

Comments on “Site-level model intercomparison of high latitude and high altitude soil thermal dynamics in tundra and barren landscapes” by Ekici et al. published in The Cryosphere Discussion

### General comments

This study compared the modeled thermal dynamics of six land models at four high latitude or altitude sites. Such a site-level comparison is useful to assess our skills in modeling cold region land surface processes. Comparing to the recent spatial results comparison by Koven et al. (Analysis of permafrost thermal dynamics and response to climate change in the CMIP5 earth system models, *J. Climate*, 26, 1877-1900, 2013), such a site-level analysis can directly related to physical processes and have a potential of identifying the specific processes which need to be improved in the land surface models. Such work is meaningful and is within the scope of the Journal. Following are some suggestions for possible improvement.

### Major comments

1. Figures 7-10 show the significant differences among models in simulated ground temperature dynamics. It would be very useful if the authors can provide a similar time-depth panel for the observed ground temperature dynamics for each site.
2. Although Figures 1 and 2 are interesting, it would be more direct to show time series of observed and modeled topsoil temperatures, especially multi-layer soil temperature observations are not available to provide time-depth dynamics as I suggested above.
3. Figure 5 shows the modeled difference of ground temperature in snow cover period and snow-free period. With this and other figures (1, 2, 4), Figure 6 seems not necessary and it is not so clear.
4. Figure 11. The temperature range of the x- axis is so wide (80 °C) that it is hard to see the differences between the model results and the observed. It may be more meaningful to plot the profile of annual mean temperature for comparison purposes.
5. Figure 12, panel d. This panel is not very clear and probably not very meaningful. Anyway, there is no close relationship between active-layer thickness and annual mean air temperature.
6. All the five models under-estimate snow depth for the Samoylov site. Are there any common mechanisms that all these models missed for such a high arctic area, or some other reasons, such as input or observations (snow drifting)?
7. The result analysis is well structure, but some messages and sentences are not very clear or not focused. Some improvement is needed (see some examples in the following minor points).

### Minor points

1. P. 4961, Line 8: “to quantify” or “to identify”?
2. P. 4961, Line 12 “Snow insulation is of major importance for estimating topsoil conditions and must be combined with accurate subsoil temperature dynamics to correctly estimate active layer thicknesses”. You demonstrated that snow is important for topsoil temperature,

especially winter soil temperature. However, the effects of snow on subsoil temperature and active-layer thickness is about how to simulate soil temperature process (heat conduction) rather than “combining snow”. Some rewording may be needed.

3. P. 4962, Lines 23-24, “we assume that lateral processes do not influence the observations”. Probably revised as “the observation sites are generally large and uniform, therefore we assume that lateral processes can be ignored and the ground thermal dynamics are mainly controlled by vertical processes”.
4. P. 4972, Lines 18-21. These sentences seem not necessary. You may say something about the importance of topsoil temperature for model validation/comparison.
5. An important message of the paper is about the effects of snow. You may focus on this at the first several paragraphs in the section 3.1. Delete some distractive sentences such as “However, summer comparison also shows considerable deviations from the observations as well as among models” (P. 4973, Line 2-3). “However, the combined effects of snow cover and vegetation insulation together with soil organic content is needed to accurately estimate soil temperatures (Schaefer et al., 2009)” (P. 4973, Line 25-27).
6. P.4977, Line 17-18, “the snow free season shows large among-model discrepancies in topsoil temperature (Fig. 6b)”. Not clear what do you mean. It is not larger than snow cover periods.
7. P.4978, Line 4-12. Such a general comment seems not necessary. It is better to be more specific based on the results.
8. P.4981, Line 14-15, Line 21-22. The effect of snow on active-layer thickness is usually not significant, especially when permafrost is cold and stable, such as at Samoylov. The over estimation of ALT at this site is most probably due to underestimate the effects of moss and peat rather than over-estimation of snow.
9. P.4982, Line 9-10, “By doing so, they attributed most of the ALT biases to insulation effects, which is mostly from snow processes in these regions”. Probably not correct. Most simple or analytical relations between topsoil temperature and ALT do not consider snow effects. They usually based on thawing-period air or topsoil temperature.
10. P. 4983, Line 1-2. The sentence, especially the word “amplified”, is not very clear.
11. P.4983, Line 7-8, “but models need more detailed representation of vegetation cover thickness”, it is better to say “therefore models need more detailed representation of moss and top organic layers”.
12. P.4968, Line18. Delete “it”.
13. P.4969, Line 13, “for degradation of this warm permafrost site”. Revised to “for degradation of the warm permafrost at this site” ?
14. P.4969, Lines18-21. The sentence is too long and not clear.
15. P.4970, Line 23. Move the bracket before the author Maturilli.
16. P.4985, Line 6. “depts” should be “depths”.