The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2022-149-RC1, 2022

Comment on tc-2022-149

Anonymous Referee #1

Referee comment on "Allometric scaling of retrogressive thaw slumps" by Jurjen van der Sluijs et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-149-RC1, 2022

General comments:

This is a great paper demonstrating the value of combining field data and expertise with remote sensing applications. By not only mapping RTS on a planimetric scale but developing a methodological framework to address volumetric change as well, expands our understanding on RTS thaw dynamics and processes greatly.

From my understanding, this research was carried out thoroughly and conscientiously, which is reflected in this manuscript.

I do feel this manuscript is very packed and dense with the different applied methods and developed workflows. The nested concept is understandable but also slightly lowers the comprehensibility of the work and makes it difficult for the reader to follow every concept and analysis presented. A bold move would be to split the manuscript and publish part of the work separately.

Specific comments

As mentioned above, I feel the manuscript is very packed.

- One drastic suggestion would be to e.g. publish the MSI work separately. Here a few comments on this:

- MSI: is the first multi-temporal mapping approach based on DEM/LiDAR datasets; the applied consistency scheme described in the supplement material is of high value and worth being presented in the main text
- the errors, limitations and uncertainties that arise from various multi-temporal datasets are not addressed; as the MSI and the results obtained here are based on different airborne stereo-photogrammetry, LiDAR and satellite DEM, it would be fundamental to discuss limitations and uncertainties
- I understand however, if the publication of the MSI separately is not feasible for the authors but I do think that the MSI can be explored more and in greater detail.

2. A different suggestion is to include a flowchart in the paper that highlights the different methods applied and datasets used. This would increase understanding and help follow the dense workflow applied in this paper.

Additional comments

- Figure 7/MSI groupings

- concept of grouping 0%, 10%, and 20-90% activity is not quite clear and straight forward
- I think to better understand and emphasise that most of the identified and digitized RTS are stabilised and/or old scars and only small areas within the big scars show active RTS slumping activity would be clearer in a different plot form and not a cumulative plot

2. Clearer differentiation between the terms used in this paper on RTS activity, RTS/slump area, area-affected, ...

- e.g. in abstract ll 23-25: 'increase in active RTS, increase in total active surface area, total area of RTS, active thaw slumping';
- it is sometimes hard to track what is referred to precisely and hence difficult to follow the results and their implications on the RTS process, form and so on

3. Consider highlighting the difference between stable and active slumps

- one of the key results and outcomes for me, is the difference between active and stable slumps and their close relationship
- the current trend is to map active RTS (at large scale) and stable RTS are often neglected due to their different (remote sensing) signal and signature. But this study shows their importance and I feel this can be emphasised more. This study provides the data for it so has every reason to make a point in promoting the importance of stable RTS and the necessity to develop methods that map these as well

Technical comments

- double check the references from Jones, M. K. W. et al. As this is a double last name, I think it should be Ward Jones, M. K. et al.
- Supplement Fig. S4: a., b., and c.: Order of LIN and NN boxplot s not the same for these 3 plots; preferably change boxplots to the same prder which makes it easier for the reader to interpret the results