We would like to thank the reviewer for the supportive comments and extensive efforts for the thorough analysis of our paper. We carefully checked all the points and tried to address all the questions and suggestions. Please find our comments (all in red color) below and the supplementary figures attached.

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.

Our aim in the submitted manuscript was to show the performance of the current JSBACH version, not to show the importance of the several improvements, as they are mostly standard for incorporating freezing/thawing schemes in land-surface models. Furthermore, assessing the improvement of adding separate snow layers compared to the previous method of snow occupying soil layers, is difficult since there won't be a logical comparison method: with the current version the 1st soil layer temperature would have to be compared to the old version's first soil layer that is not occupied with snow; but this could mean that a 6cm thick layer is compared to a possibly 90cm thick layer. That would lead to the uncertainty of comparing the temperatures of different layers with different heat transfer coefficients (heat capacity / conductivity / distance between layer midpoints…) etc.

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.

Not including a hydrological effect of the moss layer on the heat balance is indeed a substantial simplification, which is due to the technical limitations arising from coupling the 5 layer soil thermal module with the 5 layer hydrological layers and in turn having less flexibility in modifying the hydrological scheme to account for the moss layer. Such further improvements

are planned for the near future. However, we have added a model run for Nuuk site without any moss layer at the end of this document for comparison. The improvements from the moss insulation can be seen in the topsoil temperatures in figures 1-2.
Since this parameterization was the first step towards a complex organic layer/moss cover in JSBACH, we chose a simplistic approach to have just the minimal effect of thermal insulation from the moss cover. That was the reason of choosing a uniform moss cover over the entire domain. Although yet lacking the spatial variability and a real process-oriented way of representation, we do believe our method is still a valuable addition to a global scale model like JSBACH.
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.

Abstract shortened accordingly.

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.

Ciais et al. 2011 is replaced with Zimov et al., 2006.

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.

New reference added to complement the statement: Schirrmeister et al., 2013.

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

Corrected.

p. 2657 l 23: again, reference somehow unappropriate. 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.

Reference moved to the next sentence as suggested.

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.

Sentence changed as: "*At present, most of the global models include basic processes related to permafrost regions, e.g. latent heat release/consumption from*

the phase change of soil water, organic matter decomposition at freezing conditions, methanogenesis and methane related processes."
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.
Additional citations are added: Kelly et al., 1968; Goodrich, 1982; Groffman et al., 2006.

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)
The model description section is extended as: *"The moss cover above soil affects the soil heat transfer through thermal and hydrological insulation depending on the thickness and wetness of the moss. Spatial pattern of moss cover adds to the heterogeneity of soil thermal dynamics in the Arctic. To have the first step and represent such complexity, a simplified moss cover approach is chosen. As a full coupling of the thermal and hydraulic properties of such a moss layer in thermal and hydrological modules of JSBACH would be beyond the scope of this paper, a uniform moss cover was assumed for the entire domain without its hydrological effects"*
• 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?
The phase change routine is executed for each layer separately. First the heat transfer scheme calculates the layer temperature, then the phase change updates the temperature of this layer (depending on the latent energy used/received), then the next layer calculation starts. So there is no third call to the thermal scheme.
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?
The model description part is extended as:
"JSBACH mainly uses the physics package of ECHAM5 (Roeckner et al. 2003). This comprises the separation of rainfall and snowmelt into surface runoff and infiltration and the calculation of lateral drainage following the Arno scheme (Dümenil and Todini 1992). A new soil hydrology scheme (Hagemann and Stacke, 2013, in preparation) has been implemented into JSBACH that uses the same five-layer structure (see Fig. 1) as the thermal module and calculates soil water transport by using the one-dimensional Richards' equation (Richards, 1931) shown in Eq. (2). Here, the local change rate of moisture $\partial\theta/\partial t$ is related to vertical diffusion (first term on the right side of Eq. 2) and percolation by gravitational drainage of water (second term). Both processes are considered separately. Percolation is calculated following the Van Genuchten (1980) method and the diffusion is calculated using the Richtmyer and Morton (1967) diffusion scheme.

For the latter, the soil water diffusivity D of each layer is parameterized following Clapp and Hornberger (1978).
The soil water content Wi may be greater than 0 for each layer above the bedrock as there is no water available for the land surface scheme below the bedrock. Consequently, horizontal drainage (ECHAM4 formulation following Dümenil and Todini, 1992) may occur only from those layers above the bedrock. The formulation has been slightly modified as now drainage may only occur if the soil moisture is above the wilting point. Note that the previous bucket soil moisture now corresponds to the root zone soil moisture. The associated rooting depth determines the depth from where transpiration may occur. Bare Soil Evaporation is occurring only from the most upper layer."

