Response to comments by anonymous referee #2 (referee comments in bold font)

The authors once again thank the referee for the review that will result in significant improvements of this manuscript. We have carefully considered all comments and addressed them accordingly in the manuscript. Responses to the individual comments are given below.

General information. Most of the figures have been revised to enhance the continents, latitude and longitude lines with labels and also the contours of the ice sheets. We have also added panels of the present day (reanalysis) to validate the models capability in simulating the interglacial climate.

To aid the reader and support the conclusions we draw in the manuscript an extensive "supplementary material" is provided where we show the evolution of the eddy geopotential field in both summer (JJA) and winter (DJF) for all simulated cases using both interglacial and LGM ocean heat flux convergence. We also show the evolution of the sea-ice margin and upper tropospheric zonal winds over the glacial cycle using the different ocean parameterisations as well as difference plots for the summer surface temperature between the fully forced and sensitivity simulations.

The present contribution by Löfverström et al. investigates the evolution of atmospheric flow during some key points of the last glaciation, MIS5b, MIS4 and LGM. They perform a set of simulations using CAM3+SOM accounting for recently published Northern Hemisphere ice sheet topographies. The main conclusion of the paper is that until a threshold in Northern Hemisphere ice sheet topography is reached at LGM, the atmospheric mean flow does not significantly differ from interglacial conditions, i.e. when ice sheets are not present over North America and Eurasia.

This paper nicely described the main atmospheric features of those

particular key points of the last glaciation. However, to my opinion, most of those results are model dependent, as shown by the large amount of literature that the authors may found about modelling the last glacial cycle since many decades. Furthermore, the choice of CAM3+SOM (slab ocean) might not be the most appropriate model configuration to drawn the conclusions of this manuscript. Part of the explanation of what is happening over Eurasia relies on the impact of the large un–realistic sea-ice cover which is simulated by the slab ocean model employ in this study. In addition, the performance of CAM3+SOM for present-day are not shown and the LGM could be validated against LGM paleo-proxies. There are some majors points that should be addressed more in detail before acceptance:

In general, this article is lacking of key bibliography about data and modelling and I strongly recommend to update the bibliography and incorporate the references to strengthen the results of this work. In particular, the explanation of Eastern Siberian ice-free area is void of references while the authors can find many of them. Furthermore, the explanation that the authors gives for ice-free regions is sometimes overestimated. Over East Siberia, it has been demonstrated that the impact of circulation+ dust+vegetation is the key to not grow an ice sheet over those areas. Then I would recommend to look at: Kageyama et al. (1999), Journal of Climate; Kaspar et al., 2007, Climate of the Past; Roche et al. (2010), Climate of the Past; Beghin et al., 2014, Climate of the Past; Krinner, 2006, Climate Dynamics; Colleoni et al, 2010, Climate Dynamics, etc...

A proper validation part is needed: the author have to show the performance of CAM3+SOM at T85 for present-day, and also compare their full LGM simulation with paleo-proxies, especially for SSTs (with MARGO for example) and other terrestrial archives, or at least compare their seasonal climate to results from PMIP3 simulations. Given the strong difference in Kleman et al reconstruction with that of ICE-5G for LGM, what is the difference between MIS5b and MIS4? Can you demonstrate that those differenc ein topography do not alter your results? the author should demonstrate, rather that only state, that the parametrized OHT does not impact on their results (which I doubt because of the un-realistic sea-ice cover).

The authors choose to set the orbital parameters to MIS5b, MIS4 and LGM and then in the results state that until reaching LGM topography, the jet is not significantly changed. But I am not sure this is possible to draw such a conclusion with the simulations that have been performed in this work. The Arctic Oscillation is potentially very influent on what happens over the Arctic margins of both North America and Eurasia. The mean state of the AO depends on the orbital parameters (Groll et al., 2006) and have large consequences on the nearby areas. It would have been usefull to perform at least two other simulations set at LGM orbital configuration and GHGs, to demonstrate that circulation features that are described in this paper are not significantly influenced by the orbital configuration. I think the recent contribution of Beghin et al., 2014, Climate of the Past is particularly interesting in the set of experiments that they did. Finally, at different places, the authors state that one climate or another is more favourable to the built of Eurasia. How is simulated the perennial snow cover around this ice sheet and in Siberia? You cannot say wether or not a climate is favourable to glaciation unless looking specifically at the perennial snow cover.

Response:
The reviewer raise a number of valid points that clearly needs to be considered and discussed in more detail. We provide a general discussion here and specific responses below.

