The Cryosphere Discussions

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Comment on tc-2022-71

Anonymous Referee #2

Referee comment on "Brief communication: Improving ERA5-Land soil temperature in permafrost regions using an optimized multi-layer snow scheme" by Bin Cao et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-71-RC2, 2022

Summary

I have produced this review 'blind' and have not looked at the comments posted by the authors that respond to the first reviewer. So apologies if there is any repetition in the points I make.

This paper presents the (hitherto unknown) impacts of the implementation of a new multi-layer snow (MLS) scheme into the land surface component of ERA5 reanalysis (HTESSEL) which was currently operating with a single-layer snow scheme. ERA5 outputs are widely used by the community so a critical appraisal of it's ability to simulate permafrost regions is welcomed and necessary. One of the limitations of the current model set up is that thermal snow metamorphism is programmed to not respond to thermal forcing above densities of 150kg/m3 therefore the simulated densities are artificially low and because of thermal conductivity of snow, ground temperatures biased high. A sensible suite of comparative experiments were proposed to demonstrate how each incremental adjustment to the snow scheme affected soil temperature and led to moderate improvements in some regions (Europe, Alaska and complex terrain) but little/no improvement in others (North America/Tibet) – which is very interesting. Inevitably, the representation of snow and it's layer properties will not be accurate – we see this in most/all major land surface models but addition of a multi-layer scheme is a good step forward.

The average temperature biases are still quite large over some regions so this should be reported in more detail. For this reason I think Figure 2 could be better used to illustrate exactly where the world ERA5 gets subsurface temperatures wrong/right. Please see my major comments.

Overall this is a sound paper which makes a first attempt to improve soil temperature reconstructions and is appropriate for a Brief Communication format but I think at least one Figure could be revised in order to squeeze some more detail out of the analyses provided. Please see my major/minor comments.

Major:

- **More detail:** I feel interested readers would definitely like to see spatial patterns of temperature biases, rather than summary figures as are presented here so that Fig. 3 (permafrost map) may be put into better context about what is driving the discrepancy(ies). The high levels of aggregation (over time, depth and space) are likely masking some very interesting features of this output. The underrepresentation of permafrost regions seems to be related to soil temperatures being too warm in Winter in all cases. Suggestion: Why not show a 4 (region) x 4 (model experiment)-panelled figure of maps of 07-0.28m DJF Bias for the Northern Hemisphere so readers can see what the spatial patterns of this discrepancy looks like ?
- **Thermal Conductivity:** Although I am happy Calonne is an appropriate choice for thermal conductivity parameterisation I'd like to ask the authors for clarification about including the water vapour term which isn't included in Calonne's original equation. We know water vapour diffusion affects thermal conductivity and is difficult to represent. However, Calonne developed their equation based on a quadratic line of best fit using observations of snow density and thermal conductivity. Although the equation is presented solely as a function of density, it is highly probable that when the snow and density measurements featured in Calonne's Fig. 1 were taken that there might have been some water vapour diffusion happening so this effect *could* (or maybe not) already be implicitly included in the equation by Calonne. My concern is that the addition of the water vapour term by the authors may cause this effect to be 'double counted'. I'd recommend the authors re-run one of the experiments (e.g. *MLS Dis+Den* since this provides the biggest improvement) *without* the additional water vapour diffusion term in the Calonne equation and report back on whether omission of this term causes a significant change in the results, or not.
- **Model spin up**. As an experienced user of CESM2.0, which I know is not the model considered here, we've found that subsurface temperatures are extremely sensitive to spin up procedure. The authors should include information about their spin up procedure and whether they have tested the sensitivity of the results to it. See next point……
- **Permafrost extent**. The revisions don't manage to capture southern extent at all but you are only considering 2001-2002. Could this just be an anomalous year to compare to ? Or could this be a result of the spin up procedure contributing to the model state being artificially too warm to start with ? I'm not sure you should read too much into representation of a single year. Perhaps you could add some extra lines to map to show annual extent of the permafrost region as predicted by the model over the time period considered so that the reader can understand the variability of the predicted extent over the time period considered ?
- **Multi- layer snow scheme** – Perhaps the authors could comment on how well (or not) the scheme simulates the different types of snow we see across the northern hemisphere. It's good getting density and thermal conductivity right but if the snow layering and density is not accurate (which I suspect it likely is) this will feed into erroneous thermal conductivity and soil temperatures. Just a comment or acknowledgement required.

Minor:

Figure 1: Typo: Greek symbols differ between being $C_{epsilon}$ in the legend and C_{xi} in the figure caption. I think you need to change epsilon?

Figure 2 (f) Do the blue and red colours refer to ERA5L and MLS-Dis+Den ? If so please add some text to caption for clarity. But please consider revising this figure to show more geographical detail in the biases as per my major comment.

Line 6: Over which region ?

Line 7: Be specific – for which time period are the permafrost area simulations relevant for ?

Line 65 – These values were derived by Sun et al. themselves, it appears, not Jordan according to the Sun et al. 1999 paper – See their Appendix A.

Line 68: What does 'optimized' mean in this context ? When I think of optimization I think of tuning parameters to replicate observations, which doesn't appear to be the case here. The following sentences just simply describe two techniques the introduce sperate densification and layering for differing terrains.

Line 121: Land type, or vegetation coverage can have significant effects on the way that snow models evolve the physical properties of the snow. Presenting the data in Figure 2 as maps of biases may give clues as to whether this is a factor.

Line 121: Why is the word 'BIAS' capitalised ? Is it a software or do you simply mean 'bias' ?

Line 126-127: Please expand – variable performance in which aspects of climate simulation ?

Line 143: ‘MLS with’ – should this be changed to : ‘ A MLS with ‘

Line 147: I do not see this range (0.6–3.0â□¦C and 1.7â□¦C) reflected in the Table or figure. You say it is an improvement – but improvement with respect to what, and for which regions ? Lines 146-147 are not clear to me.

Line 152: …affecting soil thermal conductivity ? This could be another inaccuracy in HTESSEL

Line 161: it isn’t appropriate to recent permafrost loss rates (2002-present) to future projections to 2040.

Line 174: “Temperature observations are access from Cao et al. (2020)” -> rephrase -> “Temperature observations were made available by the authors of Cao et al. (2020).”