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Climate of the Past Discussions

Interactive comment on "The key role of topography in altering North Atlantic atmospheric circulation during the last glacial period" *by* F. S. R. Pausata et al.

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General comments

Pausata et al analyse atmospheric circulation differences between Last Glacial Maximum (LGM) and Pre-Industrial (PI) climate based on a set of simulations performed with a coupled global climate model. The authors discuss Northern Hemisphere (NH) atmospheric circulation, with focus on the North Atlantic sector. Pausata et al aim at a better understanding of the individual effect of each major difference in the forcing and boundary conditions between the LGM and the PI, i.e. ice sheet topography, ice sheet albedo and atmospheric greenhouse gas (GHG) concentrations.

Their main findings are that

1) ice sheet topography accounts for most of the simulated NH atmospheric circulation difference between the LGM and PI and

2) GHGs and ice sheet albedo effect the Sea Level Pressure (SLP) gradient in the North Atlantic region, though to a lesser extent than the ice sheet topography.

This work is of interest for Climate of the Past but it requires major revisions of the analysis and presentation of the results before it can be considered for publication. My main concerns are:

* The lack of a clear distinction between results that apply to the Northern Hemisphere and results that apply only to the North Atlantic sector/region.

* The lack of a clear distinction between results presented in the present manuscript and previous results presented in Pausata et al (2009). Specifically, the results regarding the importance of oceanic differences (SST and sea ice extent) for the atmospheric circulation differences between LGM and PI should be removed from the Abstract and the Conclusions, since these belong to Pausata et al (2009).

* The statistical significance of the results presented in the present manuscript should be assessed and presented.

* The methods used (to determine SLP gradients and locations of lows and highs) are not explained.

* The setup of the analysed simulations needs to be improved (since the manuscript by Kageyama et al is not even submitted), e.g. the treatment of the ∼130 meter lower sea level in the LGM climate as compared to the PI.

* The trends in the simulated climate towards the end of the simulation should be discussed. Since the LGM climate is radically different from the PI climate one would expect a longer equilibration time than 500 years. Brandefelt and Otto-Bliesner (GRL;

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2009) and Brandefelt et al (CPD; 2011) find that there are significant differences in the North Atlantic climate after ∼500 years of integration as compared to after ∼1500 years of integration.

Minor comments

Abstract

line 4: The ice sheets did not cover "large parts of Eurasia", I suggest changing Eurasia to Europe.

line 10: Change "(SLP), 200-hPa" to "(SLP) and 200-hPa"

line 14-16: The sentence starting "We also show that North Atlantic .." This is not shown in the present study!

Introduction

* Since this work is a follow up on Pausata et al (2009) I suggest you give a description of that study; data, simulations, analysis, results.

* Further, as shown by e.g. Pausata et al (2009) the simulated LGM climate and the LGM - PI differences are model-dependent. This should be taken into account when the results of the present study are interpreted.

* Refer to and compare your results to Laine et al (Clim. Dyn.; 2009) who study storm track variability in the PMIP2 LGM simulations.

* Refer to Byrkjedal et al (Clim. Dyn.; 2006) when you discuss the importance of sea ice and SST for the LGM NH atmospheric circulation.

p 577, line 24: I suggest you remove "(GISS model II)", if you wish to keep this information you should give the complete name of the model.

p 578, line 10: remove "that itself adjusts to insolation, ice sheets and GHG changes".

Model and Experiments

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* What are the trends in surface temperature, abyssal ocean temperature etc during the last 100 years of each experiment? Has the climate reached a quasi-equilibrium?

* The methods used to determine the SLP gradients listed in Table 3 and the locations of the subpolar lows and subtropical highs should be described here. These determinations should be associated with some error bars.

* The statistical significance of the differences between different experiments should be assessed and presented for all parameters analysed! When dealing with climate data, we should always test differences for statistical significance.

* Information regarding the sea level in the different experiments is lacking.

* It is important to state that you are not expecting that LGM be exactly equal to (LGMghg+LGMald+LGMtopo), that there are non-linear interactions in the system.

p 579, line 2: the acronym AOGCM has not been defined.

p 579, line 12: change "orbital configuration is" to "orbital parameters are"

p 579, line 15: change "For both" to "In all"

p 579, line 18: "Kageyama et al, in preparation", I thought only published work or accepted for publication could be referenced, but the instructions for authors on Climate of the Past's web are not very clear so I leave this to the editor.

