

## ***Interactive comment on “Scaling-up Permafrost Thermal Measurements in Western Alaska using an Ecotype Approach” by W. L. Cable et al.***

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This is a well-conceived and well-executed study, quite worthy of publication in The Cryosphere.

Permafrost, as a critical ecosystem component and factor in global climate impacts/feedbacks, must be better quantified spatially to enable improved estimates for: a) modes of permafrost degradation, b) impacts to landscapes and ecosystems, and c) carbon-based cumulative impacts to global climate. The authors rightly use well-developed ecotypes for this region as the basis for landscape-scale estimates of upper permafrost temperature and thermal properties based on rigorous field data. Ecotype currently comprises the best categorical scheme producible across remote landscapes which characterizes the most important surface and near-surface conditions influenc-

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ing upper-permafrost properties and dynamics. In this case, study sites are primarily lowland locations, so ecotype alone should be sufficient to achieve the study goals presented here.

This manuscript represents an important early step toward broader development of both datasets and refined approaches for synoptic estimation of upper permafrost temperatures, and ultimately other key properties like ground ice and cryostructure distribution, at regional to global scales.

I have included a few suggestions for minor revisions/edits below. With one exception, none of them are essential to enabling publication of this work, but they're pretty easy changes if the authors agree with them, and may serve as improvements to the work.

The one exception is a point I strongly encourage the authors to reconsider (discussed under "Page13, Lines 7-8", and under "Page 12, Lines 19-24", below). The authors might possibly be well-justified in their recommendation to dispense with grid-based approaches for future work. However, if so, that justification isn't yet clear, and seems contrary to other successful approaches in the literature. If the authors prefer to retain this recommendation, the justification should really be better spelled out. Otherwise, I would have to respectfully disagree with their assertion in hopes that they remove it.

Page 1, Line 20: In the opening sentence of the introduction, the authors might include N<sub>2</sub>O along with CO<sub>2</sub> and CH<sub>4</sub>. Nobody really talks much about it yet, since it's so poorly quantified in this context right now, but acknowledging the potential role of N<sub>2</sub>O might be a forward thinking inclusion here.

Page 11, Lines 8-11: This sounds like a bit of a strong statement given that there are really only two years of data. The authors might consider pulling back the language a bit to (very justifiably) claim they've captured some real inter-annual variability, without stating that it really brackets the long term variability.

Page 12, Lines 1-6: This is a really interesting point, with probable implications for

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changing permafrost conditions following ecological shifts. If there happen to be any data, or other studies, addressing size/density of tussocks and how these impact thermal regime, it would be really interesting to mention them here in the discussion.

Page 12, Lines 19-24: The authors mention a few examples of effects from landscape position and aspect, without delving into it very deeply. Down the road, the best results from this sort of approach will likely include physiographic and geomorphologic variables along with ecotype in the analyses. I fully understand why the authors did not include them in this study, and I agree with their decision; including those variables here would have necessitated a number of field sites which would have been extremely prohibitive financially and logistically. Still, I think the end-game for this type of approach is to be able to cobble together enough congruent field data from enough projects and studies to enable such inclusion, and ultimately yield more precise results across landscapes and regions. It might do the readership a service to mention that explicitly here.

Page 13, Lines 7-8: While I agree that ecotype should represent the single most important variable in this sort of approach, and that ecotype alone is fully adequate in the context of estimates generated within this study, I don't recommend dispensing with a grid-based approach entirely for future work. There are a number of analytical techniques which can combine complementary categorical and continuous data to substantially improve results, and capture within-class variability very nicely through grid analyses. This can provide real advantages for testing ideas at multiple scales over using categorical units alone. Again, there's no reason for using a grid-based approach within this study, but I think if the authors want to stick with this recommendation for future work, it should probably include more justification as to why. There may be a good reason for this which I haven't considered, but if so, it would be important to describe it, given that grid-based approaches have provided a number of valuable contributions within other studies.

Figure 10: Extremely minor point - the color assignments for litter and for cluster group

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3 are both orange. Given that there are a few colors not yet used in this figure, the authors might consider substituting one (purple, magenta or something). Would make it more quickly understood by the reader.

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