

## ***Interactive comment on “The impact of the North American ice sheet on the evolution of the Eurasian ice sheet during the last glacial cycle” by J. Liakka et al.***

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### ***Referee comment 1***

#### *General remarks*

*The objective of the authors is to model the impact of the Laurentide ice sheet on the evolution of the Eurasian ice sheet during several stages of the last glacial. Ideally, one would employ a fully coupled atmosphere-ocean-ice sheet model and make simulations over the full last glacial cycle to account for the memory of the climate system and to incorporate all feedbacks between the atmosphere, ocean and ice sheets. However,*

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*the existing comprehensive models are still rather expensive to use, implying that it is not yet feasible to perform such long experiments. One solution is to apply intermediate complexity models (e.g. Ganopolski et al. 2010). Liakka and colleagues have used an alternative approach, by running a chain of models sequentially, and by using the results of the previous step as input. The first step in this chain is the LGM simulation performed with the CCSM3 AOGCM by Brandefelt & Otto-Bliesner (2009). Secondly, these CCSM3 simulations were utilized to derive ocean heat transport (OHT) representations for the LGM and the preindustrial era that were used as a boundary condition in experiments performed with the CAM3 atmospheric GCM coupled to a mixed layer ocean model. In addition, different ice sheet configurations for MIS5b, MIS4, and LGM, based on reconstructions by Kleman et al. (2013), were also employed as boundary conditions in these CAM3 experiments. Finally, the atmospheric fields from the CAM3 experiments were applied as forcings for MIS5b, MIS4 and LGM simulations performed with the SICOPOLIS ice sheet model. The analyses presented in the paper are mostly based on the CAM3 experiments with preindustrial OHT, because the authors argue that the CAM3 experiments with LGM OHT produced a too cold climate in the North Atlantic area when compared to proxy-based temperature reconstructions.*

*The main result of the presented model experiments is that the Eurasian ice sheet migrates westward in MIS4 and LGM due to the impact of the growing Laurentide ice sheet on the atmospheric circulation. This result appears to be robust under different experimental setups (preindustrial and LGM OHT). The westward migration of the Eurasian ice sheet in MIS4 and LGM is consistent with reconstructions. However, in the MIS5b experiments, no westward migration of the Eurasian ice sheet is simulated, in conflict with reconstructions. The authors explain this mismatch by suggesting that under MIS5b boundary conditions, the ice sheet is not in equilibrium with the climate.*

*This paper deals with an important topic and the results presented are in principle of interest to the readers of Climate of the Past. However, as detailed below, I am not convinced that the experimental setup is fully appropriate to make this analysis. In my*

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view, the main problem is that essentially all feedbacks between the ocean circulation and the ice sheet evolution are very poorly represented.

### **Reply from the authors**

We do not agree that our modeling approach implies that "all feedbacks between the ocean circulation and ice sheet evolution are very poorly represented" as suggested by the referee. A slab ocean model omits the dynamic feedback but still retains the thermodynamic feedback between the ocean and the atmosphere (and thus the ice sheet evolution). Hence, any change in the atmospheric temperature would also induce changes in the SST, which in turn feed back onto the atmosphere (see also our next reply for more on the slab ocean model).

### **Referee comment 2**

#### *Main comments*

– *The presented analysis for MIS5b, MIS4 and LGM is mainly based on experiments with a preindustrial OHT. The authors argue that the LGM OHT was inducing too cold conditions in the Atlantic Ocean, with a too extensive sea ice cover. Ideally, one would use specific OHT representations from experiments specifically designed for MIS5b, MIS4 and LGM. In my view, using the preindustrial OHT is really problematic, and is very likely to produce results that are not meaningful for the last glacial conditions, as it is very clear from palaeoceanographic evidence that the LGM North Atlantic Ocean was substantially colder than during the preindustrial era. I would argue that it makes much more sense to use the LGM OHT. There is evidence that in the North Atlantic Ocean the sea ice cover was extending to at least 45N (e.g., Renssen & Vandenberghe, 2003). This would suggest that, at least for the LGM time slice, the results obtained with LGM OHT are more appropriate. I assume that the applied LGM OHT is based on the LGM2 state of Brandefelt & Otto-Bliesner (2009). I would argue that their LGM1 state would have been even more appropriate, as this state represents a stronger AMOC and less cold North Atlantic Ocean compared to the LGM2 state. In Otto-Bliesner et al.*

