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Interactive comment on "Improved soil physics for simulating high latitude permafrost regions by the JSBACH terrestrial ecosystem model" *by* A. Ekici et al.

Anonymous Referee #1

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The manuscript "Improved soil physics for simulating high latitude permafrost regions by the JSBACH terrestrial ecosystem model" by Ekici et al. documents improvements in the representation of soil processes in the JSBACH land-surface scheme. It involves a technical documentation of the implemented physical equations, as well as a validation with different data sets on a range of scales. While the manusript is generally well written and clearly structured, some sections, in particular the model description, feature many small errors and seem prepared with less care than they should have been. In addition, I have a few major points that I would like the authors to take into account:

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Major comments:

1. When citing previous work, the authors should distinguish better between studies presenting field evidence for a certain effect and modeling studies hereof. An example is p. 2657, l.5, where the at least partly modeling-based study by Ciais et al. 2011 is cited as evidence for "vast amounts of organic matter" accumulated in permafrost areas on glacial/interglacial timescales. While Ciais et al. certainly deserve credit for attempting to quantify the size and changes of the permafrost carbon pool, the fact itself has been known before from more observation-based studies, e.g. Zimov et al. 2006 (which almost certainly have inspired the later modeling studies). The same is true for the following sentence, where the modeling study of DeConto et al. 2012 (which actually focuses on potential permafrost dynamics 55 Mio years ago) is presented in conjunction with a rather crude description of the genesis of today's organic-rich permafrost. The authors should carefully consider the extensive literature on this subject (e.g. the overview on Yedoma permafrost by in Schirrmeister et al. 2013), and rephrase the sentence accordingly. There are more of such examples in the manuscript, where modeling of an effect is presented as proof of its existence (see also Minor comments).

2. The study aims for incorporating an improved representation of physical permafrost processes in JSBACH, with the longer-term goal to model the permafrost-carbon feedback (as stated in the Introduction). So, why chose a validation site in Nuuk in Greenland, which is at the very edge of the present permafrost extent, and in a region that hardly matters for greenhouse gas emissions in a global context? It would be a lot more convincing to present sites in Siberia or in Alaska (where similar forcing data sets are available). Furthermore, the authors only present time series of temperatures at the soil surface and not in deeper layers, so it is hard to judge the model performance e.g. in 1m depth. While the authors are of course free in their choice of the validation site, the mod-

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eled topsoil temperatures in Nuuk are not evidence enough to rule out potential problems with the time integration in case of soil freezing/thawing: Fixed-time-step integration schemes can potentially "jump" over the temperature region in which the phase change of the water occurs, in particular in case of large temperature gradients (as they e.g. occur in the continental climate of Siberia while being much smaller in Nuuk). Gouttevin et al. 2012a describe in relative detail that they use an artificial, unphysical freezing characteristic, that is spread out to -2°C, to prevent such numerical problems. Depending on the parameters used for Eq. 3, a large part of the latent heat change can occur in a narrow temperature range, and the authors do not provide any evidence that the time integration scheme of JSBACH can cope with that. As suggested above, the potential problem should be most significant in a continental climate with large temperature amplitudes and thus large temperature gradients in the soil. Therefore the authors should present further site validation for forcing data from continental permafrost conditions, where larger soil temperature gradients occur. In addition, the authors should make sure and explicitly state in the paper (one sentence on this would be enough) that their soil scheme is energy-conserving for a reasonable range of forcing data and soil/snow parameter sets, i.e. that the ground heat flux at the surface integrated over time is equal to the change of the energy (sensible plus latent heat) content of the ground in the correponding time interval.

Minor comments:

p. 2656, l. 4: temperature -> temperatures

p. 2657, l. 4: Gruber 2012 uses a simple model to connect probability of permafrost occurrence to air temperature, and gives the range of uncertainty to 19-25% (of areal fraction of the Northern Hemisphere). In his Conclusion, Gruber 2012 states: "While the dataset presented here can be used as a reference for model evaluation, it does then by no means represent reliable ground truth." Therefore, a more careful

formulation than "22% ... is underlain by permafrost" is warrented.

p. 2657, l. 14: Carbon can't thaw. Carbon-rich soil can.

p. 2658, l. 11: Gouttevin et al. 2012 have shown that their model is sensitive to snow, etc. The effect itself is well-established for a long time, so either the appropriate studies should be cited (e.g. Goodrich 1982, Groffman et al. 2006), or the sentence should be rephrased to reflect the modeling nature of the Gouttevin et al. study

p. 2658, l. 21: Soil freezing does not lead to dryer summers, etc. Incorporating soil freezing in the model used by Poutou et al. leads to dryer summers in the model results.

p. 2659, l. 5: on the climate system

p. 2660, l. 13: Since the total depth of soil column is a crucial parameter for realistic permafrost simulations extending over decades and centuries, it should be explicitly stated in the text, not only in Fig. 1.

p. 2661, l. 11: whereas

p. 2662, l. 5: unit of the source/sink term?

p. 2662, Eq. 3: The factor 10^3 has its origin in the fact, that Niu and Yang 2006 define the saturated soil matric potential in mm, not in m, as it is done here.

p. 2662, l. 10ff: the parameter b?

