The paper " Sub-seasonal thaw slump mass wasting is not consistently energy limitedat the landscape scale" is using repeated single-pass InSAR data from the TanDEM-Xmission 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.

We are grateful to the referee for their helpful and constructive comments. We believe that thanks to the reviewer's input the clarity and quality of the revised manuscript have improved.

That being said, I have the following comments/concerns and suggestions whose consideration might further improve the value of this paper:

Main (general) comments:

1. The paper is well written and both the description ofapplied 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, FigureS.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.

We hope we have now struck a better balance between the main document and the supplement. We have moved Fig. S8 and S18 in adapted form into the main body of the manuscript. The reason for including these two figures is that they deal with the process-based results (focus of the paper), whereas Fig. S10 is more to do with technical issues. The reason for not including S19 is that we focus on the sub-seasonal mass wasting, whereas S19 deals with longer time scales. We believe Fig. 5b provides sufficient information for our purposes; in particular, the regression analysis accounts for the fact that the explanatory variables like aspect and area are correlated. In addition, we have tried to provide a better link between the main document and the supplementary material.

2. A major component that is currently missing in the paper is a discussion of the appropriatenessof the used remote sensing data for the research at hand. I would contest that the characteristics of currently available remote sensing data such as TerraSARX 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 variably, 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.

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.

We agree with the referee and have tried to paint a clearer picture. To this end, we have added an additional subsection in the discussion. There we briefly summarize the key limitations, while also highlighting the technique's advantages and contrasting them with complementary tools such as LiDAR. We also acknowledge the uncertainties and biases throughout the introduction (e.g. 'the comparatively large measurement noise'), the methods ('As these uncertainties are comparable to the signal magnitude, a detailed uncertainty analysis is required.') and the conclusions (first bullet point).

We also pick up on existing and future observing systems that hold promise, drawing attention e.g. to the potential of higher radar frequencies such as Ku band, as they can achieve higher spatial resolutions and accuracies.

3. I was a bit confused by the use of the stacked elevation loss rate data (r_s) in thepaper. 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 clearfor 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.

We have better highlighted the temporal extent of the elevation loss rate observations throughout the manuscript. The temporal extent is now explicitly mentioned in all captions, and we have also clarified the result sections (e.g. mentioning also the number of TanDEM-X acquisitions, and highlighting that Fig 3 refers to non-averaged rates computed from successive image pairs). Finally, we have slightly rewritten the description of the computation of r_s in the methods, highlighting its purpose (visualization, spatial comparison; Fig. 5) and contrasting it with the computation of the subseasonal rates between successive image pairs, which we now refer to as r throughout the manuscript.

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/10.1002/2017JB0143092.

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

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