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*(2006), the simulated SSTs of LGM1 are compared to reconstructions, showing a good fit. I therefore strongly suggest repeating the analysis with CAM3 and SICOPOLIS with an OHT based on the LGM1 state.*

### **Reply from the authors**

There seems to be some confusion what using prescribed ocean heat transport (OHT) actually implies. In a slab ocean model the SST and sea ice are given by the surface energy balance as  $Cd(SST)/dt = F_{atm} + OHT$ , where  $F_{atm}$  is the energy flux from the atmosphere, and  $C$  the product of the water density, specific heat capacity and the mixed-layer depth. In this study we use two sets of OHT (PI and LGM) derived from equilibrated simulations with the fully-coupled model CCSM3, which uses the same atmospheric, land and sea-ice components as in the experiments presented here. Because OHT is prescribed, the change of SST (over the time scale defined by  $C$ ) is ultimately determined by  $F_{atm}$ . As a consequence, PI OHT does not necessarily yield PI SST (in fact, PI OHT yields PI SST only in the special case when  $F_{atm}$  attains PI values, thus when using PI boundary conditions). In glacial times the colder conditions typically leads to a reduction of  $F_{atm}$ , and hence to colder conditions even if we prescribe the same OHT. It is true that the North Atlantic ocean was substantially colder at the LGM than at present/pre-industrial but this is arguably also something that we capture in our simulations with PI OHT. Figure S1 (Fig. 1 in the supplement) shows the simulated SST anomalies in the North Atlantic compared to the MARGO proxy compilation. Using LGM OHT (panels c,d), the simulated SSTs are clearly colder than the proxy across the entire North Atlantic. However, even when using the warmer PI OHT, there are regions where the simulated SSTs are significantly colder than the proxy, particularly in the western Atlantic sector (panels a,b). In this region the simulated sea-ice margin extends to about 40°N (Fig S1a,b), thus even farther south than 45°N.

In summary, Fig. S1 illustrates that the SSTs using LGM OHT are too cold across the North Atlantic (compared to the proxy data) with a zonal sea-ice margin at around 40°N. To our knowledge, there is no geological evidence of a zonal sea-ice margin at those

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latitudes across the Atlantic. CLIMAP had such sea-ice margin but the reconstruction is considered problematic in many ways. More recent proxy data reconstructions suggests that the eastern sector of the North Atlantic (North Sea) was perennially ice free (Paul and Schäfer-Neth, 2003; Toracinta et al., 2004; De Vernal et al., 2005, 2006), i.e. structurally more similar to the LGM simulation with PI OHT. Because of this, we chose to use the PI OHT simulations as the "main" simulations in the manuscript, although we present results from both OHT parameterizations. This does not, however, mean that the simulated SSTs with PI OHT are in perfect match with proxy (and we certainly never claimed this to be the case). Note that the SST and sea-ice distribution in the LGM1 state in Brandefelt and Otto-Bliesner (2009) is not in perfect agreement with proxy data either (see discussion in Li and Battisti, 2008).

