

Comments on **“Improved soil physics for simulating high latitude permafrost regions in the JSBACH terrestrial ecosystem model”**, by *Eciki et al.*

General comments

This article describes very necessary refinements performed on the JSBACH model to improve its representation of high-latitude climate. In this respect, it seems perfectly suited for publication in GMDD.

The description of these developments is followed by a thorough validation of model results against numerous available datasets, which is also a great strength of the paper: the authors have great merit in using so many datasets and using them in a very reasonable way.

However, the manuscript could benefit from a more detailed model description in some aspects (especially as the cited Hagemann et al., 2013 paper is not published yet). Also, the model evaluation performed does not help assessing the improvements linked to the new developments, as no comparison between the new and the old scheme is performed. Fair enough, many modelling papers highlight the improvements induced by the representation of latent effects on the soil thermal dynamics, or the added value of a multi-layer snow scheme... But maybe just a simple plot of the soil temperature at Nuuk when using the old version of the snow model could help illustrating this, and confirm that, although still perfectible, the new snow scheme brings valuable improvements.

The lack of evaluation of the added value of each specific development may be a bit more critical for the moss layer: First, it is a common but not general feature of circum-polar landscapes. Second, moss and top-soil organic matter have not only thermal but also hydrological properties which can modulate their impact on the soil thermal dynamics (e.g. Rinke et al., 2008). I suggest that the author improve their justification for the choice of a uniform top-soil moss layer, and give some assessment of its impact. Typically, does such an organic layer exist at Nuuk, and how does it impact the thermal dynamics there? This would be the only major revision point.

At some points, the analysis of model vs. observational results could be complemented; some recommendations in this direction are mentioned in the following comments, along with further minor issues.

As a conclusion, I evaluate this paper suitable for publication, pending the revisions mentioned above and below.

Specific & technical comments

Abstract

The first 10 lines of the abstract should be cut or considerably shortened. Not that this is not interesting, but an abstract should mainly outline what has been gained by the authors' work, not recall too many known general facts.

1. Introduction

The introduction is relevant but some references have to be revised.

p. 2657 l 6: Ciais et al. 2011 is not the most appropriate reference.

p. 2657 l 9: again, this reference is not appropriate. De Conto et al. investigate mechanisms from the Eocene; glacial-interglacial periods that lead to current permafrost organic matter accumulation occurred during the Pleistocene.

p. 2657 l 19: spurned -> spurred ? ; advancement -> advances

p. 2657 l 23: again, reference somehow inappropriate. Riseborough et al., 2008 review existing permafrost models at different scales with no emphasis on other (e.g. C-related) permafrost processes crucial for climate and arctic modelling. Typically, this reference could be postponed to the next sentence, and complemented by others regarding ecological processes.

p. 2657 l 24: Some LSM also include lots of other permafrost-related processes: Cryoturbation, organic matter decomposition functions at subfreezing temperature, O₂ limitations, methanogenesis.. Freeze-thaw thermodynamics is surely crucial but these other processes should also be mentioned.

p. 2658 l 11: although this was truly highlighted by Gouttevin et al., 2012b, this comes after previous study have provided basic knowledge about these implications _ typically, Kelley et al., 1968 should also be cited.

2. Methods

2.1.

- The use of a constant and uniform moss layer over the soil does not seem very realistic...

You could at least discuss the possibility of a geographic/biome-dependant distribution of this layer (e.g. following Rinke et al., 2008)

- Phase change: is the soil thermal numerical scheme run a third time after phase change, to compute a realistic soil temperature profile after adjustments due to latent energy?

p. 2661 l 11 : whereat - > whereby?

p. 2661 l 16 : evapo-transpiration

p. 2661 l 16: as Hagemann et al., 2013 is not published yet some additional details could help the reader! Here are some questions that could be addressed:

a. How many layers / uppermost soil centimetres are concerned by the infiltration of the infiltrable water, or by evaporation? How is this infiltration parameterized?

b. "if the water and ice are fully occupying the field capacity that layer is blocked for a further water transfer.." This is not really clear. I assume that such a layer still can loose water through diffusion/percolation? Or does it mean that a saturated layer with ice content of 0.001% impedes water transport?

p2662 l 10/12: indexing issues for θ_{wmax} between both expressions

p2662 l 13: I suggest adding what the authors wrote later, e.g. the fact that thermics & hydrology are also coupled through the water phase change latent heat exchange.

- about the snow scheme : what happens when snow depth is less than 20 cm and not an exact multiple of 5 cm ?

p2664 l 26: data.

P2665 1 2: which set of soil parameters do you use?

2.4.1.

Was any gap-filling required to use this Nuuk dataset for the model? If so, a line on that would be appreciated.

2.4.2.

- comparison with the IPA map : you need to define the ‘frozen’ state in the model more explicitly, as it can have different definitions : soil (but which soil layer ?) temperature below 0°C ; fraction of frozen water content exceeding 50 %... etc.

This may also help refine/justify to what kind of permafrost (continuous, discontinuous...) you compare your model outputs to.