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?
We agree that this sentence was not completely right. So it is rephrased as: *"Each layer field capacity is updated with the corresponding layer's ice content that is created or melted in the same timestep. This allows for a more realistic water transport within the frozen layers."*

p2662 l 10/12: indexing issues for θwmax between both expressions
Thanks, now corrected.

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.
Sentence rephrased as: "*Soil heat transfer is coupled with the hydrological scheme through the phase change process as well as two parameters, the volumetric heat capacity (c) and the soil heat conductivity (λ) in Eq. (1)."*

• about the snow scheme : what happens when snow depth is less than 20 cm and not an exact multiple of 5 cm ?
If they exist, the first 4 snow layers are always 5 cm thick. Only the 5th snow layer can be more than 5cm (unlimited in size). This approach was chosen to maintain the numerical stability during rapid changes in the snow cover. The uncertainty of representing 5 cm snow layers is assumed to be negligible when compared to having no layered snow scheme.

P2665 l 2: which set of soil parameters do you use?
A parameter table for Nuuk site simulation is added to the manuscript. Please see Table1 in this document.

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.
Described as: *"For the meteorological variables, 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."

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.
The permafrost condition for each gridbox was calculated with regards to the soil temperature only. For all of the 5 soil layers the temperatures are checked if any of the layers are staying below 0 degree for at least 2 years.

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?
We agree that an average would be better and updated the figure using the suggested 1980-1990 average. However, differences to the 1990 map are negligible.

• 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?)
A simple linear piecewise interpolation from 5 layers to 200 nodes was used to create the finer vertical profile of soil temperature. This information is added to the manuscript.

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?
Using all the years in the scatter plot was not improving or changing our conclusion that JSBACH is overestimating the active layer thickness. That's why we chose to show the simpler, time averaged plot. Please see Figure 3 in this document comparing each year separately.

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.
Unfortunately this information is not available. That's why we couldn't give more details about the data product. We just assumed it's the depth of no seasonal temperature change, thus our last model layer is the logical choice to compare with that.

3. Results and discussion 3.1. Nuuk
p. 2669 l23 to p. 2670 l: too redundant with the introduction.

Section shortened as: "*It is seen from Nuuk site level comparisons that winter soil temperature do not drop as low as might be expected due to atmospheric conditions alone. Even when the air temperature is minimal in high winter (ca. –20°C, not shown), soil keeps a rather warm temperature profile (ca. –3 °C, Fig. 2) as long as snow exists on top.*"

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...
Section extended as: "…hence, late winter snow has less insulating effect than early winter snowpack, allowing for stronger coupling between air and soil temperature towards the end of the season. There are also the possible effects of rainwater or meltwater within the snowpack. 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. These dynamics can explain the mismatch in simulated versus observed springtime soil temperature in the site level simulations. Without dynamically changing snow properties and lack of these snow specific processes, our model cannot correctly represent the lower spring insulation and keeps a colder soil temperature profile."

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).
Please see the changes mentioned above. Section rephrased to mention other possible processes.

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

3.2. Circum-Arctic validation
L23 “favouring northern slopes”: this is really interesting to everyone using these data. Do you have any reference on that?
Unfortunately we do not have any reference for that. Now the sentence is deleted.

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?
Underestimating rich ice content in the some regions can definitely be another possibility explaining the ALT underestimation. At present, we do not have any information about the ice content at CALM sites but would like to include such information into the discussion. Additionally, as seen from the simulation results without moss representation in Figure1-2, moss cover does decrease the summer temperatures and should be affecting ALT.

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 Iakutia but the cold bias is less strong there. Some studies mentioned critical snow underestimation by atmospheric forcing datasets in Iakutia, 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.
We fully agree with the forcing data uncertainty. In particular snowfall underestimation or temperature overestimation during early winter leading to melting in the early snow period can lead to a substantial underestimation of snow depth hence an underestimation of insulation hence a cold bias. Future studies are suggested to look specifically at this issue.

- 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.
We have seen how the confusion aroused. By higher insulation, the aim was to address the winter/spring time insulation (that is added to the manuscript now), which actually created warmer topsoil temperatures. And we agree that summer temperatures are actually cooled down. However we wanted to focus on the spring time since there is the combined effect of snow melt and change in moss insulation due to increased wetness.
- Thick ice overburden exists in coastal area and may explain your ALT overestimation in the model.
Thanks for the supportive comment. Information added as*: “The mismatches at the coast can be due to the thick ice overburden in those areas, which are not represented by JSBACH.”*

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?

See more description about the hydrology scheme above.

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

We added the correlation coefficients as suggested.

- 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?

