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Interactive comment on "The ERA5-Land Soil-Temperature Bias in Permafrost Regions" *by* Bin Cao et al.

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Responses of Anonymous Referee #1

The authors would like to thank the reviewer for the constructive feedback, and the thorough assessment of the manuscript. Below we provide a point-to-point response to each comment, reviewer comments are given in black, responses are given in blue. Additionally, we have included details of how we intend to address these changes in a revised submission.

The study "The ERA5-Land Soil-Temperature Bias in Permafrost Regions" by Cao et

al. evaluates the performance of the ERA5L reanalysis for ground temperatures and other ground-temperature-related parameters in permafrost areas. Although ground temperature is not a main target parameter for such reanalysis products, the study will be a valuable scientific contribution and I recommend publication after carefully revising the manuscript.

Major Comment/Recommendation:

When reading through the manuscript, many important points only became clear to me very late, i.e. in the Discussion. The temperature comparisons of the different products in the Results section, for example, left me wondering on the interpretation and implications. The same applies to the findings on the sizable reduction of "permafrost area" in ERA5L, which only much later is resolved as likely being more an artefact of the model than reality. To a casual reader, the manuscript appears to make a number of potentially bold statements, without providing any hint that the interpretations/ clarification of implications are provided at some later stage in the Discussion (where some casual readers might miss it). While the strict separation of the different manuscript parts is in line with accepted methodology for scientific writing, I recommend guiding the reader through the manuscript in a better way. I have made more specific annotations and suggestions under general comments.
We agree, hints are added as suggested in the specific comments. Especially, Section 5.2 will be mentioned in the caption of Table 2 in order to avoid any possible misunderstanding. We'll also move the implication part from Section Conclusions to the Discussion.

General comments:

Sect. 2.2 Remind the reader in one sentence what HTESSEL is, this is somewhat hidden in the previous text.
In the revision, we'll change this part to *"... a more realistic representation of snow is used in ERA5 land surface model of HTESSEL."*

Sect. 2.3 and 3.1: Please add information on the depths of the available borehole temperatures and how this compares to the shallow ground representation in ERA5L. The Biskaborn-data set, for example, contains many borehole measurements at much deeper layers than ERA5L can represent, so (how) are these measurements used?
Only the observed temperature within the ERA5L soil temperature column, i.e. 0–2.89 m, were used here. In Table 1, we'll add the depth range of used soil temperature observations for each data source. In Section 3.1, we'll add *"Aligned ERA5L soil column, only the observed temperature within 0–2.89 m, were used here."*

l.112: the first criterion is unclear, is this "if T of any of the four layers is constantly below zero for two years"?
Yes. It will be changed as *"if soil at any depth of the four soil layers has an hourly temperature below 0 °C for two consecutive years (ERA5L$_H$);"*

Sect. 3.2 The added value of this is unclear at this stage of the manuscript, it seems to be rather unrelated to the main purpose, i.e. compare the direct ground T output of ERA5L to observations. This becomes clear only much later, but please add a few sentences on the purpose already here.
At the beginning of Section 3.2, we'll add *"Our results show remarkable bias of ERA5L soil temperature in winter that likely correlates with snow depth (Figure 2). For this reason, the suitability of ERA5L soil temperature and the effect of snow-density bias are further investigated with a site specific simulation example at a densely instrumented location near Lac de Gras (LdG), N.W.T., Canada (Figure 1A). This*

detailed permafrost simulation example provides an opportunity to evaluate ERA5L soil temperature under different terrain (e.g. vegetation, soil properties) and snow conditions."

Table 2+3: add the references to the different products used (at least in the caption), so that the readers don't have to search for the abbreviations in the text.
The reference will be added in the caption:

> Table 2: *"...The MAGT$_{avg}$ is the average MAGT: 2001–2018 for ERA5L, 2000–2014 for the CP map (Karjalainen et al., 2019), and 2002–2016 for the TTOP map (Obu et al., 2019)."*
>
> Table 3: *"Note that the CP map only represents permafrost distribution north of 30° N (Karjalainen et al., 2019), TTOP map represents permafrost distribution of the Northern Hemisphere (Obu et al., 2019), and the others represent the area of north of 60° S. Permafrost area from literature is given with their definition in this study.*

l. 129: the purpose of the equation is unclear, and must be explained in more detail.
To clarify the purpose, we will

1) refer to Eq. 1 in Section 3.1: *"MAAT bias and maximum snow depth (SD$_{max}$) were selected as candidate variables to be assessed as possible predictors of ERA5L soil temperature bias (see Eq. 1)"*.
2) this sentence is changed to *"The linear model is used here to predict ERA5L soil temperature bias caused by MAAT bias and snow depth in permafrost regions."*.

