

## ***Interactive comment on “Site-level model intercomparison of high latitude and high altitude soil thermal dynamics in tundra and barren landscapes” by A. Ekici et al.***

**Anonymous Referee #1**

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The study describes a model intercomparison for permanently and seasonal frozen ground in tundra and mountains. The work is motivated by the wish to evaluate the performance of different models and to discuss advantages and shortcomings. Some of the models are land surface models (LSM) coupled to atmospheric models (e.g. JULES), while others are stand-alone models (e.g. COUP) or models for vegetation dynamics (e.g. LPJ). While the study as such is done carefully, and is well described and documented, the question remains: What is the major message of this study, and for which audience is this message? Sometimes, the manuscript reads like the authors want to inform the atmospheric community that the LSMs have deficits. This impression arises as almost all of the findings in the study are reasonably well known for the

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cryosphere community, but maybe the findings are not that clear for the atmosphere modelling community. Reading the abstract and the conclusions, the main message is that snow is of high importance for modelling the ground thermal regime close to the surface, which is not a new message. Therefore, before eventual publication, the authors should after my opinion address the following points:

1. The selection of models. You should give reasons why you choose these six models, as there are obviously more models capable to represent ground thermal conditions, both stand-alone models used in permafrost science (e.g. the UaF model GIPL), but also other LSM schemes. Why use Hybrid8? If snow is lacking in the scheme it is clear that the model will fail.
2. The selection of sites. What is the background motivation here? To show diversity? I am certain there are many sites in the world where the data bases is as good or even better than of some of your sites, so why those sites?
3. The model domain and discretization. Normally, for permafrost models a domain of a few m depth is not good enough. So please, discuss this issue. There is also a different discretization with depth, also for the topsoil layer. This has an influence and should be quantified somehow.
4. The modelled deviations in relation to observations: Most of the deviations described in the manuscript are caused by different snow schemes of the models. This is fine, but it is certainly not new or surprising that near surface or ground temperatures then deviate, especially during winter. And of course, if snow is wrong, ground temperatures become wrong independently how good the soil physics is represented in the model
5. The paper structure: The paper is very hard to read, you should really consider distinguishing between pure results and discussion. This is intermixed, and therefore you must write very often sentences in the sense of “as mentioned above, snow matters...”. It is enough to do this once in a paper, and that you can achieve if you discuss snow in a discussion paragraph. Now, you do this for all result paragraphs.

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This would also considerably shorten the paper.

6. You write several times long textbook-like paragraphs that snow insulates, vegetation insulates, the soil water content is important etc, this is information well-known for the cryosphere community and can mainly be omitted.

7. For the ALT you compare a transient approach with an equilibrium approach, of course there are deviations as the simple semi-empirical relations between GST and ALT normally relate to an equilibrium climate conditions, e.g. a climate normal.

8. Conclusions: 1-4 and 5-7 are conclusions from your intercomparison, but not new for the cryosphere community. Conclusion 5 I do not understand. Please explain.

9. The model spin-up: Why different spin-up times for the different models, why not the same spin up, or you spin up one model, and start from this for all the models? Maybe this is better for intercomparison? Probably, I miss something here, but anyway.

10 Introduction: Really, give a good scientific question; objectives etc. (see comments above)

As a suggestion to improve this manuscript, I would re-write and re-structure it. One improvement could be to evaluate really the effect of the different uncertainties, so what is the isolated effect of the snow scheme for the ground temperatures, similar to the effect of the soil scheme. Which of these effects should be improved, maybe a complicated soil scheme is not that important than a more sophisticated snow scheme. Maybe in different climate settings the relative effect is different. I could imagine that a good soil scheme is more important in sedimentary tundra environments than in barren ground and rock? Maybe the snow representation can be more simple in environment A than B? Such questions would certainly increase the merit of this study as they would give useful recommendations.

Minor editorial points: - a location map is missing, I have no idea where all these sites are located - many of the figures are hard to read

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