The Cryosphere Discussions

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Comment on tc-2021-251

Anonymous Referee #2

Referee comment on "Towards accurate quantification of ice content in permafrost of the Central Andes – Part 2: An upscaling strategy of geophysical measurements to the catchment scale at two study sites" by Tamara Mathys et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-251-RC2, 2021

Review: Towards accurate quantification of ice content in permafrost of the Central Andes - Part II: an upscaling strategy of geophysical measurements to the catchment scale at two study sites

This paper aims at developing a methodology to scale local, geophysics-derived estimates of ground ice content to a subcatchment scale. The study is part of a project that uses geophysical data to estimate ground-ice in an area of the Andes. This work is currently under review as the first part of this study.

Similar upscaling attempts have been shown to be successful, but were mostly applied to high-latitude environments, whereas this study is considering a high mountain array. Hence, the authors make use of geomorphological data and field observations.

As the authors demonstrate, estimating ground ice content of high altitude, headwater environments is important to assess groundwater resources further downstream, yet a quantitative estimation of this parameter is difficult. Here, the authors build on geophysical data, presented as Part I of this study, to estimate the ground ice content throughout a wider area. By using various input parameters to classify their sites, the authors are able to provide quantitative estimates of ground ice content. While the approach is very interesting, the classification, which forms an integral part of the study, seems poorly constrained, and mostly qualitative. The authors repeat much detail of the

geophysical characterization (which is fair, given that this is the most important data set), there is very little detail on the actual classification. No maps are shown that show the other input parameters, such as slope angle, aspect, geomorphology, or the estimated soil parameters, including locations of soil probing, making it impossible to follow or understand how class parameters vary and how they were decided on. Similarly, it is not clear how the parameters that are critical for the ice content calculations (thickness, area, ice content) were upscaled, or determined, particularly for areas without geophysical data. It would be great to also see those as maps.

These limitations of the current manuscript makes it difficult to understand what the benefit of the approach is. Comparing Figures 7 and 8, the shown difference between the geophysical based estimate and the empirical approach, could well fall within the uncertainties introduced by using different classifications. Given the strong reliance on field observations, it is also questionable whether similar approaches could be used more widely to estimate ground ice contents.

Next to those rather major comments, please find below some more minor comments:

Line 14-15: I don't think that an abstract should contain references, and I wonder whether the detail on the geophysics is actually needed here, as this paper focuses on the upscaling, not the geophysics.

Line 89: In a previous sentence you mention that line locations were planned based on "safety within the mines". Does this mean that the chosen sites are active mining sites, and hence not in their natural state? That would make upscaling to natural systems impossible. According to Fig. 1, sub area 1 seems to be located within active mining, whereas others seem outside. I think some more detail is needed here on what the impact of mining on the chosen sites is to justify that mining has no impact on the results.

Line 126: “comparable near-surface substrate [...]” This is a critical assumption for the upscaling, yet the authors do not provide information on the geology and the variability of subsurface properties.

Line 154: Potential incoming solar radiation: How and based on what did you calculate that?

Line 155: Equation for estimating permafrost occurrence: It would be good to show a figure that shows the data and model fit, and also details the parameters of the model.

Line 172-173: what do you mean by "high bedrock slopes"?

Line 186-187: On what data is this threshold based on?

Line 197-199: Although you describe the input parameters, there is no clear methodology described here on how you define the classes. This needs more detail and justification.

Line 202: Given that soil properties will also impact on the ground temperature

distributions, shouldn't the soil stratigraphy be an input to the classification?

Line 295 - 298: Given that the scope of your work is upscaling, why do you distinguish areas where geophysical data has been acquired and areas where this has not been done?

Line 320 - 321: It would be great to see the estimated ground ice content as a map.

Discussion: The discussion on the geophysical results should be mentioned, but not in that much detail, as it should be part of Part 1 of this study. The uncertainty in the classification is of greater importance.

Line 390: I don't think that this is necessarily an image classification problem. But you could use machine learning to exploit relationships between surface and subsurface parameters.

Figure 4: You prescribe ice-poor bedrock of D02 with an ice content of 4%, and bedrock of A15, which is overlain by ice-rich material with an ice content of 0%. How did you define that? Similar for Fig. 5, where the 4PM model indicates higher ice contents. Does the bedrock geology play a role in your definition of the ice content. If so, shouldn't this be an input to the classification?