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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 #1

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This paper provides an evaluation of active layer thickness and permafrost extent as simulated by the NASA Catchmant Land Surface Model driven by MERRA-2. The model-generated dataset of permafrost conditions is evaluated again site data, global data and remotely (plane) sensed ALT. The comparison to the remotely sensed ALT is probably the most innovative part of the paper, but it also suffers from some drawbacks because the remotely sensed data conspicuously lack spatial variability. The paper is written clearly, the analysis is honest (not obviously trying to hide model shortcomings – but sometimes the assessment of the dataset quality seems a bit too optimistic), the figures are relevant and informative. The paper is a useful contribution, but some aspects detailed in the following could be improved.

General:

- There are lots of global permafrost simulations that are driven by reanalysis-based meteorologies. What is really the added value of this one? The fact that is uses MERRA? In that case, could you say more about specific strengths and weaknesses of MERRA, please? More generally, simulations with other metorological forcing data, and comparison to other model-generated permafrost data sets (e;g. within the Permafrost Carbon Network) could be interesting.

- Some words about potential uses of this dataset could be nice.

Specific points:

- Page 2, line 15: "simulations. . . with the land surface model (Dankers et al., Guimberteau et al., Tao et al.)" -> these are different models. The sentence is misleading, and confusingly, you write ". . .and other numerical models" afterwards. . .

- Page 2, line 20-24: Strictly speaking, the fact that 2017 set records doesn't mean that permafrost conditions will change. 2017 is only one year. It's the long-term trends that matter (2017 is of course consistent with that trend)

- Page 2, line 23: "Some aspects of the current global permafrost thermal states are ... still unknown": can you elaborate on that, please?

- Page 3, line 10: "extensive challenges" sounds bizarre to my non-native speaker's ears

- Page 3, line 16: Could you say a few words specifically about high-latitude performance? Advantages, drawbacks // other reanalyses?

- Page 3, line 26: Chen et al. is a paper in review. Can you reassure the reviewer that these retrievals are independent from the data produced here? One or two sentences would be nice anyway even if Chen et al. 2018 will be available to the reader soon.

- Page 5, line 13: because you later speak about the spinup in the trend analysis, it might be interesting to say a few words about this here. The looping through the 36

years cannot given the same soil temperatures as you would normally have if you have realistic spinup data.

- Page 8, line 15 and following: The assessment might be a bit too optimistic here: Basically one sees that the ALT is between 0.2 and 1m both in obs and simulations, not much more. Is there a significant correlation at all?

- Page 8, line 21 and following: The AirMOSS ALT retrievals. Bascically the retrievals are the same everywhere! Around 0.45 m. No variability. Are they actually of any use?

- Page 8, line 32 and following ("Excluding. . ."): Yes, OK, but then there is still no correlation. Values are just around 0.45 m and the mean ALT of the remaining sites just happens to be around that value.

- Page 9, line 9-10: "Further investigation. . .": You could nevertheless elaborate a little bit on this. Are there any common characteristics of sites with thick active layer (dry soil, highly conducive soil, southward sloping, etc.) that the model doesn't get?

- Page 11, line 1-3 – meteorological forcing dominant control: Of course. Could anyone seriously expect something different?

- Figure 7: Good that this is quantified in such a way here. Much more synthetic & interesting than figure 6.

- Page 13, line 12: Correlation might increase if time steps with snow on the ground but air temp > 0°C are not counted in Tcum - it's the soil surface temperature that counts, not the air temperature.

- Page 14, line 5 : Problems in mountain areas: Snow forcing might be severely in error in these regions

- Page 14, line 12: "The reasons. . ." – I have probably missed the information: How deep is the model soil column?

- Page 16, line 15, Mongolian ALT trends: How can you have a 25 cm/year trend over

17 years? That would mean that ALT increases by over 4 m over that period. That's quite improbable. These data are very suspicious.

- Page 18, line 20 : Âń ..addition of soil layers Âż : Would that be so difficult? Tests with more levels would really be interesting, but if they are really difficult, I refrain from asking for such test to be carried out.

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