We thank the reviewer for his/her substantial effort on evaluating our manuscript. It is clear that the reviewer's observant eye will help improve our paper in many ways. We agree that many details have been overlooked through the manuscript preparation and we appreciate the reviewer for pointing out these issues. We have carefully improved the manuscript according to the comments and provided more detailed information below (all in red color).

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 manuscript 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:

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).
   We completely agree with the reviewer that model results should not be presented as proof of existence. References are corrected accordingly: Ciais et al. reference replaced with Zimov et al., 2006 and Schirrmeister et al., 2013 is added to DeConto reference.
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 modeled 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 corresponding time interval.

The choice of Nuuk site was mainly due to data availability, not to show a perfect permafrost example. We are aware that Nuuk is situated at the edge of the permafrost region and the soil is not as carbon rich as in Siberia. However, to have a most informative model-data comparison, we need to force the model with all the meteorological observations from the site. Nuuk site has a comprehensive set of variables observed at the same place as the soil temperatures. This is not always the case in Alaskan or Siberian sites which is a pity because we wished to have more sites available for this evaluation study. Additionally, we would like to take your attention to the fact that Nuuk is a tundra site (which represents a major land cover in the Arctic) and the climate conditions are creating a soil thermal profile that allows freezing and thawing during the year. With the comparison to Nuuk topsoil temperature we aimed to evaluate the freezing scheme in general for which e.g. the existence of the ‘zero curtain’ (phase change) is important. The additional evaluations of ALT and deep soil temperature using global observation networks and the Yakutsk map are important complements to the Nuuk site temperature dynamics comparison.

Since the soil temperature observations were only up to 30 cms in Nuuk, we have shown only the topsoil temperature comparison in the manuscript. Please see figures 7-10 of this document for the deeper JSBACH soil temperatures.

The numerical schemes used for phase change calculations were briefly discussed in the introduction section. Applying the apparent heat capacity method and distributing the phase change over an artificial temperature window is one way of doing this. Our choice of adding the latent heat term as a second step was also successfully implemented in previous studies (Takata and Kimoto, 2000; Mölders et al, 2003; Lawrence et al., 2008). To support the stability of this scheme, we added the surface energy balance calculations for Nuuk and two more continental grid cells in figures 1-6 of this document. The plots indicate that the energy closure has an imbalance on the order of 5 to 15 W/m^2. As shown by Stephens et al. (2012) that many GCMs have 10 to 17 W/m^2 uncertainty in the surface energy balance when compared to the satellite observations. The JSBACH energy balance offset falls into this order of uncertainty.

Minor comments:

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 warranted.

Sentence rephrased as: "*Based on simple relationship between air temperature and the permafrost probability, Gruber (2012) estimated that around 22% (+- 3%) of the northern hemisphere land is underlain by permafrost.*"

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

Thanks. Now corrected.

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

Sentence rephrased as: *"Goodrich (1982), Kelley et al. (1968) and Groffman et al. (2006) found that snow cover strongly influences the ground thermal regime. Using the ORCHIDEE model, Gouttevin et al. (2012b) showed that the snow cover and the disappearance of snow are important factors for the plant and soil metabolic activity and biogeochemical feedbacks between the soil and the atmosphere.."*

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.

Sentence corrected as: *"It is shown by Poutou et al. (2004) that including soil freezing in their model leads to dryer summers and warmer winters in different regions."*

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

Corrected.

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.

The parameter 'total depth of the soil column' was mentioned in p.2660, l.22 in addition to the figure caption.

p. 2661, l. 11: whereas

Deleted.

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

Thanks. Description added: "*S*: source and sink terms (s^{-1})"

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.

Thanks. We corrected the equation by removing the term 10^3

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

Thank for this hint. We added a respective description of this parameter: "*b*: Clapp and Hornberger exponent (-)"

p. 2662, l. 10ff: in the definition of the units, volumetric fractions are partly given as mm^{-1} and partly as m^3m^{-3}.
The ecosystem model assumes all fluxes being defined per square meter. Therefore, in the original manuscript we sometimes used the short definition of volume also in this way. However, this is literally incorrect and has been improved in the revised version.

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

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

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

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

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

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

p. 2665, l. 12: The parameters (soil, vegetation, etc.) used for the simulation should be stated.
A parameter table for Nuuk site simulation is added to the revised version of the manuscript. Please see Table1 in this document.

p. 2665, l. 16: How many years?
Information added: from July 2008 to December 2010

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).

This is a good point. We fully agree with the reviewer that more site information is useful. However, even after extensive searching we could not find more detailed information on soil conditions and exact snow measurement point. These information were not available from the data reports, and as we mentioned above, the aim was to test the freezing scheme regardless of the permafrost condition or existence of carbon rich soil at the site.

p. 2666, l. 4: at an altitude

Thanks. Now corrected.

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

Information added: *"For the meteorological variables, the time period used was July 2008 to December 2010, while the soil temperature was available from August 2008 to December 2009. The downloaded ascii files were combined in a netcdf format file and minor gap-filling was needed to create continuous climate-forcing to force the Nuuk site level simulations."*

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?