As we have already stated in the manuscript, it is perhaps better to view the PI and LGM OHT as two possible end-member representations during the last glacial cycle that bracket the uncertainty range of the CCSM3 model. In this context it is important to note that both simulations yield a global and annual mean cooling of approximately 5°C at the LGM compared to PI (Löfverström et al., 2014), thus within the range of the PMIP2 and PMIP3 coupled models (see e.g. Braconnot et al., 2007; Harrison et al., 2014; Löfverström et al., 2014). What the actual OHT was at LGM is not known as state-of-the-art atmosphere-ocean models exhibit various results; some suggest that the LGM Atlantic Meridional overturning circulation (AMOC) was stronger than at present, while other suggest a weaker LGM AMOC (e.g. Weber et al., 2007; Brandefelt and Otto-Bliesner, 2009; Brady et al., 2013). Another reason for using two distinctly different OHT representations in our study is that we do not only deal with LGM and PI time slices, but also with two "intermediate" ones (MIS5b and MIS4). For these intermediate time slices, it is difficult to say whether LGM or PI OHT is more appropriate as virtually nothing is known about the sea-surface conditions in these elusive times. Therefore it is important to use (at least) two significantly different OHT representations for those time slices in order to evaluate their impact on the atmospheric circulation.

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It is important to note that the main conclusion of the study is not very sensitive to the employed OHT. Regardless of OHT prescription (PI or LGM OHT), the climate response to the North American ice sheet is qualitatively very similar (compare Figs. 2, 3, 4 with S4, S5, S6; see also Löfverström et al., 2014). In other words, the stationary wave field induced by the North American ice sheet is not very sensitive to the employed OHT. As a result, the North American ice sheet induces a westward migration of the Eurasian ice sheet for MIS4 and LGM (Figs. 5 and 9) for both OHT representations, although less pronounced for LGM OHT. As stated in the Discussion section, the reduced westward migration in the LGM OHT simulations is attributed to the colder background climate and the non-linear nature of the PDD model; because larger parts of Eurasia are affected by colder (below freezing) temperatures in the LGM OHT simulation, the effect of the temperature anomalies on the ablation is reduced.

Since the stationary wave response in our experiment is not particularly sensitive to the employed OHT, we do not see the point of conducting an additional experiment using another OHT representation, such as the LGM1 OHT as suggested by the Referee. In addition, the LGM1 ocean state from Otto-Bliesner et al. (2006) is not in equilibrium (see Brandefelt and Otto-Bliesner, 2009, for details). Using a non-equilibrated ocean state to calculate the OHT to the slab ocean would be problematic, particularly since we only deal with equilibrated (steady-state) simulations in this manuscript.

With regard to this comment by the Referee, we will clarify some issues in the methodology section (particularly Section 2.1). In particular, we will stress a bit harder why we use two substantially different OHT representations, and that both representations yield a mean cooling at the LGM within the range of the PMIP2 and PMIP3 models. We will also add a sentence or two on why we use the LGM2 but not the non-equilibrated LGM1 state from Brandefelt and Otto-Bliesner (2009). In addition, to avoid confusion, we will clarify how the slab ocean model works; in particular we want to stress that PI OHT does not lead to PI SST and that the model ocean dynamics but has a thermodynamic interaction with the atmosphere.

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### **Referee comment 3**

– As noted, the LGM OHT used in the CAM3 experiments is derived from the CCSM3 simulations of the LGM climate. In my view, it is important to establish if the CCSM3 LGM climate is consistent with the LGM climate simulated by the CAM3 model. The atmospheric components in both models are basically the same (CAM3), but the two setups have different resolutions, very different ocean models and the simulations use different boundary conditions, e.g. the ice sheet configurations. If the climates are not consistent, I would argue that the CCSM3-derived LGM OHT should not be used in the CAM3 experiments.