Firstly, we emphasize that the main thrust and novelty of this paper is in its analysis of the pre-LGM circulation. The LGM is of course both interesting and important but it is only one of the four cases considered and the main focus of the study is really on the influence of the pre-LGM ice sheets and the evolution of the atmospheric circulation leading up to the glacial maximum. This is after all the title of the manuscript "Evolution of the large-scale atmospheric circulation in response to changing ice sheets over the last glacial cycle".

The reviewer comes back several times to the choice of running a slab-ocean model instead of a fully coupled AOGCM and point out that there is a model dependence and that the slab-ocean configuration it not and optimal choice for this type of study. We fully agree with the claim that there is a model dependence. This has become particularly evident from the coordinated modelling studies in CMIP/PMIP where the models use identical forcing protocols and still diverge in many important aspects, though they all use fully coupled AOGCMs. An AOGCM is arguably superior to a slab-ocean model in many ways but the latter is considerably cheaper to run and allows for a much greater number of perturbation simulations that is not computationally feasible to do in a fully coupled model. Our strategy therefore was to use the slab-ocean approach but quantify the uncertainty due to changes in ocean circulation by repeating all experiments using two end-member prescriptions of OHT, one representing modern pre-industrial conditions and another the LGM. Note that both these OHT prescriptions have been obtained from equilibrated simulations using the fully-coupled AOGCM version of the model. In the revision, we have added extensive supplementary material showing results for both choices of OHT, which shows that our main conclusions are robust to ocean changes in ocean circulation (and sea-ice cover).

The question of model dependence remains, of course, and we have stated this clearly in the revised text. The only claim we make about robustness of our results is the deflection of the wind field over the North American continent that results from the interaction between the mean flow and the Rocky Mountains in winter. This is a real world feature that is explained by basic theory and also reproduced by all models regardless of their complexity. Theory states that the mechanical forcing of stationary waves is proportional to the mean flow times the topographic slope ($\mathbf{u} \cdot \nabla h$). Since the incipient ice sheets are located in the lee of the Rockies, the mean flow is largely parallel to their height contours and thus the stationary wave pattern is qualitatively unchanged from the interglacial distribution. This is of course only true as long as the mean flow is allowed to circumvent the ice margin. However, since all models simulate a deflection of the wind field downwind of the Rockies in winter it is not far fetched to assume that the "lack" of mechanically forced stationary waves by the North American ice sheets in the pre-LGM climates will be a robust feature among models and it is therefore also likely to have been the case in reality.

Specific comments:

Page 1382 - line 16: there are some imprecisions about the dates: 2.6 Millions years (I always see 2,7 Myrs in literature, can the author, in any case provide with a reference for this first sentence?)

Response:
There seems to be some confusion in the literature regarding the start of

Quaternary and Pleistocene. We have included a reference (Gibbard and Kolfschoten, 2004) to support the number 2.6 Myrs given in the introduction.

Page 1383 - lines 7-8: please provide a reference for the British ice sheet or remove the statement.

Response:
We have added the following reference, Bradwell et al. (2008), suggesting a confluence of the Eurasian and British Ice Sheets.

Page 1383 - line 25: you only cite MARGO, why don't you cite CLIMAP and QUEEN also? MARGO is only dealing with SSTs

Response:
That is a good point. We have added the suggested references.

Page 1384 - line 20: to me, "interglacial conditions" means: maximum in sea level + high GHGs + no/few ice over the Northern Hemisphere. Please remove the imprecision and find another terminology.

Response:
A statement is included to emphasize that we refer to the atmospheric circulation, not other factors such as the global sea-level, greenhouse gas concentrations or the number or size of the ice sheets. The sentence now reads:

"An important unanswered question about the pre-LGM climate is whether the atmospheric circulation characteristics were more similar to those in the LGM or in the interglacial."

Page 1385 - lines 8-11: please include the proper bibliography about Eastern Siberian ice-free causes.

Response:
We have added a number of references discussing possible reasons for the lack of glaciation in Alaska and East-central Siberia:

"Similar questions arise regarding the documented absence of major ice sheets in places where they could be expected, such as Alaska and East-central Siberia (Svendsen et al., 2004; Kleman et al., 2013), and the reasons for the general south-westward migration of the Eurasian Ice Sheet through the last glacial cycle (Sanberg and Oerlemans, 1983). Ice core analysis has revealed that the atmospheric dust concentration was considerably higher over the glacial cycle than in the present atmosphere (Mahowald et al., 1999). Theories have therefore been put forth suggesting that dust deposition may have contributed to the absence of major ice sheets in Alaska and East-central Siberia (Calov et al., 2005a,b; Krinner et al., 2006; Colleoni et al., 2009). Other studies show that changes in the the vegetation cover (Claussen et al., 2006; Colleoni et al., 2009) and SST distributions (Colleoni et al., 2011) may also have added to hinder the developments of ice sheets in these areas."