The choice was mainly done by considering the upland or lowland CALM sites as well as choosing the sites with longer time period of data. Some of the sites with only one or two years of ALT measurement were excluded from the comparison. The measurement method was also considered such that the spatially averaged sites were used instead of single measurement sites or the sites that estimate the ALT from ground temperature. In general, vegetation cover is assumed to be representative of the selected CALM 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.

Please see the Figures 7-10 below. There is freezing and thawing until the third soil layer at Nuuk in JSBACH. The zero-curtain is more pronounced with the amount of water in the second layer. Since the third layer can partly hold water (due to soil depth), there is less phase change.

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?

It is true that according to the JSBACH simulation, the lower soil layers are not representing permafrost conditions. As stated above, it was important to have freezing and thawing in the upper layers to evaluate our phase change scheme. Of course, our comparisons to active-layer thickness measurements (CALM), permafrost temperature (GTN-P) and the Yakutsk map are important additional parts for the evaluation of the heat balance scheme. Please see also our replies to the comments above.

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.
Section extended with: "*Effects of rainwater infiltration or snow meltwater on snow properties are also suggested to be important. Snow properties can be altered due to water percolation into the snowpack. Additionally snow albedo changes with these processes. Such effects are still not represented in the current version of JSBACH.*"

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.
Thank you for the information. Now cited.

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...
We completely agree with the reviewer. Sentence extended: "*Reasons for this mismatch are mostly explained by scale issues and site-specific conditions together with the model vertical resolution*".

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.
Thank you for the accurate information. We were not certain about the order of uncertainties. Now the sentence is deleted.

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.
Thanks for the hint to the Langer et al. study. We also agree with the reviewer in general and also assume that last century temperature was not warmer than 1901-1930 for most places, although we cannot judge this at the moment based on data. Therefore, we delete this sentence.

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.

Having a 10 m soil depth is not enough to capture the depth of zero annual amplitude in some areas. This can lead to uncertainties in the soil temperature simulations. Alexeev et al. (2007) have mentioned the importance of using more than 50 m soil depths for capturing the decadal temperature variation in the soil. However, we agree with the reviewer that past climate determines deep soil temperature. The exact climate during the past centuries is unknown for many remote areas and the effect of e.g. the little ice age will hardly explain a temperature underestimation of several degrees Celsius. It is correct that we do not know if having a deeper soil column will warm or cool the 10 m temperatures, in particular when using the same climate for the spin-up time period. Therefore, the sentence is rephrased as: *"A deeper soil column representing up to 50 meters is suggested to improve the permafrost temperature results around 10 meters (Alexeev et al., 2007), in future model versions; although effects of having a deeper soil column is not clear yet."*

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".
Thanks. Now deleted.
Also, only about half 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.
To address this point a lateral discharge model (Hagemann and Dümenil, 1998) is used to cross check our results. Using our JSBACH results, daily runoff and drainage outputs are used to force the hydrological discharge (HD) model to calculate a more accurate river runoff. Please see the figures 11-12 below. The HD model showed a similar runoff curve for both rivers. The method of shifting runoff values by 2 months in the JSBACH outputs was in match with using the lateral flow model. However, the HD model simulated less runoff in the Lena river basin.

References:

Alexeev, V. A., Nicolsky, D. J., Romanovsky, V. E., and Lawrence, D. M.: An evaluation of deep soil configurations in the CLM3 for improved representation of permafrost, Geophys. Res. Lett., 34, L09502, 2007.

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

Hagemann S. and Dümenil L.: A parameterization of the lateral waterflow for the global scale. Clim Dyn 14: 17-31, 1998.

Schirrmeister L., Froese D., Tumskoy V., Grosse G. and Wetterich S. (2013) Yedoma: Late Pleistocene Ice-Rich Syngenetic Permafrost of Beringia. In: Elias S.A. (ed.) The Encyclopedia of Quaternary Science, vol. 3, pp. 542-552. Amsterdam: Elsevier

Stephens G.L., Li J., Wild M., Clayson C.A., Loeb N., Kato S., L'Ecuyer T., Stackhouse Jr P.W., Lebsock M. and Andrews T.: An update on Earth's energy balance in light of the latest global observations, Nature Geoscience, 5, 2012.

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.