### **Reply from the authors**

Yes, the large-scale features of our LGM simulation (with LGM OHT) is consistent with LGM2 state climate in Brandefelt and Otto-Bliesner (2009). The similarity is hinted in Fig. S1 which shows a zonal sea-ice margin in the North Atlantic at around 40°N, thus essentially identical to the LGM2 state sea-ice margin in Brandefelt and Otto-Bliesner (2009). Also features such as the global annual mean temperature and the equator-to-pole temperature gradient are very similar between the simulations (for details: see Löfverström et al., 2014). However, as the Referee points out, the boundary conditions used here and in Löfverström et al. (2014) are not identical to Brandefelt and Otto-Bliesner (2009). Perhaps most importantly, the simulations employed different representations of the LGM ice-sheet topography; here (and in Löfverström et al., 2014) we used the ice-sheet reconstructions from Kleman et al. (2013) whereas Brandefelt and Otto-Bliesner (2009) used the ICE-5G glacial orography (Peltier, 2004). The ICE-5G LGM reconstruction has a substantially higher ice dome ( 1000 m) in North America than in Kleman et al. (2013). Therefore one cannot not expect the responses to be completely identical between the simulations.

We will add a sentence to the methodology section and the figure caption of Fig. S1 stating that the climate in our LGM simulation (with LGM OHT) is consistent with Bran-

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defelt and Otto-Bliesner (2009).

### **Referee comment 4**

– If understand correctly Löfverström et al. (2014), a modern annual-mean mixed layer depth is applied in the slab ocean model to specify the ocean's heat capacity in all the glacial experiments used in the present study. Why was this done and what is the impact on the results? I propose to explain this in the methodology section.

### **Reply from the authors**

The mixed layer depth is important as it controls the response time of the ocean temperature to changes in the surface energy balance. The LGM winds are generally stronger than in the PI climate, especially in midlatitudes in the North Atlantic sector. However, it is not only the strength of the winds field that is influenced by the LGM boundary conditions, but also the orientation and spatial location of the circulation anomalies. Löfverström et al. (2014) found, in accordance with Li and Battisti (2008), that the LGM winter jet is stronger, more zonal and spatially confined compared to the PI climate. These changes in isolation yield a deeper mixed layer extending rather zonally across the North Atlantic basin. However, Brandefelt and Otto-Bliesner (2009) found that the LGM mixed layer depth in the North Atlantic actually decreases somewhat when the model equilibrates. No explanation for this result was provided, but it is likely due to the expansion of the sea-ice cover that reduces the mixing effect of the wind; figure 1 in Brandefelt and Otto-Bliesner (2009) shows a mixed layer depth in the GIN seas of about 100 m in the equilibrated LGM state, which is comparable to the PI counterpart.

We used the PI mixed layer depth because the LGM correspondence was not saved on the CCSM3 data server. It is not obvious how changes in the ocean mixed layer depth would influence our results, but according to the results presented in Brandefelt and Otto-Bliesner (2009), the largest changes in the North Atlantic region (of order 50-100 m) are found where the sea-ice cover is perennial in both the LGM simulation and in proxy data, which suggests that the PI mixed layer depth works equally well

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in these regions as there is virtually no heat exchange between the atmosphere and ocean. The changes elsewhere are smaller (of order 10 m), suggesting that the effect of changes in the mixed layer depth likely is small.

**Referee comment 5**

– *In my view Section 4 could be improved by discussing the obtained results relative to previous studies on the evaluation of ice sheets, for instance Ganopolski et al. 2010 and Beghin et al. 2014. Are the results consistent? If not, what is the reason?*

**Reply from the authors**

We will follow your suggestion and extend the first part of the Discussion section to include a comparison with the above mentioned studies.

**Referee comment 6**

*Minor comments*

– *Figures 2, 3, 4: I wonder what the statistical significance is of the simulated anomalies. I suggest to perform a test (e.g. t-test for temperature) and to show only results that are statistically significant.*

**Reply from the authors**

We agree that introducing a significance test to our figures will increase the robustness of our conclusions. We will follow your suggestion and use a Students t-test (at 95% confidence level) to test the statistical significance of the simulated anomalies in panels (d), (f) and (g) of Figs. 2, 3 and 4, (and panels b, d and f of Figs. S4, S5 and S6). Preliminary tests, however, indicate that essentially all the colored shading (but not necessarily the white areas) in those plots are statistically significant, so we do not expect the plots to look much different. We will, however, add a short message in the captures to those figures stating that the colored shading depicts only the statistically significant values.

