Interactive comment on “Hypersensitivity of glacial temperatures in Siberia” by Pepijn Bakker et al.
Anonymous Referee #1

Bakker et al. look to understand the mechanisms responsible for Siberian climate at the LGM. To do so, they use a combination of PMIP2/3 simulations and CESM1 sensitivity tests. The authors find that the Siberian region has a large temperature and precipitation spread among models. Using their CESM1 sensitivity tests, Bakker et al. explore the sensitivity of the Siberian region to model physics, ice sheet configuration, and vegetation response. They find that the Siberian temperature response is most significantly influenced by the vegetation, especially when using CAM5, but ice sheet geometry and model physics are also important. Overall, this is a nice study that I believe will be a valuable contribution to understanding climate in a largely overlooked region at the LGM. However, I have a few questions about the model configurations and would like a bit more detailed exploration of mechanisms before publication.
We thank the reviewer for the kind words and for having a critical look at the manuscript.

Major Comments:
Additional information about the model setup is required. How was the original LGM simulation, from which these experiments were branched, configured? This is important, because as the authors find, the climate produced by CAM4 and CAM5 can be quite different. Therefore, despite branching from a previous run, I am not convinced that 200 years of spin up is sufficient. Including top-of-atmosphere energy imbalance would provide a first order estimate of how close these simulations are to equilibrium.
The LGM simulation from which we branched of was run with CAM5 and not including CN-dynamics (Carbon-Nitrogen-Dynamics). This simulation was run for a long time (>1000 years) and was very close to equilibrium, also shown by the TOA imbalance of -0.023Wm^{-2}. The relatively short, 200 year, LGM simulation with different model setup resulted in somewhat larger TOA imbalances for the simulations including CN-dynamics (-0.1Wm^{-2} for using CAM4 and -0.185Wm^{-2} using CAM5), however, we deem them sufficiently small, especially considering that the TOA imbalance for the corresponding PI simulations is of similar magnitude (-0.106Wm^{-2} for using CAM4 and -0.117Wm^{-2} using CAM5). The TOA imbalance resulting from the switch from CAM5 to CAM4 (without CN-dynamics) is negligible (0.014 for using CAM4 and -0.023Wm^{-2} using CAM5).

Also, are 30 year averages enough to produce true climatologies in this region? There are a lot of decadal oscillations that can impact climate for long periods (e.g. Deser et al. 2012). I don’t think that this will significantly change results, but I do recommend a quick comparison with a longer average, such as 50 years, to make sure.
Working with 50-year averages rather than 30-year averages makes very little difference indeed.

Finally, how were the CLM4 cases with “interactive vegetation” spun up? If not spun-up properly, it can take hundreds of years for the carbon cycle to come into equilibrium, which could impact your vegetation distribution.
The simulation with CN-dynamics were spun up using pre-industrial values. For the regions that have become land under LGM sea-level fall we used a nearest neighbor approach to obtain initial conditions. Indeed, especially the soil carbon pool takes centuries to equilibrate and therefore there are still trends in the different CN-pools. However, the trends in the local (Siberian) vegetation carbon pool, the most relevant for our analysis, is less than 2% of the total PI-to-LGM change in carbon pool over a 50-year period (both in CAM4 and CAM5). We deem this relatively small, but agree that a statement should be included in the manuscript to mention these trends. We added the following to the methodology section “Carbon pools in

the litter and soils take centuries to equilibrate. However, we find that the trends are sufficiently small after 200 years to perform a robust analysis of the surface climate (changes in Siberian (global) vegetation carbon pools over the years 150-200 are less than 2\% (0.6\%) of the total PI-to-LGM change)."