Supplementary Material:

Table1: JSBACH model parameters used in the Nuuk site simulation

Veg. cover type	Tundra with 10 cm moss cover
Porosity (θ_{sat})	46%
Field capacity	36%
Soil depth before bedrock	36cm
Soil mineral heat capacity (c_s)	2213667($Jm^{-3}K^{-1}$)
Soil mineral heat conductivity (λ_s)	6.84 ($Wm^{-1}K^{-1}$)
Saturated hydraulic conductivity	$2.42x10^{-6}$ (ms^{-1})
Saturated moisture potential (ψ_{sat})	0.00519 (m)
Clap and Hornberger exponent (b)	5.389 (-)

SURFACE ENERGY BALANCE OF JSBACH SIMULATIONS

The surface energy exchange is calculated for three sites with the equation below. In addition to the Nuuk site (which was used in the paper to evaluate soil processes), we have included two more continental grid-cell in Siberia (lat: 72.2N, lon: 126.27E and lat: 64.75N, lon: 140.25E).

$$balance = Rnet + SH + LE + G + Snowmelt - Q_{surf}$$

Rnet = net radiation at the surface
SH = sensible heat flux
LE = latent heat flux
G = ground heat flux
Snowmelt = energy used for melting snow
Qsurf = change in the surface heat storage

As it is seen from Figures 4-6 that the net radiation input during summer is used first with the snowmelt and then mostly with the sensible heat flux. We have calculated all the energy flux terms by using the JSBACH outputs. In this fashion, by counting on a backwards calculation of all these terms, there is some uncertainty involved in the overall balance. Nonetheless, the *balance* term shows small values compared to the other components and this supports that time integration scheme is working well for Nuuk as well as more continental grid-cells.