Page 1387 - lines 20-21: the author should demonstrate this statement

Response:
We have added figures in the supplementary material to support this statement. There are some differences in both the location and strength of the stationary eddies but the large-scale circulation features that we focus on (primarily the tilt of the Atlantic jet in the winter and the evolution and westward propagation of the high anomalies over Alaska and Siberia in summer) are not particularly sensitive to the ocean heat transport (OHT).

The warm surface temperature anomalies in Alaska and central Siberia in MIS 5b and MIS 4 appear in the same regions and with approximately the same strength regardless of the OHT. The "lack" of warming east of the Eurasian Ice Sheet at the LGM is also obtained using the interglacial OHT that has less sea-ice in the eastern North Atlantic.

Page 1389 - lines 1-5: given the strong difference in Kleman et al reconstruction with that of ICE-5G for LGM, what is the difference between MIS5b and MIS4? Can you demonstrate that those difference in topography do not alter your results?

Response:
MIS 5b and MIS 4 are periods with very different amount of glaciation in North America and Eurasia, see Fig. 1 and 2. The ice margins in these pre-LGM time periods agree with other data-based reconstructions of the North American (Stokes et al., 2012) and the Eurasian Ice Sheet (Svendsen et al., 2004). The key result of our paper is that the circulation over the North Atlantic changes very little despite these large changes in boundary conditions. This is due to the fact that both MIS 5b and MIS 4 feature a large open corridor between the Rockies and the Keewatin/Quebec ice domes. While there is clearly uncertainty in the precise volume and structure of the

MIS 5b and MIS 4 ice sheets in the Kleman et al reconstruction, it is highly unlikely that the ice sheets would have covered the entire North American continent before the LGM.

We have added a note in the text about how our LGM ice sheets conform with the PMIP3 reconstructions.

Page 1390 - lines 7-8: if you would have used a proper AOGC; you would have seen that this difference is much less than what you obtain using the slab ocean. Please include a statement of comparison with PMIP3 results

Response:
We thank the reviewer for pointing out the need for references confirming our findings with the modelling community. When examining the literature we found the following numbers presented from PMIP2 and PMIP3 models.

Braconnot et al. (2007) and Otto-Bliesner et al. (2009) found that the annual global-mean temperature decrease at the LGM compared to the interglacial in the coupled PMIP2 models bracket about 3.1-5.8°C. CCSM3, which use CAM3 as atmospheric component yields a temperature decrease of 4.5°C (Otto-Bliesner et al., 2006, 2009). More recent studies using the updated PMIP3 boundary conditions show similar numbers, e.g. Brady et al. (2013) obtained a 5.5°C temperature decrease in CCSM4 and Kageyama et al. (2013) 4.5°C in IPSL_CM5. Finally, Ullman et al. (2014) reported 5.1°C and 5.4°C using two different LGM ice sheet reconstructions in GISS ModelE2-R.

The annual global-mean surface temperature in the interglacial range about 12.2-14.0°C in the same studies mentioned above and the surface temperature in the, alleged, true LGM equilibrium state in CCSM3 (Brandefelt and Otto-Bliesner, 2009, on which we base our LGM OHT) is 7.9°C which is 0.3°C warmer than what we obtain here. The corresponding number for IPSL_CM5 with PMIP3 boundary conditions is 7.7°C (Kageyama et al., 2013).

Neither of the above mentioned studies report numbers for the global surface temperature change averaged over DJF and/or JJA so that comparison cannot be made. To what extent these numbers are roughly correct is a question that cannot be answered with the present data/reconstructions. However, they clearly show that the annual global-mean surface temperature change from the interglacial to the LGM obtained in the present study is reasonable with respect to the rest of the modelling community, despite the use of a slab-ocean model.

We have added a paragraph presenting these numbers and putting our result in perspective with the rest of the modelling community.

Page 1391 - lines 25-29: please include a statement about how unrealistic is this large sea-ice cover.