Limiting the analyses to JJA limits the mechanistic understanding. Are you sure that the summer changes are mainly a result of summer processes? Also, a more rigorous exploration of the local radiative effects versus heat transport would be useful. For example, albedo and cloud radiative forcings would be more insightful than snow and cloud cover.
Thank you for this interesting remark. We have performed an additional analysis looking at the seasonal cycle of PMIP multi-model variability for Siberian temperatures, cloud cover and snow cover. For all three variables it is clear that the large increase in differences between the various PMIP models going from PI to LGM is a summer feature. In the other seasons, temperature variance also increases somewhat, but cloud cover variance doesn't change while snow cover variance is in fact decreased.
In the updated manuscript we will include these figures in the supplement.

The authors argue for the necessity of additional CESM simulations based in part on the number of variables available for analysis from the PMIP simulations, but proceed to explore only basic outputs from their CESM experiments. Additional analyses to explore why the temperature changes in CESM with different configurations is warranted. At a minimum, areas of perennial snow cover are worth including. What about sea ice? Maybe a PDD and/or energy balance calculation would be insightful. With additional information, the authors could make a much more significant statement about which simulations would produce an ice sheet in Siberia at the LGM. From there, additional model assessment with proxies is possible. Are the models that produce a Siberian ice sheet too cold (probably) or too wet, etc. . .? What does this suggest about Siberian climate at the LGM?
Our main reason to include CESM results is to be able to show which differences between PMIP simulations can potentially lead to large differences in Siberian JJA temperatures (ice sheets, atmopheric model and vegetation feedback). Such a separation of factors is not possible for the PMIP ensemble. A secondary reason is that for PMIP simulations we do not have all the output variables available. This is less of an issue now that we have found that for all but one of the PMIP simulations the geopotential height variable is available. Still, an important variable in our analysis, meridional atmospheric heat transport, is only available in the CESM simulations. We do not think that a more detailed analysis of the CESM results should be part of this manuscript.
In a new table we will include CESM information on Siberian temperatures, minimum snow cover, cloud cover, precipitation and sea-level pressure.

Specific:
P1 Line 20: Further south than 50 ◦ N in many locations in North America.
We have changed the line to read "down to ~40 ◦ N in some areas.

P1 Line 21: Much of Alaska also did not have ice.
Indeed much of Alaska was also ice free during the LGM. We've changed the line to read "A notable exception was..."

P1 Line 25: Didn't some of these modeling studies limit their ice domain to exclude Siberia? Double check.
The work by Abe-Ouchi et al. (2013) was a free running modelling experiment that did not exclude Siberia from their domain (in fact they do simulate a Siberian ice sheet when applying an additional cooling factor). The other two studies are combined model-data driven

reconstructions and as such they use the absence of an ice sheet in reconstructions as target in their modelling exercise.

P2 Line 2: Citation for the sea level statement?
We deem the notion that sea level was globally lower during the LGM as common knowledge and as such a reference is not needed here.

P2 Line 30: This dust feedback is mentioned in earlier (e.g. Mahowald et al., 1999; Ganopolski et al., 2010). What about the direct radiative effect of dust (e.g. Schneider et al., 2006)?
Thanks for pointing this out. We have added a reference to Mahowald et al. 1999 on line 27. We did not add it at line 30 as those are studies specifically discuss the evolution of the Siberian ice sheet in relation with LGM dust deposition. We prefer not to include a reference to Schneider et al. (2006) since they do not specifically discuss Siberia.

P4 Line 1: Link is messed up.
Thank you for pointing this out. I has been corrected.

P4 Line 10: Should be 1.9x2.5 ◦
Indeed that is more specific. We have adjust it.

P5 Line 16: Shouldn't this citation be for an ice sheet reconstruction paper? Peltier et al. (2015) maybe?
This reference has been updated to Ivanovic et al. (2016).

P5 Line 34: Not sure that ensemble is the correct word.
We use the word ensemble here to refer to a group of experiments.

P6 Line 5: Need to spell out LGM_CAM5_noVeg first.
Thank you for pointing this out. We have removed the acronym.

P6 Line 14: Why not look at the snow cover in the model?
We agree that this line is confusing and have therefor removed it. In the manuscript we do look at snow cover.