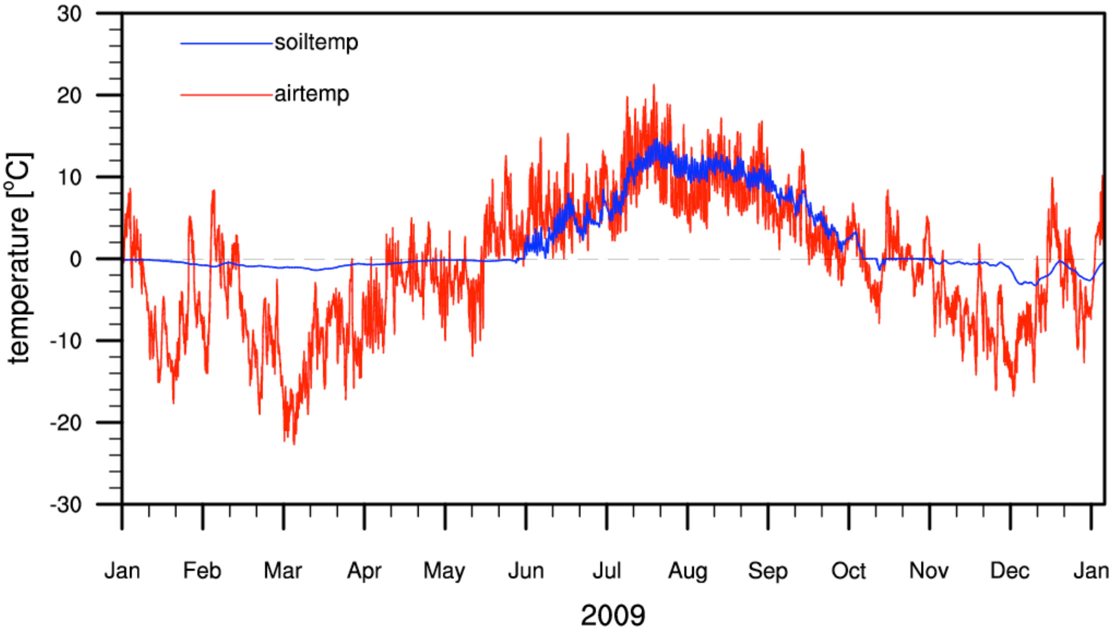

Figure1: Air and first soil layer temperatures at Nuuk for 2009

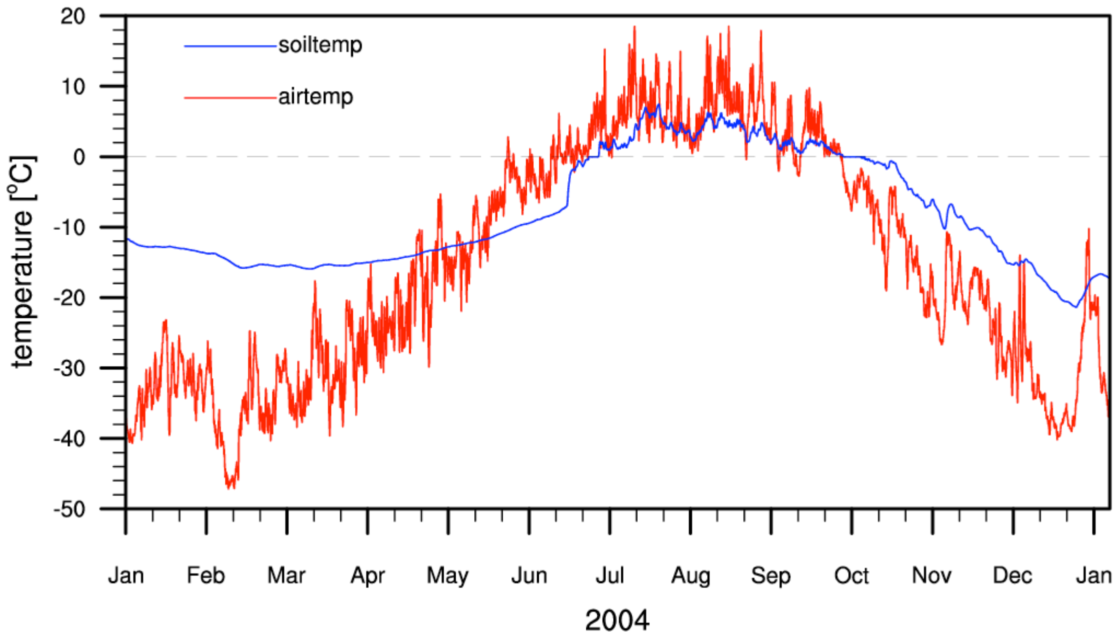

Figure2: Air and first layer soil temperatures at Samoylov for 2004

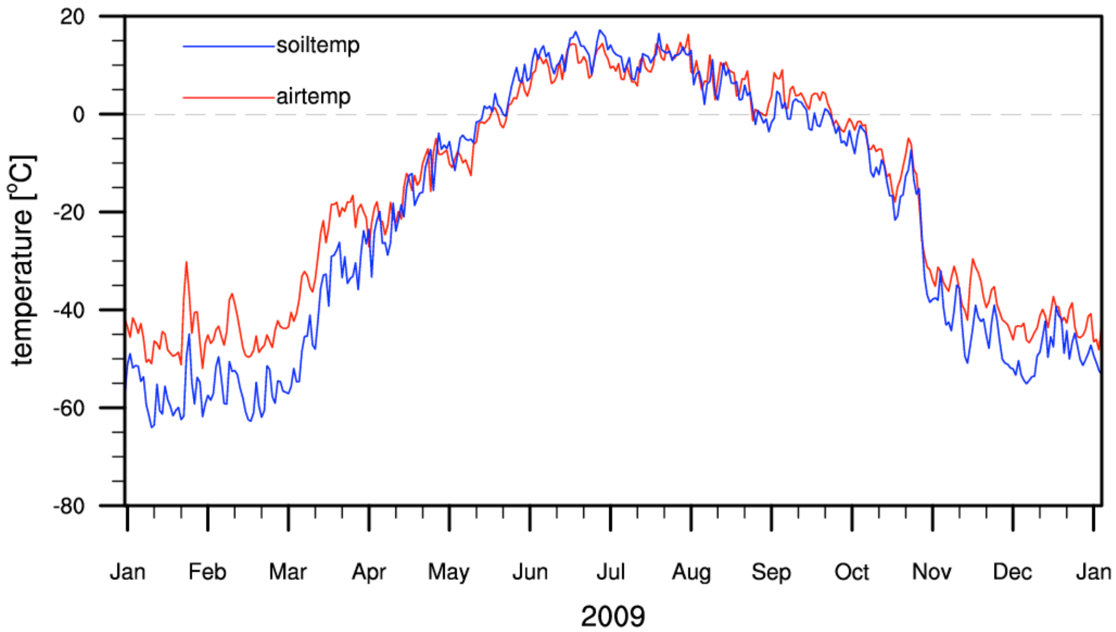

Figure3: Air and first soil layer temperatures at Siberian grid cell for 2009

