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Interactive comment on "Permafrost Variability over the Northern Hemisphere Based on the MERRA-2 Reanalysis" *by* Jing Tao et al.

Anonymous Referee #2

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Overall comments:

This paper used in-situ data and a remote sensing based ALT (active layer thickness) data to evaluate a model-based ALT dataset. Overall, I think it is a useful study. The analysis was done in a comprehensive way, and the results were acceptable. However, the analysis regarding the model uncertainty is somewhat general – considering we already have a good knowledge of the ability of global land models in NH permafrost simulation. I think, the study could benefit from more in-depth discussions on this aspect. The details were provided below.

Major comments:

1. Page 4 Paragraph 3: I have questions regarding how the ALT was calculated. The

paper indicates here it was calculated based on the simulated ice content. Does the model consider unfrozen water in frozen soils? If it does, please provide information on how the model calculates unfrozen water content. If not, this definition will be same as using a 0°C temperature threshold for thawed-to-frozen depth calculation. This information is especially important for the deep soils due to year-round low temperatures and coarse vertical resolution of the model at deeper depths.

The above declaration seems contradictory to "The use of the 0°C degree threshold in CLSM for determining the thawed or frozen state of the soil may explain the model's underestimation of ALT." (Page 9, Paragraph 1). So I am confused what methods were actually used to determine the thawing depth/ALT. Please clarify.

2. Page 5 Paragraph 3: The spin-up scheme is questionable, though the authors themselves acknowledged this. Why do the authors using the meteorology for the entire 36-year period for spin-up? If the design is to reduce the uncertainty introduced by a single-year surface meteorology, spin-up using the first few years during the period will be more acceptable.

3. I have questions regarding the vegetation effects on permafrost simulation in Northern Alaska (Page 11, Paragraph 2). Those 4 northern flights were dominated by "dwarf trees" as indicated by Fig. 2b (really?). Moreover, the changes in simulated maximum snow depth due to vegetation in those flights were much smaller comparing with the experiment homogenizing the forcing data (Fig. 6c). So I would expect the impact due to snow changes for the homogenizing vegetation experiment would be smaller comparing with the experiment homogenizing surface forcing, while this is not the case shown in Fig. 6a-b. Can the authors explain why?

On the other hand, the Alaska North slope is dominated by tundra, while the vegetation map in CLSM indicates mostly dwarf trees or shrubs (Fig. 2b). Would this introduce uncertainties to the analysis on the contribution of different factors (i.e. forcing data, vegetation and soil) in Fig. 6?

Also, from Fig. 6, it does not seem to me that homogenizing soil parameters has much bigger impact on simulated ALT and surface soil temperature than homogenizing vegetation (at least for the northern flights), as the authors indicated in Fig. 7. Maybe I miss something here?

4. Most of the value for AirMOSS radar retrievals, I think, is in its ability to characterize land surface heterogeneity. Simply averaging AirMOSS data to a much coarse-resolution (i.e. 9km in this study) to compare with the land model simulations is not very insightful in terms of exploring the value of this dataset. I agree with the authors that the current AirMOSS retrievals seem having large uncertainties; most notably, its ALT retrievals were in a very narrower range. However, the inconsistency of the ALT spatial pattern at some of AirMOSS flights may come from the model itself. For example, at the DHO flightâĂŤthis is the flight with most in-situ sites available, the model ALT generally increases from the north to the south, while the in-situ data show large variability, and do not show a clear increasing trend from the north to the south (Fig. 5a). There are also a number of studies pointing out that ALT is extremely variable at local scale. Therefore, analysis using a dataset like AirMOSS in this aspect would be more valuable.

5. It would be more interesting if the authors could provide more insightful analysis regarding the model ALT uncertainties or the correlation analysis, including:

a) Why does the model show much stronger correlation with maximum SWE in portions of NH permafrost region than with air temperature? b) It would be helpful if the authors could give more explanations regarding why the model fails in western Siberia? Those areas also include continuous permafrost, so I do not think it is too challenging for global models to capture the permafrost distribution there. c) The ALT trends shown in Fig. 13a seem not very consistent with the trends of temperature indices shown in this figure. Have the authors explore the changes in snow cover duration? A longer snow free season generally leads to warmer soil temperature and thus deep ALT esp. in the southern area. d) Page 18, Paragraph 3: I do not quite agree with the authors' explanation why the model fails in the Mongolian sites. For those sites, the model simulated ALT is generally less than 1.4 m, therefore, the coarse resolution at deeper soils in the model set-up (i.e. layer 5: 1.4-3m) should not be a major contributing factor there. Much drier conditions, sparse vegetation, and perhaps uncertainties in soil texture data (eps. in deep soils), I think, are more likely contributing more to the model uncertainties.

Minor comments:

1. Fig. 4: The text in Section 2.3 indicates most of the comparisons against AirMOSS data will be at the 4 flights in the Alaska north slope. So I wonder why COC flight was included in this figure esp. considering the AirMOSS retrievals were not included?

2. Fig. 4 a-b: were the sites arranged according to the latitudinal changes?

3. Page 9, Line 21: Does MERRA-2 not provide air temperature at 2-m surface height?

4. Section 3.2 and Figure 6: Part of the IVO flight lies in the Brooks mountain range with very low SOC content (Fig. 2c). It may be not a good representative of the average conditions in this area, at least considering SOC variability.

5. Section 3.2: Would it be easier to follow if the description regarding the idealized experiments was included in the Methods section?

6. Fig. 7 is not very informative. I suggest summarizing the results in a table.

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