

## ***Interactive comment on “Sub-seasonal thaw slump mass wasting is not consistently energy limited at the landscape scale” by Simon Zwieback et al.***

### **Anonymous Referee #2**

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The paper "Sub-seasonal thaw slump mass wasting is not consistently energy limited at the landscape scale" is using repeated single-pass InSAR data from the TanDEM-X mission to assess the analyze the sub-seasonal thaw slump activity in two two ice-rich study sites during summer 2015. The analyzed data indicates that mass wasting in the assessed areas is not always energy limited at the landscape scale. The level of detail to which the data sets are analyzed (both scientifically and technically) is impressive. The results achieved are manifold and highly valuable for this field of research. Overall, this is an impressive paper that definitely warrants publication in this journal. That being said, I have the following comments/concerns and suggestions whose consideration might further improve the value of this paper:

C1

Main (general) comments: 1. The paper is well written and both the description of applied methods as well as the discussions of achieved results are clear. However, the split of the material into "main paper" and "supplemental information" is not always appropriate and hinders the reading and comprehension of the material. While I understand the motivation between splitting the material into a more scientific discussion and a more technical analysis, some of the figures that are currently in the supplemental content might be better placed into the main paper to improve clarity. For instance, Figure S.19 provides a much better view of the associations between headwall elevation loss rates and slump characteristics than Figure 5b. Both figures should be grouped together and discussed together. Other figures that I would prefer in the main paper are S8, S10, and S18.

2. A major component that is currently missing in the paper is a discussion of the appropriateness of the used remote sensing data for the research at hand. I would contest that the characteristics of currently available remote sensing data such as TerraSAR-X significantly limit the information that can be extracted about thaw slump dynamics. From my point of view, the following limitations exist: 2.1 Temporal sampling: The sampling rate of 11 days seems borderline sparse given the high temporal dynamics of confounding processes such as precipitation and radiation inputs. Despite significant day-to-day variability, very little change remains when these variables are averaged over the 11-day period, making an assessment of associations difficult. 2.2 Spatial sampling: As acknowledged in various places in the paper, the 12m resolution of the InSAR-derived DEM data does not allow for a direct comparison between model outputs and surface lowering as sub-pixel variations give rise to an unknown and spatially varying scaling factor. Higher resolution would significantly improve the reliability of the remote sensing data as well as the conclusions that can be drawn based on these data. 2.3 Accuracy of surface lowering measurements: While the achieved measurement accuracy (60cm) is impressive, it is still a limiting factor especially for an analysis of processes in the scar zone, where height change rates are at the noise level.

C2

It would be great to see an additional sub-section in Section 5 “Discussion” that is dedicated solely to the appropriateness of the used remote sensing resources and to suggestions for future sensors that could provide more insight into this field of research.

3. I was a bit confused by the use of the stacked elevation loss rate data ( $r_s$ ) in the paper. While it is technically clear how  $r_s$  is calculated, it is not disclosed how many multi-temporal samples were used to calculate  $r_s$ . Furthermore, is it not entirely clear for which individual analyses  $r_s$  was actually employed. From my reading, I found that  $r_s$  finds very limited application in the paper and was used only once to analyze the spatial variability of the volume losses in the second half of summer. Instead, I am assuming that the elevation loss values in Figures 3 and 4 were not temporarily averaged, even though this is not clearly stated in the paper. I would appreciate a clearer statement about the use of the parameter  $r_s$  in this paper.

Minor (specific) comments: 1. Page 4, line 16: Please add the following reference to the sentence ending in “in volcanology and glaciology”: Kubanek J., Westerhaus, M., & Heck B. (2017). TanDEM-X time series analysis reveals lava flow volume and effusion rates of the 2012–2013 Tolbachik, Kamchatka fissure eruption. *Journal of Geophysical Research: Solid Earth*, 122, 7754–7774. <https://doi.org/10.1002/2017JB014309>

2. Page 18: Repeated identical statements seem to appear (compare lines 20 – 27 and lines 29 – line 2 on page 19). Please fix.

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