

Interactive comment on “Greenland Ice Sheet influence on Last Interglacial climate: global sensitivity studies performed with an atmosphere–ocean general circulation model” by M. Pfeiffer and G. Lohmann

Anonymous Referee #1

Received and published: 10 April 2015

This study presents an extensive set of climate model simulations focusing on the Last Interglacial (LIG) climate and in particular on the impact of changes in the characteristics of the Greenland Ice Sheet (GIS) on simulated surface temperatures and how these results compare to proxy-based LIG temperatures. However, the sheer number of experiments, some of which do not have a clear function as far as I can tell, make the manuscript overall difficult to follow and makes that it lacks focus. If these issues and a number of comments and questions are answered I see the manuscript fit for publication in *Climate of the Past*.

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Main comment: The aim of the manuscript seems to be to determine what the impact is of changes in the characteristics of the GIS on surface temperatures and how this impacts the model-data comparison of LIG temperatures. This is a very relevant question and the presented sensitivity experiments with different sizes of the GIS allow one to investigate which size yields the best model-data comparison with respect to surface temperature anomalies. However, while the resulting temperature changes from the sensitivity experiments with different sizes of the GIS are thoroughly discussed (perhaps too extensive, see one of the next points), what lacks is a good discussion of these results and their implications. The manuscript would greatly improve if it would present less detailed descriptions of the results and more interpretation and a deepened discussion. Here I'm thinking about questions like what size of the GIS yields the best model-data comparison? Through which mechanisms do changes in the GIS geometry change surface temperatures in the surrounding regions? How do the results compare to other data sources (ice core data for instance) and model experiments (GCMs and ice sheet models) and, finally, if indeed the results allow one to determine whether or not including GIS changes give an improvement of the model-data comparison, is for the right reason? These issues are certainly partly discussed throughout the manuscript, for instance in lines 26-29 of page 961 and lines 1-14 of page 962, but since they are the main topics of the manuscript (and the most novel aspect of it) I think they should be more thoroughly discussed and appear both in the abstract and the conclusion.

General comments: 1) The results section is rather long and hard to follow. I see a couple of things that could be changes to improve this. Firstly, since all numbers are given in the table and figures, this part could be more focused on the most important

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finding. Secondly, the reader could be guided through this section by including a short introduction of what is to come. Finally, this section would improve significantly if it is made clear what the purpose is of the different sensitivity experiments and why they are discussed in a certain order.

2) There are a couple of simulations which do not have a clear purpose as far as I can tell. Can the authors clarify the reason of including the simulations with different CH4 levels (LIG-1300m-alb-CH4) and the experiments LIG-GHG, LIG-125k and GI? And similarly, why do the authors include a HOL-tr simulation? What does it tell about the main topic of this manuscript, being the impact of changes in the characteristics of the GIS on surface temperatures during the LIG?

3) Some of the presented results are not clearly linked to the main topic of this manuscript. What is the link of the main topic with sections 3.2 and 4.3? Making more clear why these results are presented and how they relate to the main research questions of the manuscript would greatly improve the structure, flow and therewith readability of the manuscript.

4) A difficulty in this study is the lack of a clear explanation of the mechanisms that cause the high-latitude Southern Hemisphere warming resulting from the lowering of the GIS. Although a fair point is made on lines 23-24 of page 957 that it is beyond the scope of this manuscript, I have problems with the fact that the manuscript does refer to these changes in a number of occasions. For instance line 3 page 937 indicates that this study will go beyond investigating the impact of a reduced GIS on the Northern Hemisphere, thus into the Southern Hemisphere. On lines 14-19 of page 964 the results of the model-data comparison is discussed for the high-latitudes of the Southern Hemisphere and compared to how other models perform. Either do not discuss these regions or do, but then also explain the mechanisms behind it.

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5) Throughout the manuscript many results are presented and discussed that detail on the impact of GIS elevation and extent changes on the LIG model-data comparison. However, it does not become very clear if overall including these changes improves the model-data comparison. In lines 18-30 of page 952 it appears that the Turney and Jones data are better matched when including GIS changes, while the CAPE data are better match with a PI GIS configuration. The next paragraphs seem to make clear that it is not easily established whether or not including GIS changes improves the model-data comparison. This point should be made more clear and discussed more thoroughly. For instance, what does it indicate that including GIS changes leads to an improved model-data comparison in locations far away from the GIS itself, while in the Northern Hemisphere high latitudes the comparison does not improve? Why are the figures that show the model-data comparison for the simulations with PI GIS configuration not included in the main manuscript?

