related to permafrost thaw?

p. 8, l 25: What is the rationale behind to use always a 1a-mean?

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.

p. 9, 1. Paragr. : This is important for your reasoning, so you may show that somehow.

p. 10: Why did you choose 10 m depth for the CG2 model?

C4

p. 10, l. 19: This is method description, move.

p. 10, l. 30 ff. Much figure text here, move.

p. 12, l. 6: This is not acceptable; (CG2 shows increase of lower limit, and personal communication). Either include an analysis/figure or omit.

p. 12, l. 26 ff: You may be right, but the comparison “10s of meters” against “100s of meters” is not appropriate. I miss here an analysis of e.g. the potential SW radiation between north and south, they would give of course an indication of what differences one could expect between aspects. This is easy to perform in a GIS or so.

Fig 3: See comment above (better with topographic map)

Fig. 4: delete

Fig. 5: delete

Fig. 6: too many colors which differ little, find another way of presentation. Why 1 a mean?

Fig. 7: Many figures show the same, condense?

Fig. 12: What do you want to show here? Positive in 4 m depth just prior of detachment time? Is this the depth of sliding plane or so?

---

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-223, 2016.