If I understand correctly, you relate the bias in MAGT to the bias in MAAT, using the snow depth (which has no bias, I guess since measurements are not available?).

Yes, snow measurements are not available.

Does the intercept of 0.15 make sense, i.e. zero bias in MAAT and zero snow produces an MAGT bias of 0.15? Should one not rather prescribe an intercept of 0 in the equation? I guess it would not change much, considering the R2 of 0.47 of the relationship.

We can expect uncertainty of the linear model with R^2 of 0.47 since it was fitted with limited observations, i.e. 239 grid cells. However, the intercept of 0.15 makes sense. It means even no MAAT bias and snow cover is present, ERA5L soil temperature in permafrost regions could still have bias that may from the other variables, i.e. due to the mismatched depth of observations and ERA5L soil layer.

Table 2: I assume the comparison is done for individual years when- and wherever an entire year of observations is available?

Yes, for MAAT, SO, and MAGT evaluation, the comparison is done for individual available years, while the $MAGT_{avg}$ is the average MAGT for the entire long period. In the caption, we added *"MAAT, SO, and MAGT were evaluated for the individual year, while $MAGT_{avg}$ was carried through once for the entire period."*

How does this relate to CP and TTOP which represent longer periods, are only observation that span the entire periods used? If not, to what extent does the availability of observations influence these comparisons - many observations are likely taken in recent years, which on average were warmer than earlier periods. There is the passage starting with "Note that the performance of CP and TTOP maps may be lower here than reported in: : :", but the implication of this is not really clear.

In Section 3.1, we'll add *"The TTOP and CP map are derived using equilibrium model, and MAGT is given as an average of the entire period (MAGTavg), i.e. 2002–2014 for the CP map and 2002–2016 for the TTOP map, without uniform/specific soil depth. For better evaluation purpose, we aggregate all available observed MAGTs during the period by averaging, and then compared against the $MAGT_{avg}$ of these two maps. Note that the performance of CP and TTOP maps may be lower here than reported in*

the original publications due to differing observations (depths, periods and proportion of sites in mountains) used." to clarify. The sentence of *"Note that the performance of CP and TTOP maps may be lower here than reported in...",* will be removed from Section 4.1

Table 2 seems to suggest that ERA5L is considerably better than CP and TTOP for PF areas, but it is unclear if that conclusion can indeed be drawn. This is not only considering the study periods, but also the spatial distribution of the measurement sites (heavily biased towards China, SE Russia and Alaska according to Fig. 2). This point is adequately discussed in 5.2, but it would be good if some of it could be mentioned already here. At least include a statement "see Sect. 5.2 for a detailed discussion" in the text.

Yes, the summary statistics with sparse data would be misleading. In the revision, we'll add *"Note that the summary statistics present here are based on sparse data and need to be interpreted in light of the considerations outlined in Section 5.2."* in the caption of Table 2.

l. 137: typo "bilinearly"

Will be revised.

Fig. 1: add units in the figure.

The unit will be included in the legend.

Fig. 2 is only presented in one sentence in the text. This should be presented in more detail. I suggest using this to motivate Section 4.3 (see also comment above).

Figure 2 will be added in Section 3.2–*"Our results show remarkable bias of ERA5L soil temperature in winter that likely correlates with snow depth (Figure 2)."*

Table 4: Are there any snow density measurements from the site that could clarify which one of the models is right (or if both are wrong).
There's no snow measurements used here. As we've stated *"we do not imply that GEOtop-based simulations are correct or representing metamorphism in Arctic snow accurately, they demonstrate that simulations with snow cover of similar mass but different density are able to match ground-temperature observations far better than ERA5L."*. In fact, simulating critical snow physical variables in Arctic is challenging (see Domine et al., 2019).