This is a very good point and we do not have any answer at the moment. Our model is clearly not useful to address such question, e.g. glacier mass is not represented in the current scheme. In addition, we think that human influence on Yenisey runoff needs to be taken into account when studying trends and variability of runoff in more detail. All that is beyond the scope of this paper but would clearly be an interesting study.

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. Konig 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).

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 (-)

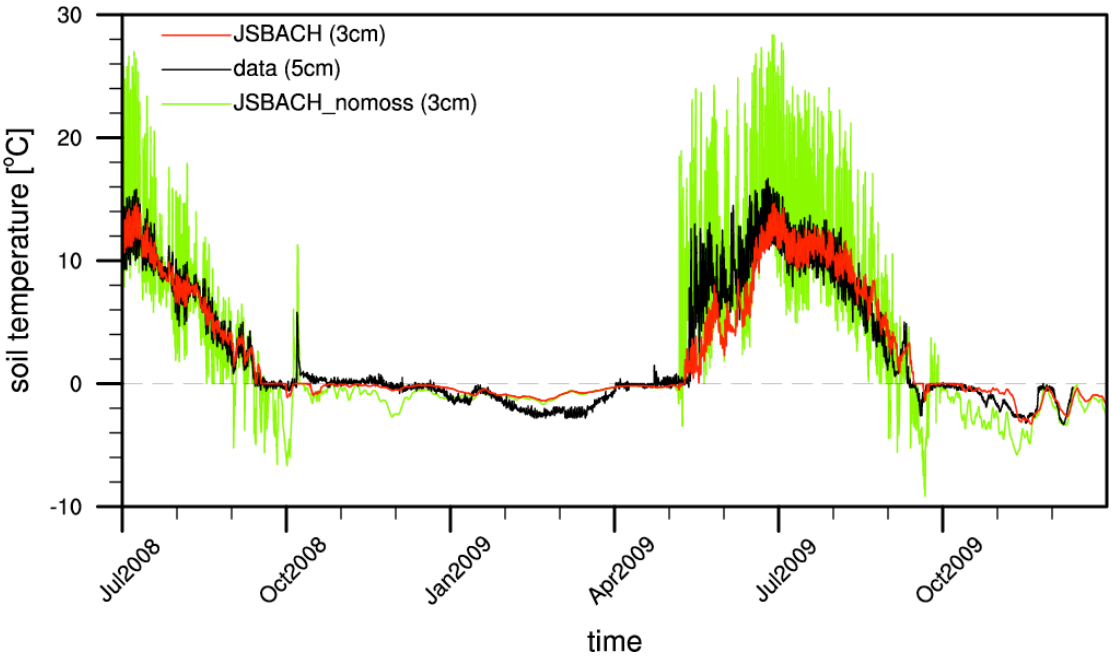

Figure1: Soil temperature comparison from 1st JSBACH layer with/without moss cover and observed data.

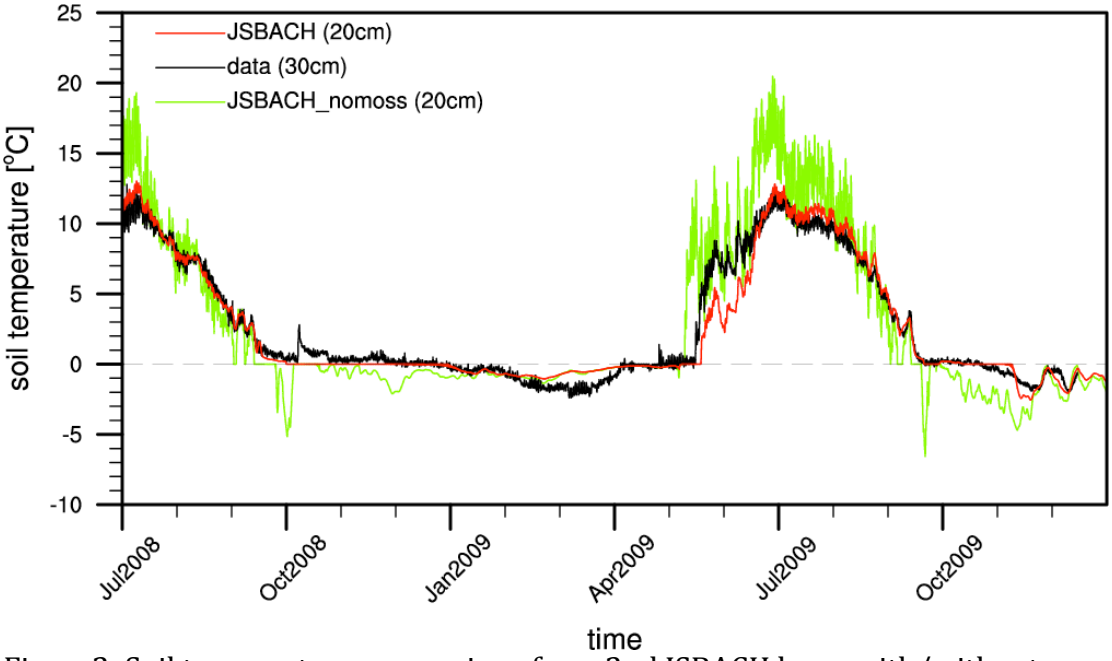

Figure2: Soil temperature comparison from 2nd JSBACH layer with/without moss cover and observed data.