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**Referee comment 7**

– *Page 5205, line 6. "The stadials are referred to as the Marine Isotope Stages (MIS) 5d (106-115 kyrs BP), 5b (85-93 kyrs BP), 4 (60-74 kyrs BP) and 2 (12-24 kyrs BP)". This sentence is confusing, as the meaning of stadials is not identical to that of Marine Isotope Stages. For instance, MIS4 includes 3 stadials according to the Greenland ice core record (e.g. Rasmussen et al. 2014) and MIS3 also includes stadials. So I suggest rephrasing.*

**Reply from the authors**

Thanks for pointing it out. We will rephrase.

**Referee comment 8**

– *Section 2.1: I suggest including more information on the experimental setup, particularly the CAM3 experiments. For instance, for how many years have the CAM3 experiments been run? I suggest including a table with all boundary conditions and forcings. A flow diagram that explains the full experimental setup would also help.*

**Reply from the authors**

We will add information about for how many years the CAM3 simulations were run (60 years), and how many of those we used to create the climatologies (25 years). We will be more informative regarding the forcings and boundary conditions (BCs) in Section 2.1. Perhaps more importantly, we will state more explicitly that the forcing protocol and the BCs are identical to Löffverström et al. (2014).

**Referee comment 9**

– *Page 5210, line 26: To estimate the fractions of solid and liquid precipitation, a limiting temperature is set. If the temperature is less than -10C, all precipitation is solid, and if it is above 7C, all precipitation is liquid. Between these temperatures, there are varying fractions solid and liquid precipitation. I was wondering what the rationale is for using*

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-10C and 7C? On what are these values based?

#### **Reply from the authors**

See also our reply to Referee 2. The temperature limits for liquid and solid precipitation are based on Marsiat (1994). These values are default in SICOPOLIS (see e.g. Greve et al., 1999, 2011), but is used also in other ice sheet models (e.g. Langen et al., 2012). We will add the Marsiat reference to the ice-sheet model section.

#### **Referee comment 10**

– Page 5212, 2nd paragraph, starting line 11: Please clarify what experiments you compare here. Only the EA-only simulations, or also the fullGlacial runs?

#### **Reply from the authors**

Thanks for pointing it out. It refers to the EAonly simulation. We will clarify this in the text.

#### **Referee comment 11**

– Figure 6: Is the longitude for the Eurasian ice sheet mass centre for the EAonly experiment on MIS4 consistent with Figure 5c? Visual inspection of the latter figure suggests that the centre of mass in the Barents Sea at  $\sim 30^\circ$  E, while Figure 6 suggests  $\sim 55^\circ$  E. How is the centre of mass defined?

#### **Reply from the authors**

Yes, it is consistent. The longitude of the center of mass ( $\lambda_c$ ) is defined over the entire Eurasian continent. This implies that the relatively small ice sheet in eastern Siberia in the EAonly simulation also contributes by increasing  $\lambda_c$ . Hence,  $\lambda_c$  should be interpreted as the "average longitude of ice in Eurasia" rather than the "average longitude of the largest ice sheet in Eurasia". We will clarify this in the text.

#### **Referee comment 12**

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– Page 5222, line 8: "between the MIS4 and LGM extents and the proxy suggests..." I propose to replace "proxy" by "proxies"

#### **Reply from the authors**

Fair enough. We will change "proxy suggests" to "proxies suggest".

#### **Referee comment 13**

– Page 5223, line 17: should be "yields cooler summer temperatures" - Page 5223, line 22: should be "an equivalent"

#### **Reply from the authors**

Thanks for pointing it out. We will change this.

#### **Referee comment 14**

– Page 5224, line 13: should be "our results are"

#### **Reply from the authors**

Thanks again. We will change this too.

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