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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 #1

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This study uses single-pass interferometry (an idea similar to DEM differencing, but in terms of interferometric phases) from bistatic TanDEM-X data to measure elevation changes at thaw slumps. Based on their results that show temporal evolutions in the summer of 2015 over two large areas in the Arctic (Tuktoyaktuk and Lena Delta), the authors quantitatively pointed out that the surface subsidence over headwalls didn't always track the changes in the input thermal energy, a conclusion that is stated in the title. This is an innovative and interesting work that I deem suitable for publishing in TC. However, I still have some comments for the authors to consider.

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Discussion paper

1. The authors pushed to the limits of the TanDEM-X data for these two landscape-scale studies on individual thaw slumps. Overall, I agree with the authors' strategies and conclusions. The authors may consider adding a summary of the following limits from the data in the discussion session.

The first limit is the spatial resolution: the multi-looked dh images have postings of 12 m, corresponding to an area limited of 1 ha, yet only 14% of the slumps are larger than 1 ha (page 8, line 28). Moreover, the analysis or interpretation is not based on individual resolution cells, but on spatially aggregated ones to active parts within each slump. I agree with this spatial-averaging approach. But I suppose this further reduces the number of slumps that can be investigated, simply because the active parts of the chosen slumps must contain several 12-m pixels. Generally speaking, would the overall results and conclusions about "not t consistently energy limited" be biased towards large slumps? Is it possible that small slumps are more likely to be energy-limited?

The second limit is the uncertainties and biases. The authors have carried out a detailed analysis on this in section 3.1.2. Spatial aggregation also helps.

The third is the limited temporal sampling. The authors produced 4 to 6 data points of temporal elevation changes for their sub-seasonal studies. This is probably the best data one can use for regional-scale mapping, thanks to the 11-day repeat of the TanDEM-X data. But there is a mismatch between the relatively poorly-sampled elevation changes and the daily meteorological changes (e.g., Fig 3). The cluster analysis indeed helps to boost the confidence level, reduce the contamination of local anomalies in individual time series, and reveal the three overall temporal patterns (Fig. 4).

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Discussion paper

2. The conclusion on “the widespread presence of an insulating veneer of debris or snow on the headwalls” is largely speculative. I understand the authors’ logic and agree that this is a possible reason for the inactive phase in June. But I wish the authors can provide more direct evidence for this assertion.

3. The authors may include a clear, representative slump to illustrate the temporal changes as revealed by the TanDEM-X data, like what is presented in the supplementary materials but for a typical case. I believe this could help the readers better understand the strengths and limitations of the data as well as the key results (time series and elevation loss rate map).

4. Can the authors present the same sets of results for their two study areas? Namely, maps of slump activities and dynamics (like Fig 2a,b) for Lena River Delta, and map of elevation loss rates (like Fig 6b) for Tuktoyakuk?

5. It’s not clear to me how “Time-average elevation loss rates were computed by stacking time series of individual Δh measurements” (section 3.1.3, page 8). My understanding is that the elevation loss rates are estimated at the middle of subsequent acquisitions (Fig 3). At first, I had to guess that the authors used several pairs that have the same mid epoch but different spans to average (like two pairs day 1-day 14 and day 12-day 34 have the same mid epoch on day 23). But this would produce sparser sampling that what is shown in Figure 3. Then I found this description in S.1.1 “The stacked elevation loss rate rs was computed from the time series of estimates assuming a constant rate.” Please clarify this stacking method. What is always helpful is to provide a list of images used and interferometric pairs generated and their spatial baselines (in supplementary materials).

Lastly. I felt that I reviewed two super-long papers: the main manuscript focuses on the key ideas and results centered around thaw slump geomorphology and dynamics as well as their meteorological drivers; and the supplementary material that describes the technical and mathematical details related to the measurements of elevation changes from single-pass interferometry, and clustering analysis (as well as numerous other detailed results). I understand why the authors opted this way of dividing the dense contents into two documents, esp. for TC readers. But I had to constantly go back and forth between these two documents, which greatly disrupted my reading. Practically, I found it is very difficult to include my review comments on the supplementary materials as there are not enough space and no line numbers to refer to. I provide a few comments below. I can give a more detailed review of the supplementary materials in the next round, provided that they are friendlier to reviewers.

Page S1 “The stacked elevation loss rate rs was computed from the time series of estimates ...” See my 5 comment above.
Page S2 “such a positive correlation was indeed observed (Fig. S3)”. But I have to say that the positive correlation looks weak to me.
Page S7, Table S1: the units for along-track baseline should be m. And what is ‘effective’ baseline?

Figures related:
Figure 1e: Lack of vertical scale as the reference in the photo. Readers can have a guess from the caption though.

Figure 4, first row plots of normalized rates: clarify how the normalization is done. Why the maxima of normalized rates are not 1?

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Figure 4, TDD plot: would the averaged TDD within each period be more consistent with the averaged elevation loss rates than the maximum TDD? I don't expect this would change the increasing trend in TDD though.

Figure 5a: why the elevation changes rates and their uncertainties are positively correlated?

Figure 6c. Naturally, readers expect to see close-up images for all the four boxes in 6b.

Minor comments:
Page 1, line 7: during summer *of 2015*
Page 1, line 14: the slump area *and headwall height*
Page 1, line 20: One of the motivations of this study is to advance geomorphic modeling/prediction of thermokarst. But the two papers cited (Lewkowicz, 1987; Günther et al., 2015) are both observational work, not modeling work. Please provide more situation references.
Page 2, line 5: add a comma before "which we here .."
Page 3, line 15: 'rate-limiting' is used interchangeably as 'energy-limiting'. To avoid possible confusion, change it to 'energy-limiting' or 'energy-limited'.
Page 3, line 16: the temporal signature *of volume loss*
Page 3, line 23: extra heat is used to heat and thaw the cold active layer as well.
Page 4, line 15: Shuttle Radar Topography Mission (first letters are capitalized)
Page 5, line 23: replace 'tundra lakes' with thaw lakes or thermokarst lakes
Page 6, line 29: products (plural form)
Page 6, line 30: what is the source of the input DEM? Also page 8, line 32: provide more information about the pre-disturbance DEM.
Page 7, line 12: explain what is isotropic seasonal subsidence and why it is expected to be similar at the spatial scales of your interest.

Page 8, line 3: how small? Can add the estimated magnitude as presented in the supplementary materials.
Page 9, line 3: ‘earlier generation’ implies multiple cycles, and more implicitly that the life cycle is about 10 years by comparing images from 2004 and 2016 . Somewhere earlier, best in the introduction, this can be mentioned.
Page 11, line 13: Fig. S8*c*, to be more specifically.
Page 11, line 29: “two peaks in mid-July and mid-August. But Fig 3d shows at least four peaks during this period. Please clarify what are the two peaks.
Page 15, line 12: “suggests a strong influence of downstream sediment dynamics..” please elaborate more on this.
Page 16, Figure 5b shows that elevation loss rates are correlated with the relief. Any comments?
Page 18, delete sentences starting from line 32 to the end of the paragraph. Same sentences appear earlier (starting from line 23). Copy/paste mistake.
Page 19, line 10: change ’effect increased’ to ‘increase’

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