Response to Review #1 by A. W. Balser on, “Scaling-up Permafrost Thermal Measurements in Western Alaska using an Ecotype Approach”

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We would like to thank A. W. Balser for his helpful review of our manuscript. We agree with his comments and have revised the manuscript accordingly. Below, A. W. Balser’s comments are given in italics and our response as regular text in blue.

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 influencing 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 N2O along with CO2 and CH4. Nobody really talks much about it yet, since it's so poorly quantified in this context right now, but acknowledging the potential role of N2O might be a forward thinking inclusion here.

We agree and have included N_2O in the text.

"Interest in permafrost as a potential source of the greenhouse gasses CO_2, CH_4, and N_2O has increased…"

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.

The language in this statement has been pulled back a little however, we feel the support for this statement is quite conclusive given the permafrost temperature at depth represents a long-term average.

"We think these years likely bracket the longer-term mean ground temperature (and deeper permafrost temperature) because in 2012–2013 the slope of MAGT with depth was negative (Figure 8), indicating colder than average MAGT and mean annual air temperature (MAAT)."

Page 12, Lines 1-6: This is a really interesting point, with probable implications for C2 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.

We also find this to be a very interesting point and have observed this many times while visiting field sites early in the winter. Unfortunately though, we are unaware of any data or studies addressing the size/density of tussocks and how this would impact the thermal regime.

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.

We fully agree that in some areas ecotypes might not be relevant or completely explain the variation in permafrost thermal regime. We didn't feel that this (page 12, lines 19-24) was the right place to address this so a sentence has been added to the conclusion (page 13, lines 9-11) that addresses this.

"However, in some areas (e.g. mountainous terrain or barren landscapes), variables other than ecotypes (e.g. slope, aspect, or microtopography) may become more important, in which case they could be used in addition to, or instead of ecotypes."

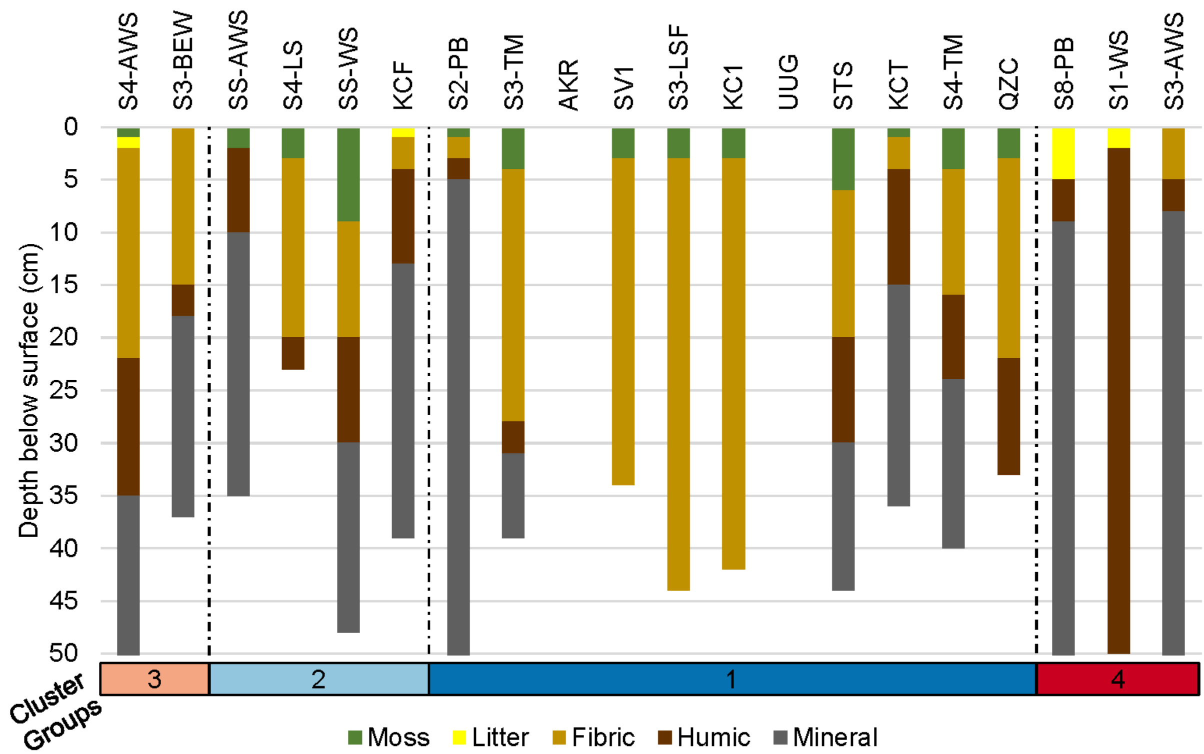
Page 13, Lines 7-8: While I agree that ecotype should represent the single most important variable in this sort of approach, and that ecotype alone is fully adequate in the context of estimates generated within this study, I don't recommend dispensing with a grid-based approach entirely for future work. 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.

We have removed the implication that our "ecotype approach" should be used instead of a grid-based approach and suggested instead that the ecotype approach offers an improvement in spatial resolution without increased computational demand.

"Accordingly, we recommend that future permafrost modeling efforts consider using an ecotype approach as it offers increased spatial resolution without increased computational demand (i.e. a model only needs to be run once for each ecotype)."

Figure 10: Extremely minor point - the color assignments for litter and for cluster group C3 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.

Thank you for the suggestion, the color of litter in Figure 10 (below) has been changed so it is easier to distinguish from cluster group 3.


S4-AWS
S3-BEW
SS-AWS
S4-LS
SS-WS
KCF
S2-PB
S3-TM
AKR
SV1
S3-LSF
KC1
UUG
STS
KCT
S4-TM
QZC
S8-PB
S1-WS
S3-AWS
Depth below surface (cm)
0
5
10
15
20
25
30
35
40
45
50
Cluster Groups
3
2
1
4
Moss
Litter
Fibric
Humic
Mineral