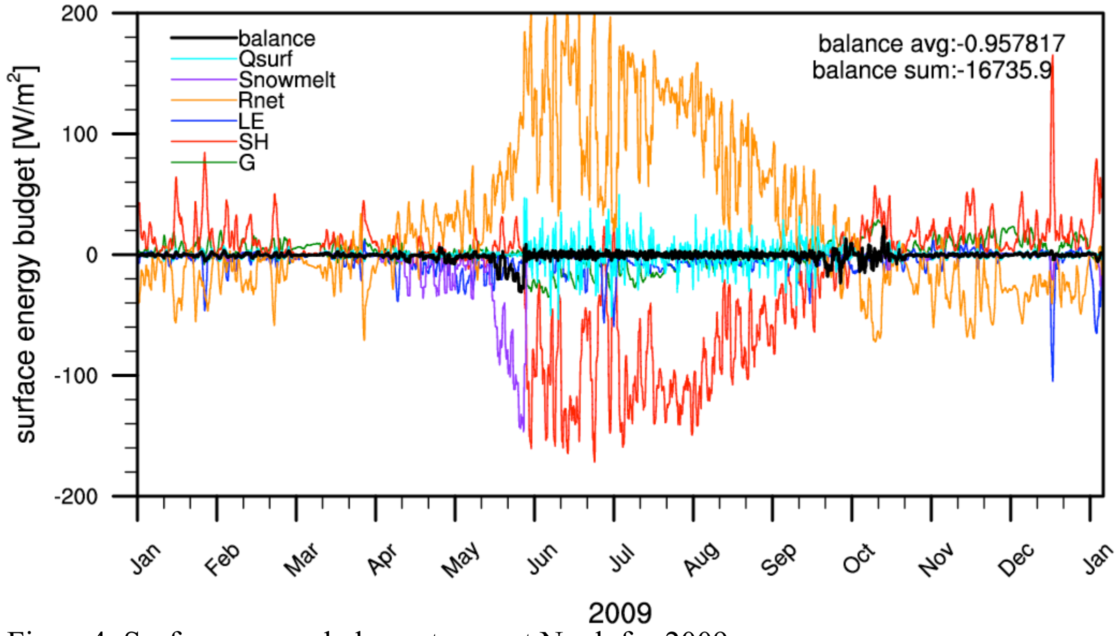

Figure4: Surface energy balance terms at Nuuk for 2009

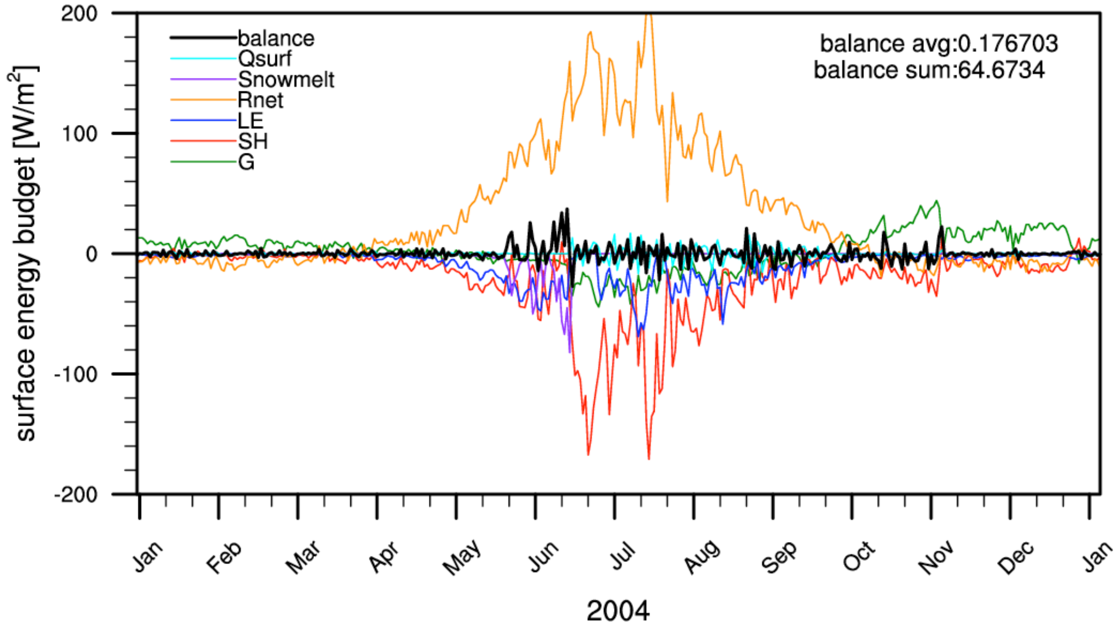

Figure5: Surface energy balance terms at Samoylov for 2004

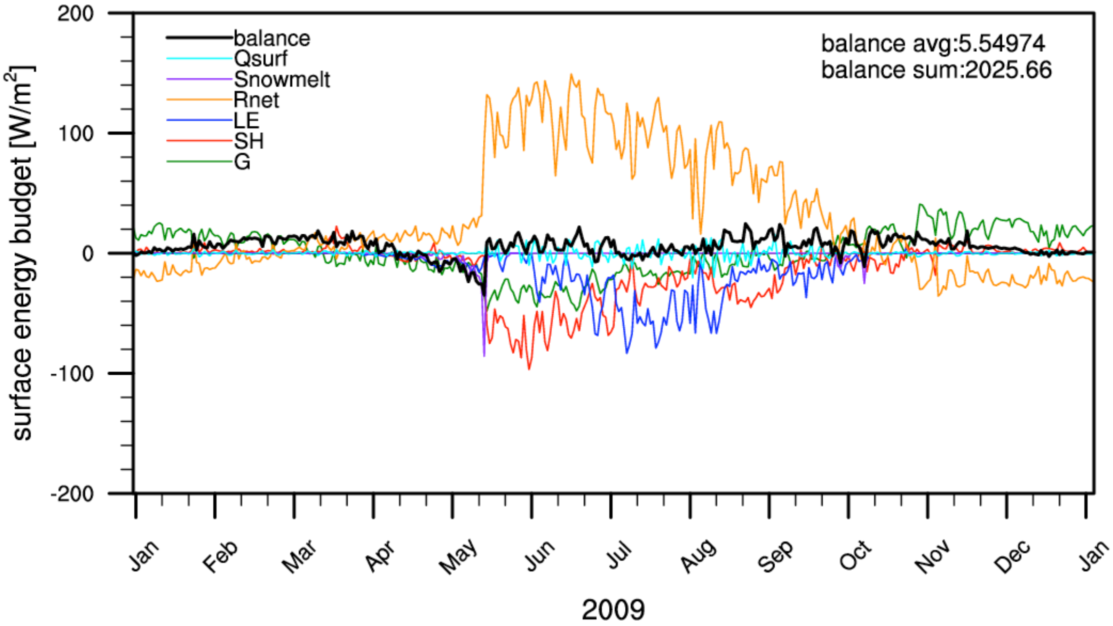