P6 Line 4: Cloud radiative forcing would be more insightful.
Cloud radiative forcing is unfortunately not available for all PMIP2 and PMIP3 LGM simulations.

P6 Line 13: Did you analyze CCSM3, as used in Liakka et al. (2016), to better understand this discrepancy? Could use a bit of additional discussion.
We did not analyze their results and think understanding these differences is not the goal of this manuscript because the reasons in the 17 different models are likely manifold.

Figure 1: Make the continental outlines thicker.
Figure has been updated

Figure 2: Darker green would make it easier to see.
Thank you for the suggestion. We decided to keep it as it is.

Figure 3: Add winds and/or height anomalies to better highlight the circulation changes.

The contents of this figure have been changed from sea-level pressure to geopotential height anomalies at 500hPa to provide a much more direct indication of large-scale atmospheric circulation changes.

P10 Line 16: Why not plot the same variables as in the PMIP runs with CESM?
By showing geopotential height anomalies at 500hPa in figure 3 for the PMIP models and by adding summary information on CESM-based Siberian temperatures, minimum snow cover, cloud cover, precipitation and sea-level pressure in a new table, we now effectively show the same variables for PMIP and CESM results as long as they are available.

P10 Line 22: How is surface roughness over the ice sheets configured? The results of Brady et al. (2013) suggest that this is important.
This is indeed one of those things that are uncertain for LGM simulations. We have chosen a simplified approach assigning a constant value similar to other areas that are ice covered at present day, but we agree that this is yet another mechanism that could impact temperatures since the sensitivity in the northeast Siberia to perturbations of the large-scale circulation is so large.

P10 Line 8-2?: It would be great to plot some of the differences mentioned.
We are not entirely sure what the reviewer is referring to in this comment, but assuming it is on the differences between CAM4 and CAM5, we would argue that such an analysis should really be performed by the experts who know all the details of the two atmospheric models.

P11 Line 20: How do you define vegetation density?
We use the term density here to describe in general how much vegetation there is per unit area, which in the model is mainly determined by the combination of the leaf area index and the stem area index.

P11 Line 22: This vegetation feedback has been found to be important for Arctic climate before (e.g. Jahn et al., 2005; Tabor et al., 2014).
Thanks for pointing this out. On page 11 we have added a line acknowledging this "Previous studies also found a leading role for vegetation feedbacks in LGM Arctic temperatures (Jahn et al., 2005)."

Figure 5 A: There must be a strong local feedback in Siberia. Maybe plot snow cover or albedo?
Indeed there are multiple strong local feedbacks as described throughout the manuscript (snow cover, cloud cover changes etc). In a new table we have included information for the various CESM simulations on temperature, precipitation, cloud cover, snow cover and sea-level pressure in the Siberian target region. Indeed temperature and the summer snow fraction are related, showing a local feedback.

P11 Line 23: Does this mean the vegetation dies?
The different vegetation zones move southward, including the zone that has very little vegetation cover. In CESM the plant functional types are prescribed and thus not changing between the different experiments. They prescribe a mixture of different PFT's in every grid cell and the apparent southward shift of the vegetation zones is thus a change in the dominance of certain PFT's within the individual grid cells. We currently can't tell how the results would change if the PFT's would be interactively calculated using a full dynamic vegetation model.

P11 Line 26: Does Lawrence et al. (2011) discuss this Arctic LAI issue?

Thanks for pointing this out. Indeed Lawrence et al. (2011) also discuss that in CLM4 Siberian surface and soil temperatures are biased low (compared to observations) while CLM3 they where biased somewhat high. We added a short statement in the manuscript "….and is in line with the cold bias in Siberian surface temperatures described by Lawrence et al. (2011)."

Figure 6: How were your PI runs configured for your LGM-PI anomalies? The vectors are very hard to see in panel C. Please change the color.

We do not understand the first part of this question. What is meant with configured in this context? We have made the vectors more clear.

Figure 7: Extend the temperature range in panel B.

Thanks for pointing this out. We have updated the figure to be more readable.