You may also want to drop a line on why the year 1990 is chosen to compare the model outputs to the IPA map (with respect to the historical data sources that are compiled within this map). Wouldn't a 1980-1990 average be more appropriate for this comparison; does it change things?

- comparison of ALT at CALM sites:

Do you use a special interpolation method for your temperature profile? (e.g. fitting an exponential profile to your 5 layers values?)

Averaging over the years with available data at the sites suppresses a possibly huge interannual variability; performing a year-by-year comparison could help isolate specific years and conditions when the model performs better or worse. Does a scatter plot (like Fig4) without averaging over the years help improve your diagnostic of model performances and your conclusions?

2.4.3.

- permafrost temperature map: does this dataset specify a representative depth for the dataset ? If so, mentioning it would be valuable for comparison purposes.

3. Results and discussion

3.1. Nuuk

p. 2669 l23 to p. 2670 l: too redundant with the introduction.

p. 2670: Though not being an expert on snow, I'd like to point out some inaccuracies (further inaccuracies may remain...)

a. “with higher density the snow insulation effect decreases due to increased heat conductivity”. This is unfortunately not that simple and there is a wealth of literature in favour or against a deterministic relationship between snow density and conductivity (e.g. Sturm et al., 1997). Besides, this gravity-driven densification is clearly not the only process affecting the snowpack conductivity (for instance highly insulative depth hoar can form at the bottom of arctic snowpack on the course of the snow season; Sturm and Johnson, 1992).

To avoid drowning into a complexity that does not match the snow model used here, you could take the snowpack gravity-driven densification and concomitant increased in thermal conductivity as a plausible evolution of your snowpack and derive your analysis from that. But do not imply that this is the ‘usual’ way that snow evolves...

L 16 : the spring **lower** insulation.

b. the results you obtain at Nuuk can also be symptomatic of other snow-related mechanisms : rain on snow events ; percolation (and thermal advection) of rain water/meltwater within the snowpack, that gradually warm up and partially thaw the soil; resulting in soil temperature close to 0°C in late April and May while your model is still below 0°C. Rain on snow events or surface melting also decrease the snow surface albedo (something your model probably does not represent) and enhance the solar energy absorbed by the snowpack in spring. You may check in your data weather such rain-on-snow / surface melt events are plausible and if so, complement your analysis in this direction. References on that can be found in Westermann, 2009 (PhD thesis).

c. Langer et al. 2013 surely highlight this effect but earlier references are also needed (e.g. Zhang et al., 2005).

3.2. Circum-Arctic validation

P 2671 l 12: suppress the “have”

L23 “favouring northern slopes”: this is really interesting to everyone using these data. Do you have any reference on that?

Could the ALT overestimation by the model be induced by an underestimation of the ground-ice content? Are some of your stations located within identified ice-rich permafrost regions? Does the moss layer in the model reduce your ALT overestimation?

p.2672 l 20 : around 10 m -> at 10 m depth

3.3. Continental scale validation

- permafrost temperatures : soil column depth surely explains part of the cold bias but there must be other reasons leading to this specific error pattern. For instance, Kolyma regions experience as extreme temperature gradients as Yakutia but the cold bias is less strong there. Some studies mentioned critical snow underestimation by atmospheric forcing datasets in Yakutia, and from my experience this is still a deficiency of state-of-the art climate forcing data like WATCH. You may want to mention or investigate that.

- ALT differences over Yakutia : using a uniform moss layer at high altitudes is indeed subject to discussion; however, the insulating effect of this layer should prevent from summer warming (and thus lead, if you overlook the winter effect, to thinner ALT, which is the contrary to what you state ...) Please do clarify this or argument against me.

- Thick ice overburden exists in coastal area and may explain your ALT overestimation in the model.

3.4. River runoff validation

- As I stated regarding the Introduction, additional precisions regarding the hydrological soil-freezing module are needed to enlighten this part. Additionally, how does freezing affect infiltration?

- For both Lena and Yeniseï, correlation coefficients on the Fig 10 and 12 could support your analysis.

- The divergence between modelled and observed runoff for the Yenisey over 1982-2000 is a stunning feature, and possible causes could be explained more readily: global dimming, increased CO₂ effect on stomatal conductance.. Would a possible contribution from glacier & permafrost melt be of significant magnitude when compared to model-to-data divergence?

4. Conclusion

p. 2676 11: suppress have, you even can use the present tense.

References (when not cited by the paper already)

Kelley J.J.J., D.F. Weaver, and B.P. Smith (1968), The variation of carbon dioxide under the snow in the Arctic. *Ecology*, 49: 358–361.

Sturm, M. and J. Johnson (1992), Thermal conductivity measurements of depth hoar, *J. Geophys. Res.*, 97(B2), 2129–2139, doi :10.1029/91JB02685.

Sturm, M., J. Holmgren, M. König and K. Morris (1997), The thermal conductivity of seasonal snow, *J. Glaciol.*, 43(143), 26-41.

Westermann, S. (2009). The Sensitivity of Permafrost. *PhD thesis*, University of Heidelberg, Germany.

Zhang, T. (2005), Influence of the seasonal snow cover on the ground thermal regime: An overview, *Reviews of Geophysics*, 43(4).