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Climate of the Past Discussions

Interactive comment on "The impact of different glacial boundary conditions on atmospheric dynamics and precipitation in the North Atlantic region" *by* D. Hofer et al.

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This is a short first reply and a more detailed point-to-point response will follow after the editor's decision.

> Anonymous Referee 1
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> Received and published: 3 February 2012
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> This paper studies a classical topic: the atmospheric response, especially precipitation, to different glacial boundary conditions, but now in a state of art model CCSM4. The results are all reasonable and consistent with

> all previous studies. It is, however, very boring to read, because, except for the use of another model, there is nothing really new (in spite of a lot of work, indeed). Perhaps, the only thing interesting is a relatively clear identification of the dominant role of Laurentide Ice Sheet on the response of rainfall, storm track in the North Atlantic region, relative to other forcings. In particularly, the Lagrangian storm tracking is interesting, not commonly used in paleoclimate modeling analysis. Therefore, I would not recommend the paper to be accepted in its present form. Instead, I suggest the authors to refocus the paper on the role of ice sheet on the atmospheric dynamics in the North Atlantic region.

We do not agree with the referee's opinion that the paper shows nothing really new. Clearly, we agree that several existing studies presented the impact of changes under specific glacial boundary conditions (e.g., topography). However, the main purpose of our study, i.e., the analysis of precipitation and the connection to the underlying atmospheric dynamics like storms is rather new in this field of paleo-climate modeling. This is acknowledged by the referee. Additionally, we are not aware of modeling studies, which investigate the sensitivity on different boundaries during different states of the last glacial.

However, it seems that this focus of the paper is difficult to grasp, possibly due to the rather long evaluation section that distracts from the main results. The reason for the detailed evaluation part is to show the model's capacity to produce a realistic glacial climate and to put into perspective the resulting differences due to the changed boundary conditions. We think such a section is essential for the work, but acknowledge that it could be substantially shortened to guide the reader to the main topic of the paper – the sensitivity of connection between precipitation and atmospheric dynamics on different glacial boundaries. To do so, we will remove in the revised version Fig. 2 and 4, and focus only on the aspects where our simulations differ from the reconstruction and other simulations. Apart from a shortening of the evaluation section, the manuscript

will also be tighten in section 4 - especially the discussion of the SAT differences.

> P70, L15: "the coastal line is taken as zero. . . , For the LGM. . . ." The description of the land-sea mask change is not clear. Please rewrite these and makes it clear.

We will clarify the description of the land-sea mask in the revised version.

Changed (p. 70 l. 15-18): ...difference of Peltier's present day and LGM values. The sea-level is lowered by about 120 m with respect to the modern level except for the Caspian Sea which is kept at its present-day extent and the coastal line is adjusted accordingly.

> P74, L10-20: LGM model-data comparison. It will be useful to include the comparison with CCSM3 at least for LGM1 and 2, if this paper really wants to address the LGM model-data comparison. I guess the score of data-model consistence may be similar in CCSM3 and CCSM4. This again, is my point of this kind of study, so what? The big picture is simulated in all models.

The evaluation analysis is also performed for the coarser resolved but fully coupled model CCSM3. As the referee points out the differences between the simulations with respect to temperature and precipitation are small. As the evaluation part will be substantially shortened we will not show additional results from the CCSM3, but we will mention the agreements between the two versions and with other simulations.

> P78, . . . there are so many "not shown". I understand, because there is nothing really new and interesting. There is why the whole paper it boring.

We agree with the referee that the SAT shows changes that are not new which is why no figure is shown there and these results are presented in rather few sentences. However, we do not agree that this translate to the whole paper, as to our knowledge a direct comparison of the impact of the investigated boundary conditions has not been attempted so far.

To omit the "not shown" and to enhance the readability of the paper, the discussion of the SAT will be further shortened. However, the SAT can not be omitted completely, as the different ocean surface conditions - which are one of the investigated impact factors – can influence the atmosphere only by changing the SAT.

> P78, L20-25: it is very difficult to see the difference in response between different ice sheets in Fig.8. (Fig.9 and 10 may be ok, but not Fig.8). To focus on this effect, it is best to difference each other, perhaps, difference LGM ice sheet.

Fig. 8 has been changed to include also the differences to MWLGM.

> Also, ice sheet effect is a classical problem. There have been many papers on this. Manabe discussed it in 70's, and Kutzbach in CLIMAP papers discussed it in 80's. A recent paper on ice sheet effect is Eisenman et al. (2009, Rain driven by receding ice sheets as a cause of past climate change, Paleoceanography 24, PA4209, doi:10.1029/2009PA001778.). The discussion here should be put in the historical context. There are new things here, the storm track analysis is new. But, still, other major features should be in historical context. I think a short paper focusing on ice sheet effect on storm track and precipitation will indeed be interesting.

We acknowledge that the discussion of the papers mentioned by the referee is missing in the present form of the manuscript and will be included, either in this paragraph or

in the discussion section. As the evaluation section will be shortened, the focus of the paper (namely the effects of the different boundary conditions on precipitation and storm tracks) should be clarified for the reader.

P79 L5: "A lower altitude of. . . . In the North Atlantic at 20oN". Which figure this refers to? I can't see these.

A reference to Fig. 8 has been added.

Interactive comment on Clim. Past Discuss., 8, 63, 2012.

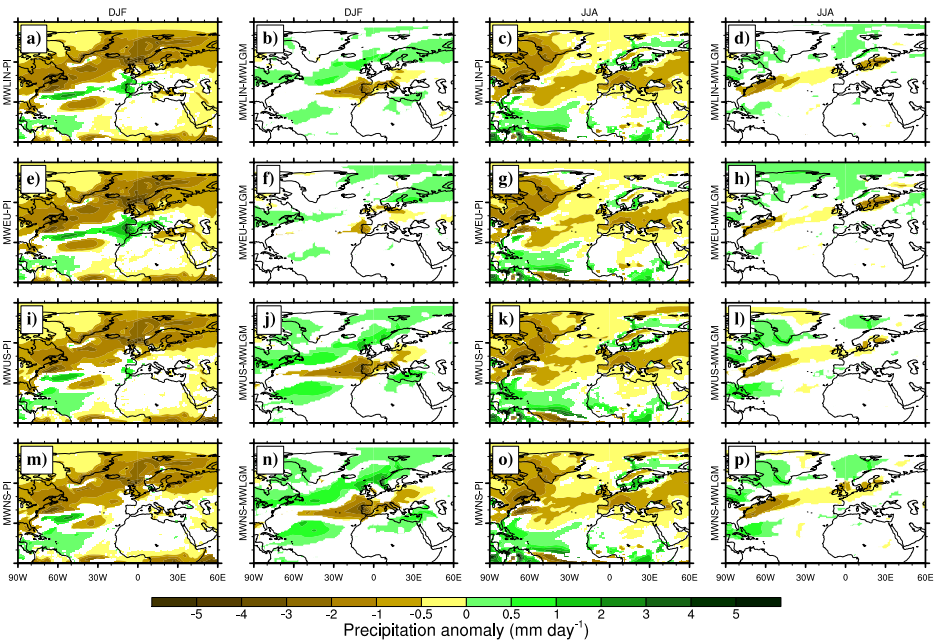

Fig. 1. Winter (DJF, left) and summer (JJA, right) precipitation anomalies with respect to PI (first column) and to MWLGM (second column) for MWLIN (a-d), MWEU (e-h), MWUS (i-l), and MWNS (m-p).