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

Anonymous Referee #1

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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.

General comments:

Sect. 2.2 Remind the reader in one sentence what HTESSEL is, this is somewhat hidden in the previous text.

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?

l.112: the first criterion is unclear, is this "if T of any of the four layers is constantly below zero for two years"?

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.

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.

l. 129: the purpose of the equation is unclear, and must be explained in more detail. 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?). 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.

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

l. 137: typo "bilinearly" Fig. 1: add units in the figure.

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).

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).

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.

l. 168: what do you mean by "although less permafrost processes are coupled"?

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? Furthermore, ERA5L cannot really represent the discontinuous and continuous permafrost zones, so fractional PF coverage is by definition not included.

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.

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.

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