Response:
We have added the following statement discussing the LGM sea-ice cover compared to contemporary reconstructions and model results:

"Note that the sea-ice cover obtained in our LGM simulation resembles the CLIMAP (1981) reconstruction which is more extensive than in more recent proxy-data reconstructions, especially in the eastern Atlantic where the perennial sea-ice line is found to not venture past 55°N (Kucera et al., 2005; De Vernal et al., 2005, 2006; MARGO, 2009). Recent modelling studies also show a more moderate Atlantic sea-ice cover than obtained here, see, e.g., Braconnot et al. (2007); Li and Battisti (2008); Pausata et al. (2011)."

Page 1393 - lines 10-13: "blocking" refer to particular atmospheric conditions. Please reformulate and change the terminology.

Response:
That is a good point. The paragraph is rephrased to avoid confusion. It now reads:

"In fact, the entire flow normal to the ice sheet is obstructed and forced on a poleward track. A similar meridional splitting was also found by Manabe and Broccoli (1985) and Cook and Held (1988). Note that the anticyclone over the north-eastern Pacific is strong enough to even force low-level easterlies over parts of Alaska and Siberia."

Page 1396 - lines 15:18: similarly to a previous comment, if you would have used a AOGCM, you would have obtained a more reasonable sea-ice cover. Please include a statement on this in the text.

Response:
As noted above, our LGM ocean heat flux convergence is computed from the equilibrium state obtained in the fully coupled CCSM3 simulation conducted by Brandefelt and Otto-Bliesner (2009). Our sea-ice cover is thus very similar

to what is obtained in an AOGCM, see their Fig. 2.

Page 1399 - line 13-16: you cannot conclude on the robustness unless you use a proper AOGCM. Furthermore, this is strongly model dependent, so you should use a multi-model ensemble to conclude that. Please remove this statement, or reformulate with more moderate words.

Response:
We have reformulated the final sentence of this part of the discussion with a slightly weaker phrasing. It now reads:

“Overall, the conclusion that can be drawn from this discussion is that the atmospheric circulation’s response to the Northern Hemisphere ice sheets during winter may have been muted through large parts of the last glacial cycle, and possibly even at the LGM.”

A comment on the need of multi-model comparison to draw robust conclusions. The reviewer is of course right that a multi-model study is needed to be more certain about the circulation changes caused by the MIS 5b and MIS 4 ice sheets. However, as written in an earlier paragraph of the discussion:

“The North American ice sheets begin their development in the trough downwind of the Rockies, with the ice margin roughly parallel to the mean flow, where they cannot efficiently excite stationary waves (as this requires flow normal to the topography). // ... // Note that it is not by chance that the ice sheets develop where they do: it is precisely the northwesterly advection by the pre-existing flow that creates favourably cold conditions over northeastern North America. We therefore expect this to be a robust effect that should be replicated also in other models as long as they faithfully reproduce the large-scale flow over the Rockies.”

In short, the meridional deflection of the wind field over the North American continent is a result of the flow interaction with the Rockies, which is a feature explained by fundamental theory that is also captured by all models regardless of their complexity, i.e. the whole range of models from linear barotropic channel flows to fully coupled AOGCMs. Unless the latitudinal position of the mean flow change considerably from the interglacial during MIS 5b and MIS 4 (which is not the case for the LGM simulated by AOGCMs in the PMIP projects) it seems unlikely that a vastly different flow pattern would emerge over the continent and thus interact differently with the ice sheet topography. This is nonetheless an interesting topic for a potential follow-up study.

Page 1400 - lines 1-9: this part deserve a proper bibliography

Response:
We have added some perspectives with both proxy-data (dust deposition and pollen based reconstructions) as well as a number of modelling studies.

Page 1401 - lines 10-14: you can't be so affirmative: you use only one ice sheet reconstructions, prescribed in one atmospheric model coupled to a slab ocean. Please reformulate appropriately, including perspectives with other models.

Response:
This comment is a bit challenging as the reviewer asks us to add multi-model perspectives on the atmospheric circulation over periods that have never been simulated before using geologically constrained ice sheet reconstructions. Also, this part of the manuscript is where we conclude the findings of our study. A comprehensive discussion of the results, including perspectives with other models for the LGM, is provided in the section called "Discussion".

Figure 8: please also includes panels for LGM.

Response:
The idea with this figure is to show that the warmer summer surface temperatures in central Asia in the full MIS 4 simulation are indeed due to the presence of the Eurasian ice sheet. We therefore do not feel the necessity to do the same exercise with the LGM as that is not the point we try to make.

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