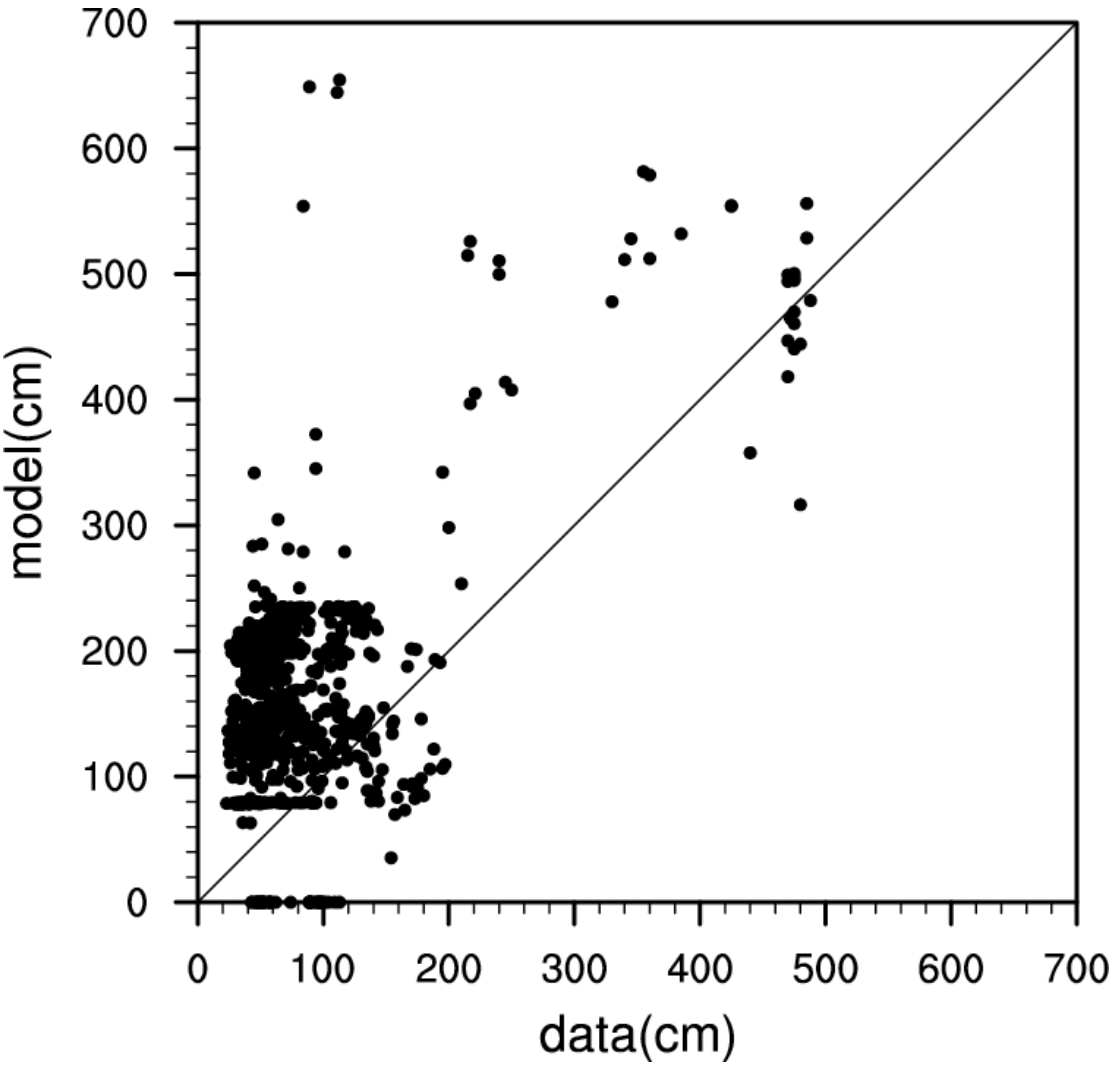

Figure3: Scatter plot of the observed active layer thickness (ALT) from the CALM network versus the JSBACH results