The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-137-RC1, 2021

Comment on tc-2021-137

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Referee comment on "Assessing volumetric change distributions and scaling relations of retrogressive thaw slumps across the Arctic" by Philipp Bernhard et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-137-RC1, 2021

The work by Bernhard and colleagues provides a tantalizing demonstration of reliable repeat monitoring of thermokarst mass-wasting from a space-based platform. This work potentially fills a number of important gaps in broadscale monitoring of topographic change in ice-rich terrain, and could provide important support for ongoing assessment of the extent and acceleration of thermokarst processes in the Arctic. The paper applies some well-known allometric analysis, based on established principles from studies of mass-wasting in temperate environments.

The analysis focuses first on thaw slump volume, the average volume change among the three change models potentially available (ln 122-123). It is important to note in this paper that thaw slumps are chronic, multi-year (often multi-decade) features that produce variable eroded volume over time, and the erosional intensity and morphological complexity tends to change with the age of the feature. This is a critical point of distinction from landslide studies that commonly examine the scaling of the total erosion from a scar zone. Both approaches have important outcomes: from an annual yield perspective, the area-volume scaling relationships presented here agree well with established power law parameters, and the resultant regression will likely be helpful for estimating annual yield from mapped RTS polygon areas. From the perspective of the full scar erosion depth, measures of the time-integrated changes in morphology can yield a regression that might tell us more about the longer-term trajectory of the landscape. However, generating a 'pre-disturbance' surface can be time consuming, and there is the prospect of erroneous reconstruction, particularly in the case of larger slumps in complex topography. I'm not suggesting the latter analysis be incorporated, but it is important to highlight this distinction.

The authors should perhaps clarify that the 'area' term denotes primary scar zones (only) - not including spoil zone or other reworking, for clearer comparison with other datasets. There is invariably some detritus that fills the primary erosion site, particularly in older and larger RTS features on more subdued slopes, so the precise volume of most recent erosion is not always accessible.

The term 'volumetric change rate density' (ln 127) is clarified as 'volumetric change per unit area', but the statement goes on to say this is calculated 'by dividing the study region size by the total volumetric change rates', which seems to be rather the reciprocal – and a 'change rate' (e.g. ln 275) is different from volumetric change. I'm perhaps misunderstanding your intent here, but some clarification of this specific yield term is needed.

While the TanDEM-X elevation dataset has broad statistical characterization of the vertical accuracy (ln 101-102), the problem of volumetric change in landslides, gullies and other mass-wasting zones present a more specific problem: how well is the the scar zone volume characterized by the grid of elevation values interpolated in and around it? Given the focus on allometric relationships, it is important to assess the propagation of various errors, some that are likely to vary with scale. As the scale of erosion features approaches the pixel resolution, the estimated volume will be increasingly approximative. Admittedly the problem of error characterization and propagation in landslide inventories has not advanced very far generally, but given that this work could be a stepping stone to even further extrapolations of sediment and carbon export, it would be quite helpful to establish a list of factors that contribute to error and some estimation of the overall precision that can be achieved with this methodology. Some calibration with finer-scale elevation datasets could help with this problem, as well.

The results show some noise in the scaling relationship, which is certainly not unexpected given the diversity of drivers and physiographic factors that govern thaw slump development. The capacity to explain this variability based on remotely-sensed landscape factors is limited, but as stated, with further refinement of methods and proxy measures of ground conditions (ice content, soil thickness, base-level controls), there is great potential to advance our understanding of the transformations of the landscape that are underway. It would be good to see some further speculation on the reasons for variation in the scaling exponents in different regions - what do they signify? Section 5.2 is conspicuously brief on this. In the Banks Island dataset, for instance, smaller erosion features tend to be shallow surficial failures, resulting in proportionately smaller volumes in that part of the size spectrum, and thus a steeper regression curve. In the Peel Plateau setting, there is very little confining topography to arrest headwall development, and thus the relatively larger features can get *very* large, again contributing to a steeper relation. Other sites may see less topographic variation across scales, which might contribute to a shallower slope on the regression curve. Glacial legacy plays a very important role in moderating this relation, as well. Some further, fairly general, geomorphic terrain interpretation could yield insights into how conditions change with scale.

The prospects for broad scale repeat monitoring thaw slump evolution is appealing - the work presented here shows that with a good supporting dataset of landscape information there a good possibility of achieving this, and advancing models for periglacial landscape evolution in the Anthropocene. The paper is well structured, and the charts and graphics are nicely rendered, but there are quite a few typos and grammatical issues in the text; this should be carefully reviewed before resubmission. There is some confusion regarding the numbering of figures in Section 4 and 4.1 (Figs 3-5) that require some attention. A few points are listed below. With the resolution of these minor points and a few points addressing error/uncertainty and some interpretation of the regression slopes, I recommend advancing this paper to publication.

l. 18: suggest 'underlain' by permafrost
l. 29: nutrition => nutrients
l. 35: insides => insights
l.39: It's not clear to me that a frequency distribution is a scaling law.
l.40: 'disturbed area' => more explicitly, the erosion site (not deposition)
l.48 were => where
l.49 vertical - I think you mean horizontal, here.
l.50 were => where
l.56 only became available in the last few years
l.59 ..and have observed the global land mass two to three times, now.
l.72 ..modelling and will further improve..
l.83 Due to the large extent of some areas
l.95 The incident angles (?)
l.97 We only studied winter acquisitions due to the low..
l.144, 147 - beware affected vs effected - meaning is not clear, here.
l.150 volume
l.167 ..is to relate the area..
l.177 violine => violine. Use either comma or thin space between thousands in presentation of numbers.
l.184 exponential
l.185 potentially
l.199 By contrast, the PDF based on..
l.204 exponential decay coefficients
l.248 ..this universal scaling also applies to permafrost landscapes..
l.258 ..order of magnitude higher growth rates..
l.259 relict ice
l.313 ..data availability only allowed us to compute elevation changes..
l. 325 insides => insights