l. 152: Make it clear that this is "ERA5L PF extent as defined in this study", it is clear that the shallow soil column makes it very difficult to relate this to "true PF extent change". Such statements can easily be misunderstood, compare to "Lawrence, D.M. and Slater, A.G., 2005. A projection of severe near-surface permafrost degradation during the 21st century. Geophysical Research Letters, 32(24)." and the resulting comment by Burn & Nelson. This issue is again explained later in the discussion, but make it clear already here, that this by no means represents real PF extent changes.
It will be changed as *"Near-surface permafrost area of ERA5L as defined in this study decreased with a rate of -0.11 (-0.08) $\times 10^6$ km^2 year^{-1} based on hourly (annually) soil temperature (Figure 6)."*

l. 168: what do you mean by "although less permafrost processes are coupled"?
Compared to CLASS-CTEM presented by Melton et al., (2019), HTESSEL includes less physical processes regarding permafrost. We'll change this part to *"...although fewer permafrost specific processes are included in the HTESSEL..."* to clarify.

L. 170: When I look at Fig. 5, I don't quite understand why there is a "remarkably low

bias in PF extent". Your explanations later seem to go in the direction that this might rather be a coincidence, since biases in different regions cancel each other?
The low bias of ERA5L summary statistics in Table 2 is a coincidence as the warm bias in high latitudes (Canada and Alaska) and cold bias in mid-low latitudes canceled each other (Figure 3). The "remarkably low bias in permafrost area" is because 1) ERA5L can only represent the "near-surface" permafrost area due to the shallow soil column; 2) warm bias of soil temperature in high latitudes, especially in northern Canada and Alaska (Figure 1).
Furthermore, ERA5L cannot really represent the discontinuous and continuous permafrost zones, so fractional PF coverage is by definition not included.
The 50% permafrost coverage is used for the IPA map regarding continuous and discontinuous permafrost. Details are present in Section 2.4–*"By following Melton et al., 2019, a threshold of 50% (continuous and discontinuous permafrost zones) for the IPA map and 0.5 for the PZI map, respectively, are used for permafrost area estimation and for comparing areas with binary maps."*

Sect. 5.4: Dedicated snow models like CROCUS and Snowpack also include formulations for compaction due to wind drift which likely occurs at LdG(?). If I understand correctly, this is neither included in the ERA5L model nor in GEOtop? This should be stated, especially since there seem to be no field measurements of snow densities from the site which could clarify which model is more right? I would certainly agree that the GEOtop snow densities look much more realistic, but that's more an educated opinion, rather than science.
The snow compaction due to wind effects is represented in GEOtop (2.0) following Pomeroy et al., (1993), while not in the ERA5L. We considered the wind compaction for all terrain types in LdG except the tall shrubs site. In section 3.2, we'll add *"Snow compaction due to wind effects is considered in 1-D for all terrain types except the tall shrub site (Pomeroy et al., 1993)."*to clarify. In addition, we'll change the last sentence to *"An additional contribution of GEOtop to higher snow densities in tundra*

environments may be considering the effect of blowing snow (cf, Pomeroy et al., 1993)" to clarify.

Discussion general: Consider adding a Section "Implications" or similar – especially the findings on the snow cover and the shallowness of the ground representation are very interesting also for improvements of further reanalysis generations. To me it almost looks like that one might get a pretty good performance for permafrost parameters by doing a couple of obvious improvements of the ground and snow models (which likely wouldn't even cost a lot of additional computation). You study is a great reference for this, and stating this clearer will likely increase the impact of the paper.

The implications were given at the end of the manuscript as part of the conclusions. In the revision, we'll move this part to the new Section 5.5 Implications (as below) in order to make the manuscript more readable:

"While global reanalyses provide urgently needed meteorological drivers for permafrost simulation, their soil data is not well suited for directly informing permafrost research or local adaptation decisions. As such, simulations using permafrost-specific land-surface models driven by reanalyses (Cao et al., 2019a, Fiddes2015) will likely gain importance. Making future soil-temperature products like ERA5L directly usable will require significant permafrost-specific alterations, especially snow cover and the shallowness of the ground representation, to the land-surface models used. If indeed the value of the parameter c_ξ in the snow metamorphism of HTESSL is in error, then this would be an easy improvement."

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