

Interactive comment on “Characterizing permafrost soil active layer dynamics and sensitivity to landscape spatial heterogeneity in Alaska” by Yonghong Yi et al.

Anonymous Referee #2

Received and published: 2 November 2017

Title: Characterizing permafrost soil active layer dynamics and sensitivity to landscape spatial heterogeneity in Alaska Author(s): Yonghong Yi et al. MS No.: tc-2017-87

General Comments This is a well-written paper about the development of a model for Alaskan active layer thickness mapping based on satellite and ground data. The model was developed in response to a need to better understand spatial complexity (vertical and horizontal) of active layer thickness across varying biomes of Alaska. Complexities in the vertical distribution of SOC were most problematic/uncertain. Complexity and uncertainty remain, but improvements have been made in our understanding of how we might map active layer thickness across larger regions of the Arctic. I have no

Printer-friendly version

Discussion paper



major concerns about this paper, but would suggest addressing some of the comments below.

Specific Comments 1. P. 2 In the paragraph beginning with line 19 you state that information is lacking about the subsurface spatial variability of SOC, yet don't acknowledge others that have attempted to tackle aspects of this (although not Alaska). Suggest looking at Burnham and Sletten 2010 (doi:10.1029/2009GB003660) and similar refs within.

2. P. 2. Could you add a sentence between lines 27 and 29 stating the goal of the project?

3. I don't quite understand lines p.5 28-29 "The OLT was used to define the depth of the model soil layers with 100% SOC fraction." Does this mean you are assuming that 100% of the carbon extends only to the base of the OLT? Is this because you know you have Histels at these three tower sites? Was this for simplification of the parameterization process?

4. P. 7 line 2. While it helps simplify the analysis to assume an exponential vertical decay of SOC, this is not always the case for highly cryoturbated soils. In many cases there is more SOC at depth than at the surface (especially in patterned ground covered surfaces). I don't suggest changing this parameter, but do acknowledge that it can be highly variable for your higher latitude regions.

5. P. 8 paragraph beginning with line 18. Has bulk density been taken into account somewhere here when estimating SOC?

6. P. 14. It's unclear that getting a better estimate of subsurface SOC variability is ever really possible, but you could look to other arctic SOC studies showing that the most underestimated SOC stocks in the active layer are those found in the least vegetated arctic biomes.

7. Figure 8. A bit of clarification could be used in the caption. I'm unclear how uncer-

[Printer-friendly version](#)[Discussion paper](#)

tainty in 8a is represented by centimeters.

8. This is not absolutely necessary, but it would be good to add a note in the conclusion about the possibility (or not) of expanding this model to broader areas of the Arctic.

Technical Corrections 1. Very surprised to see that Arctic is misspelled in the first line of the abstract 2. P. 6 line 16, I don't believe that "in prep" submissions are considered acceptable references. 3. P. 7 line 17 I don't believe that "in review" submissions are acceptable either. 4. P. 13 line 13. Indicate instead of indicated 5. Minor point. Scale bar in Figure 3b appears taller from 70-100% than the rest of the bar. 6. Figure 5. I recommend not using blue and purple colors as they are not distinct enough from one another.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-87>, 2017.

TCD

Interactive
comment

Printer-friendly version

Discussion paper