Figure6: Surface energy balance terms at Siberian grid cell for 2009

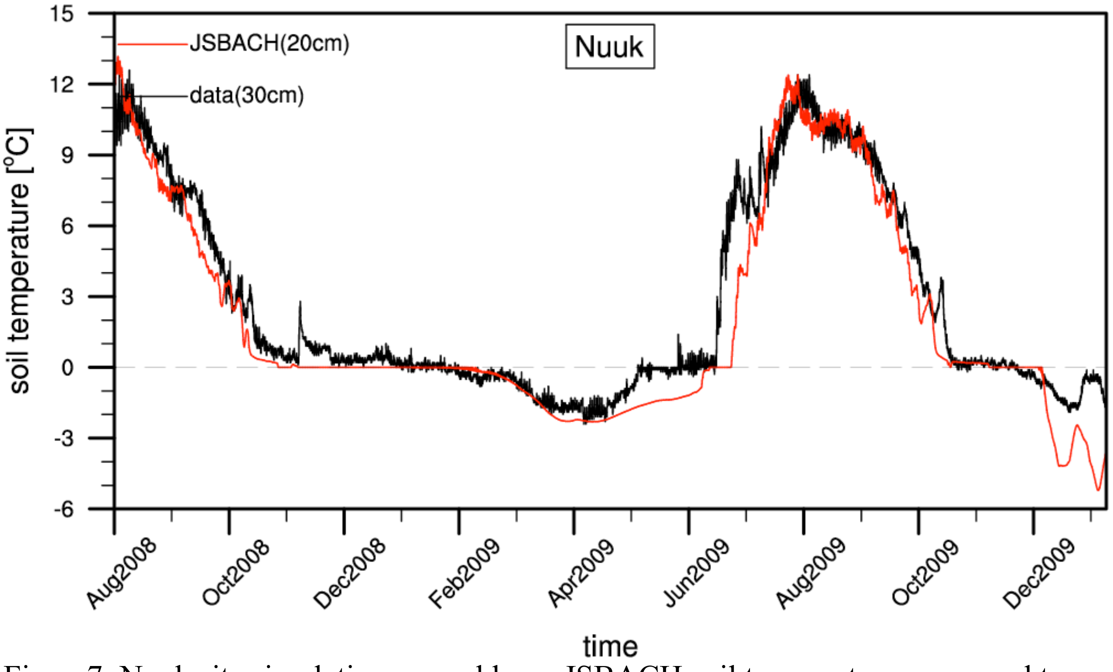

Figure7: Nuuk site simulation second layer JSBACH soil temperature compared to observed soil temperature.

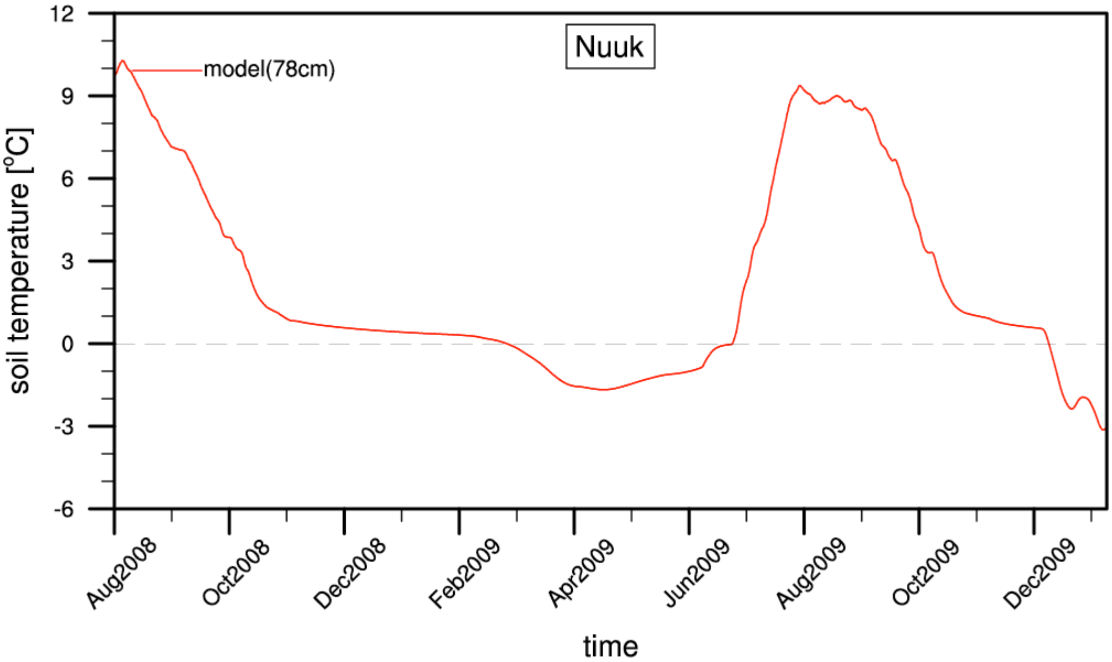

Figure8: Nuuk site simulation third layer JSBACH soil temperature.

