

Interactive comment on “Evolution of the large-scale atmospheric circulation in response to changing ice sheets over the last glacial cycle” by M. Löffverström et al.

Anonymous Referee #2

Received and published: 11 May 2014

The present contribution by Löffverström 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

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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 draw 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 over-estimated. 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 topology do not alter your results?

the author should demonstrate, rather that only state, that the parametrized OHT does

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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.

I therefore recommend publication of this work, after dealing with those moderate revisions.

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?)

Page 1383 - lines7-8: please provide a reference for the British ice sheet or remove the statement.

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

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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.

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

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

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?

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

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

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

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.

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.

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

Page 1401 - lines 10-14: you can't be so affirmative: you use only one ice sheet

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reconstructions, prescribed in one atmospheric model coupled to a slab ocean. Please reformulate appropriately, including perspectives with other models.

Figure 8: please also includes panels for LGM.

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