Results

* The results section is quite confusing since the authors sometimes describe the results for the NH and mostly only for the North Atlantic sector. This section requires re-writing with focus on clarity. Perhaps you could move all the results regarding the NH to the first paragraph of the Results section and then state clearly that the rest of the section will deal only with the North Atlantic.

p 580, line 20: I suggest you remove "(see Fig. 1 as well as Fig. 1 in Pausata et

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al, 2009)". Add a new sentence "This is shown in Figure 1 where the location of the centers of the subtropical highs and subpolar lows are displayed."

p 580, line 26: "The SLP gradient in both the Atlantic and Pacific sectors is weaker in the LGMtopo compared to the full LGM experiment". This sentence gives the impression that the difference to the PI climate of the SLP gradient is smaller in LGMtopo than in LGM in both the North Pacific and the North Atlantic region. This is however not true, in the N Pacific LGMtopo and LGM have differences to PI in the SLP gradient of the same sign, the difference is even larger in LGMtopo than in LGM. In the N Atlantic on the contrary, LGMtopo shows no difference in the SLP gradient as compared to PI, whereas for LGM the SLP gradient is substantially larger than in PI.

p 581, line 1-5: Suddenly, without notice, the results are only valid for the N Atlantic region!

p 581, line 1: "LGMghg and LGMalb simulations exhibit stronger SLP gradients, ..." Stronger than what?

p 581, line 2: "but much less change (compared to the control simulation) in the location of the subtropical highs and subpolar lows." I do not agree, the shifts shown in Fig 1 for LGMghg and LGMalb are not "much less" than in LGMtopo

p 581, line 8-10: "The sensitivity experiments presented here reveal that these ...." Simulations with one AOGCM can only "indicate" the importance of of topography, not "reveal".

p 582, line 11: add "(not shown)".

p 582, line 12: remove ", and has been verified by comparing maps of the climatological-mean response and its interannual variability."

p 582, line 13: there is also a distinct northward shift of the zonal wind speed maximum

p 582, line 14-25: It is quite possible that your discussion regarding the influence of

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the albedo on the temperature gradient influencing upper level winds is right. However, since the relation is not that simple for the present and future response of the upper level wind to enhanced GHG concentrations (Hoskins, Science, 2003) the ideas presented here need to be proven. Include a figure of the zonal mean (or sectorial mean) temperature as a function of latitude and pressure.

p 583, line 12-14: Why is SAT, and not SST, analysed in this section?

p 583, line 26-27: Add a note on the non-linear effects that are evident from Fig 4 (LGM $\sim$= LGMtopo+LGMghg+LGMalb).

Discussion

* Please clarify regarding which results are valid for the NH and which are valid only for the N Atlantic

p 584, line 21: suggest to change "...much smaller influence" to "smaller, sometimes even opposite, influence".

p 584, line26: "In a world with increased GHG concentrations ...." Remove or rewrite with reference to Hoskins: Enhanced GHG concentrations gives a weaker meridional temperature gradient at the surface, but a strengthened temperature gradient in the upper troposphere - lower stratosphere (i.e. at 200 hPa) which gives a strengthening of the 200 hPa wind (Hoskins, Science, 2003).

p 585, line4: suggest changing ", even when ..." to ", also when ..."

p 585, line 7: suggest changing "discrepancy" to "difference"

p 586, line 1-3: Rewrite to acknowledge the fact that this conclusion is based on only ONE model.

p 586, line 5-7: Remove the sentence starting "Presumed links ...."

p 586, line 8-11 and line 17-19: Do you have any reference for the statement that the

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atmospheric circulation mean state is very sensitive to model-model differences in the treatment of the topographic boundary condition? Else, rewrite making it clear that this is a speculation.

p 586, line 11-13: Clarify if the "different coupled climate model simulations " were simulations performed with different models or different simulations performed with the same model.

p 586, line 13-15: for the reader to judge the significance of getting "remarkably similar SLP fields" using SST distributions from two different coupled simulations we need to know how different the SST distributions were.

p 586, line 17-19: suggest change "The results of this study" to "The results of the present study"

Conclusions

* Please clarify regarding which results are valid for the NH and which are valid only for the N Atlantic!

* The role of the SST should not be regarded as a result of the present study!

p 587, line 1-3: Specify if these effects are in the same or opposite direction as compared to the effect of the topography.

Table 3 and 4

* Table 4 is an almost duplicate of table 3. Please remove one table and check carefully that the correct information is given in the remaining table. It looks as if Table 4 is correct.

* Statistical significance of the differences in the SLP gradient

New table

* Suggest including a new table with the displacement of the locations of the subpolar

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lows and subtropical highs

Figure 1

* It would be easier to follow the reasoning in the Results section if isolines for the difference in climatological SLP (LGM*-PI) were shown for all experiments

* Increase font size for titles and colorbar

Figure 2

* Increase font size of title

Figure 3

* Include 0-90N in the figure, interesting to see what happens on the southern flank of the jet.

Figure 4

* Statistical significance

* Increase font size of title

Interactive comment on Clim. Past Discuss., 7, 575, 2011.