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p. 2662, l. 10ff: in the definition of the units, volumetric fractions are partly given as mm^{-1} and partly as m^3m^{-3}.

p. 2662, l. 10ff: the unit of acceleration is ms^{-2}

p. 2662, l. 16: amounts ... influence

p. 2663, l. 5: according to the definition in Eq. 4 (density times heat capacity), it must be specific heat capacities here, not volumetric.

p. 2663, l. 5: the volumetric soil water content has been defined as theta without subscript "w" on the previous page.

p. 2663, Eqs. 8/9: insert proper units to the forefactors, or clarify that all variables must be inserted in the units given.

p. 2663, l. 12: S is used both for source/sink in Eq. 2 and for saturation.

p. 2665, l. 12: The parameters (soil, vegetation, etc.) used for the simulation should be stated.

p. 2665, l. 16: How many years?

p. 2666, l. 2: From this extremely sparse description of the Nuuk site, it is impossible to judge whether the simulations performed for this site are an acceptable match for the data. There should be details given on the borehole, the measurements, the soil conditions and stratigraphy, the soil ice content, the snow conditions, the time series of meteorological data, in particular data gaps, the measurement setup (e.g. is the

snow depth measured at the same spot as the ground temperature? If not, is there strong wind drift of the snow at this site?), etc. Furthermore, a short assessment of the general permafrost conditions in the area should be given, in conjunction with a clear statement on the representativeness of the (one) site for permafrost conditions worldwide (which is the target of the model).

p. 2666, l. 4: at an altitude

p. 2666, l. 9: For which period were the data downloaded? How exactly is the forcing file created?

p. 2667, l. 9: There are several vegetation tiles per grid cell in the model. How is this taken into account when it comes to selecting the most appropriate site?

p. 2669, l. 7: From the time series of topsoil temperatures, the performance of the soil freezing scheme can not really be inferred. Please also show deeper soil temperatures.

Fig. 2: Does this situation represent permafrost conditions at all? From a quick glance it looks like the yearly average of the measured ground surface temperatures is quite a bit above zero degrees?

p. 2670, l. 6 ff. I doubt that this conclusion can be drawn from the presented data so easily. Yes of course, snow gets compressed, there is snow metamorphosis, etc., but there is also meltwater infiltration in spring and a great deal of uncertainty in the many model parameters. To back up this statement, the authors would have to perform a sensitivity analysis of their model.

p. 2670, l. 19: Another recent study highlighting the large influence of the thermal snow properties on modeled permafrost extent and temperatures is Westermann et al. 2013.

p. 2671, l. 17: I think the considerable model uncertainty (5 soil layers, third layer in which the active layer terminates at most of the CALM sites, is almost 1 m thick!) should not be forgotten here...

p. 2671, l. 27: Soil subsidence is on the order of mm and cm. The mismatch between model and measurement is often many tens of cm. Please delete this statement! I agree that there is some uncertainty in the CALM measurements, but in the light of the significant mismatch presented in Fig. 4, this measurement uncertainty is most likely negligible.

p. 2672, l. 20: With a soil column depth of 10 m, and a model period >50 years, the results should be widely independent of initialization. Therefore, I strongly doubt this statement. If the authors disagree in this point, I would like to see a sensitivity analysis concerning the initialization. Langer et al. 2013 performed such an analysis for a transient permafrost model and found a low model sensitivity to initialization even for a considerably shorter model period.

p. 2673, l. 15: I don't understand why a deeper soil column should lead to warmer soil temperatures. It will improve the transient response of the model under changing climate conditions, but it will not make soil temperatures warmer, unless past climate conditions were warmer than today (which is not the case) and the heat stored in deeper ground layers now warms the upper layers from below? Please explain. On the other hand, incorporating e.g. a geothermal heat flux at the lower boundary would

indeed result in warmer soil temperatures. However, given the small geothermal gradients I doubt that a mismatch of several degrees can be explained.

p. 2675, l. 21: the size of the Lena River basin is on the order of 2.500.000 km2, please reconsider the use of the word "bigger". Also, only about half of of the Yenissey catchment is underlain by permafrost (how much is it in the simulations?), so a lot of other factors will play a role. In addition, I am not entirely sure that a meaningful hydrograph can be created from simply summing up the runoff of all grid cells along such long rivers. The authors assume that all water reaches the ocean after two months, but this will strongly depend on the distance of the cell from the mouth. Also the shift by two months seems rather arbitrary, why not shift the model output by e.g. 1.7 months and then compute monthly sums? That would change the height of the runoff peak, and is just as plausible.

References:

Goodrich, L., 1982. The influence of snow cover on the ground thermal regime. Canadian Geotechnical Journal 19 (4), 421–432.

GROFFMAN, P. M., HARDY, J. P., DRISCOLL, C. T. and FAHEY, T. J. (2006), Snow depth, soil freezing, and fluxes of carbon dioxide, nitrous oxide and methane in a northern hardwood forest. Global Change Biology, 12: 1748–1760. doi: 10.1111/j.1365-2486.2006.01194.x

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Westermann, S., Schuler, T. V., Gisnås, K., and Etzelmüller, B.: Transient thermal modeling of permafrost conditions in Southern Norway, The Cryosphere, 7, 719-739, doi:10.5194/tc-7-719-2013, 2013.

Interactive comment on Geosci. Model Dev. Discuss., 6, 2655, 2013.