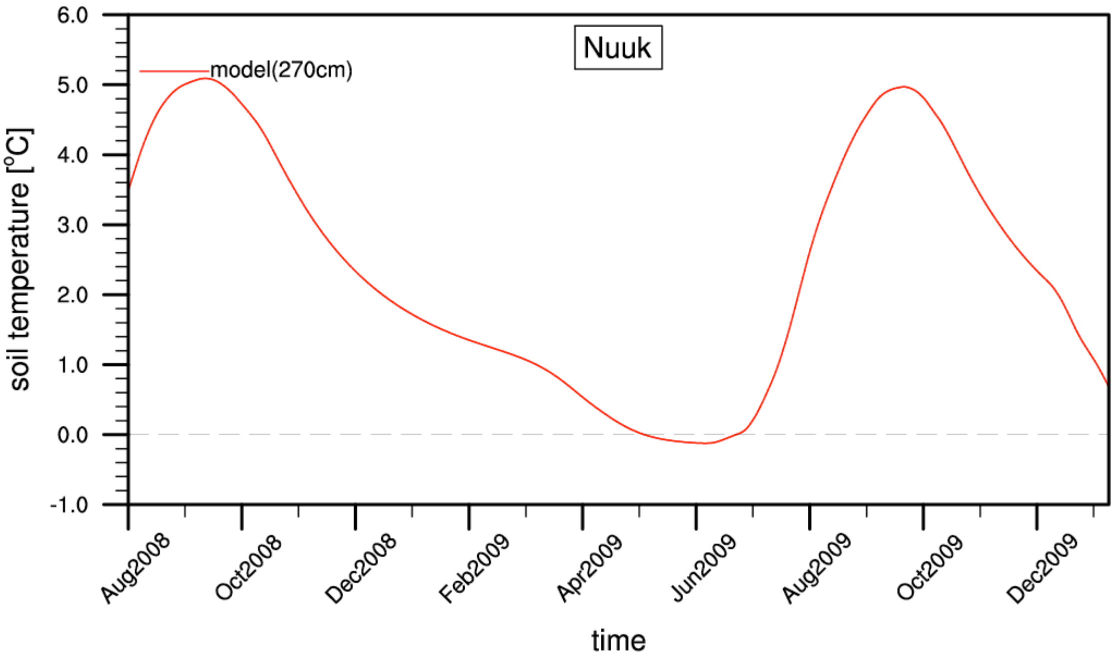

Figure9: Nuuk site simulation fourth layer JSBACH soil temperature.

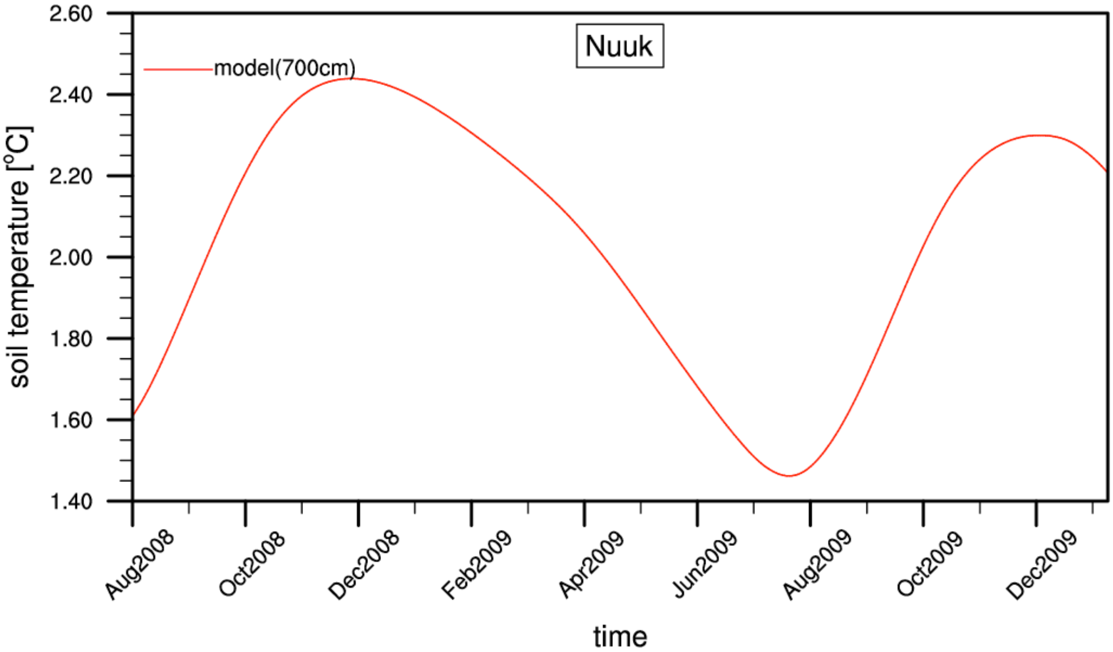

Figure10: Nuuk site simulation fifth layer JSBACH soil temperature.

