

Comments on “**Characterizing permafrost soil active layer dynamics and sensitivity to landscape spatial heterogeneity in Alaska**” by Yi et al., published in *The Cryosphere Discussion*, doi: 10.5194/tc-2017-87, 2017.

General comments

This paper reports a spatially integrated modelling and analysis framework with strong satellite data support, including surface temperature, snow coverage, depth, and density, and soil moisture. The authors also used airborne radar data for their analysis. This approach represents the new development and future directions of satellite based permafrost mapping and monitoring. With more satellite and airborne data available in the coming years with the support of NASA’s ABoVE program, such an approach will greatly improve our capacity to understand and monitor the northern ecosystems and permafrost. The data description, sensitivity tests, and results analysis are clear and detailed, and the sources of uncertainties and limitations have been indicated in the discussion section. Such a new development and the preliminary results are worthy to be published.

Major comments

Actually, I do not have major concerns about this paper since it represents a new approach development with great potentials, rather than the final results and conclusions. With more and more data available from the ABoVE, such an approach (this framework or similar ones) will be improved gradually. With this, I suggest that the tone of the paper and language may put more attention on this forward looking and future development perspective. Before describing the model and input data in section 2.1, you may add a paragraph about the ideas and design of the overall framework.

Page 3, Line 24: “The soil process model was run at 1-km resolution and 8-day time step”. The original model is run using a one-day time step implicitly. A time step of 8-day is too long. Clarify whether that is the case or you interpolated the 8-day land surface temperature data.

Page 6-7: SOC distribution in soil profile. Most northern lands have a pure organic layer (including lichen, mosses, peat or organic materials from leaf and roots) above the mixture of

mineral and organic matter, and this pure organic layer is very important for active layer thickness. Clarify whether your treatment of SOC distribution with depth reflected this phenomenon. The fraction of soil organic matter (SOM) content in a layer depends on the bulk density of the soil layer and the amount of SOM in this layer. So it would be useful to explain how do you estimate the bulk density of the soil layers and the general vertical distribution patterns of the fraction of SOM.

Page 14, Line 4, “after introducing a statistical distribution of the regional SOC spatial pattern”. Put more explanation in the method section about this statistical distribution.

Figure 3. The model over estimated ALT at most CALM sites when permafrost probability is lower. It would be interesting if you can put some explanation about that systematic bias (due to model, input data, or spatial resolutions etc.?).

Figure 5 is not very clear, probably is not necessary.

Figures S2b and S2c: for easier comparison, is it better to use percentage of pixels in a latitude zone rather than number of pixels in the zone?

Minor comments

At some places, you used the phrases “soil active layer”, “permafrost active layer”. You may just say “active layer”.

At many places, you used the phrase “model ALT”, it may be replaced by “modeled ALT”. You also used “model ALT simulation” at many places. You may just use “modeled ALT”.

Page 5, line 11: delete “first”.

Page 6, line 5: “at October 2015”, revised to “in October 2015”.