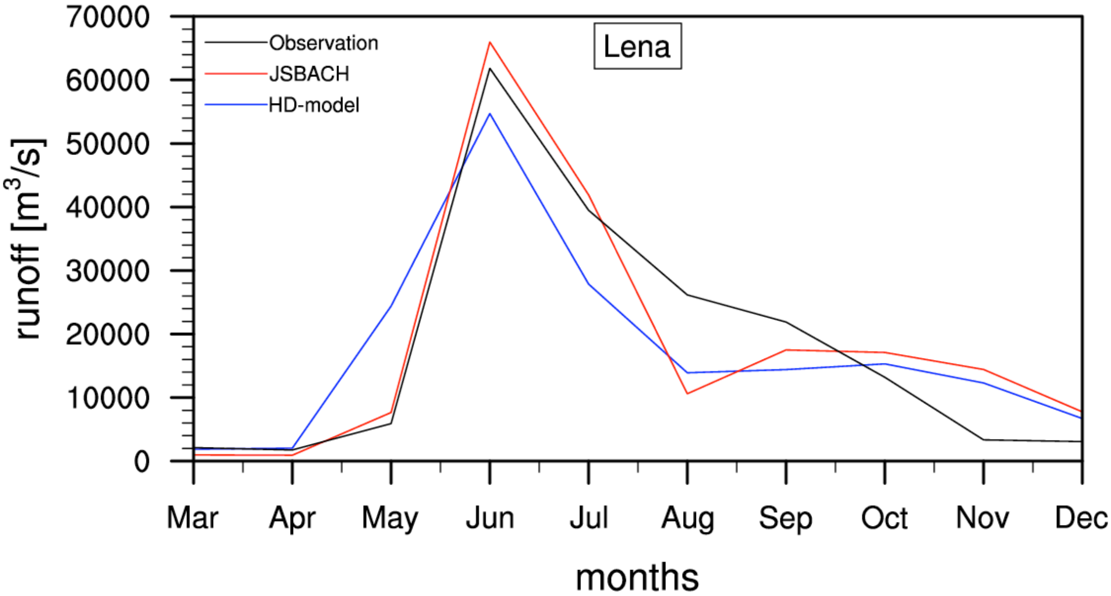

Figure 11: Monthly means of HD model run for Lena river runoff compared to previous JSBACH run and the observed runoff values. (Note that JSBACH run values are shifted by 2 months).

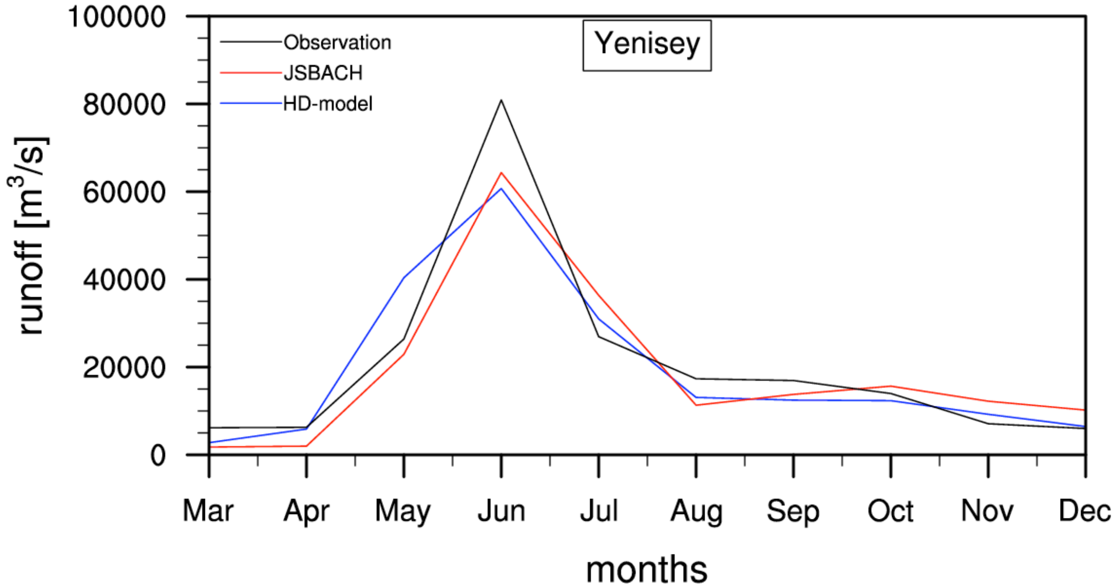

Figure12: Monthly means of HD model run for Yenisey river runoff compared to previous JSBACH run and the observed runoff values. (Note that JSBACH run values are shifted by 2 months).