6) On line 12-13 of page 957 it is mentioned that the changes in atmospheric circulation are small. Nonetheless, afterwards a number of important results are linked to changes in atmospheric circulation. For instance the changes in the AMOC strength (lines 2-7 of page 958) and the cooling west of Greenland (lines 25-29 of page 958). Including a description of the changes in the atmospheric circulation would greatly improve the manuscript. How do the changes compare to results in the recent publication by Merz et al (2014a, 2014b), see the 'interactive comment' by Andreas Born for more details.

Minor comments:

Line 4 page 934: "...with a notably lower Greenland Ice Sheet...". Isn't it under discussion whether or not this lowering was really 'notably'?

Line 21 page 934 (and also line 6 page 938): Why are the transient simulations used

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to investigate the possible impact of a seasonal bias in the proxy-records?

Line 9 page 935: Past geologic timescales?

Line 10 page 935: 'are a useful test bed'. This sounds like there are other test beds as well, are there?

Line 1 page 936: 'is also considered'. It is not clear what the word 'also' is referring to.

Line 4 page 936: 'at the expense of winter insolation in the tropics'. Do you mean the winter insolation in the mid-high-latitudes of the Northern Hemisphere?

Line 13 page 936: 'is considered to be'. This is perhaps a bit too strong, at least when you are talking about the LIG in general.

Line 20 page 936: This sentence makes it sound like the GIS is the only possible contribution to the global sea level. Please clarify.

Line 22-23 page 936: This sounds like there is a specific proxy that gives information about the contribution of the GIS in particular to sea level changes. Please clarify.

Line 24-30 page 936: It would be helpful for the reader if you could summarize these studies by providing the range of estimates of the contribution of LIG GIS changes to global sea level. Further on in the manuscript these numbers can be compared to the changes that are imposed in the different sensitivity experiments.

Lines 14-23 page 937: This paragraph starts out by discussing previous studies that have investigated LIG GIS, these studies don't so please move them to another section for clarity.

Lines 9-11 page 938: I don't see why this sentence is here. Please remove or move to another part of the manuscript.

Lines 12-27 page 938: Use this paragraph to make clear what the reader can expect in the remainder of the manuscript. Including a short description of the different simulations that will be presented and what their purpose is with regard to answering the main research questions.

Lines 20-21 page 939: This line appears to say to models with flux corrections cannot be used to study climate states beyond the present. Please clarify.

Line 26 page 939: Include previous LIG studies.

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Line 1 page 940: Perhaps include a short description of the orbital forcing of the LIG (130kyPB) to help the reader understand the results. Is the transient orbital forcing described in the manuscript or depicted in the supplement?

Line 9 page 940: Why use mid-Holocene GHG values?

Line 11 page 940: Make clear why increased CH₄ levels are used. It appears from table 1 that the CO₂ levels are also slightly different. Perhaps the description of the different GHG forcing can be moved to the end of this paragraph.

Line 16 page 940: Include a description of how these GIS changes translate into meters sea level equivalents and how this compares to literature estimates.

Line 12 page 941: Perhaps move the description of the transient simulations to here?

Line 4-5 page 942: Why is a Holocene simulation included?

Line 7 page 942: what kind of near equilibrium state? What are the forcings of this equilibrium simulation?

Lines 19-28 page 942: Are lines 19-20 discussing the definition for the equilibrium experiments and the other lines for transient simulations? Are the 50(100) coldest or warmest months consecutive months or taken from throughout the LIG? If the latter is the case, how does this relate to the dating uncertainty in proxy-records that the authors try to capture with this method?

Lines 4-15 page 943: One are the CAPE temperature reconstructions considered summer temperatures and the Turney and Jones temperature reconstructions annual mean. Are they in general different types of proxies or is it related to the different geographic locations or a different interpretation of the proxies?

Line 4-16 page 944: what is the direct impact of the changes in GIS elevation on local temperatures through the lapse rate and how does this compare to the total simulated temperature changes?

Line 15 page 944: Is the 0.5Sv change significant?

Line 15 page 945: Is the 0.2Sv change minor or perhaps even smaller, say negligible?

Line 22 page 945: is this +0.24C value the same for NH, SH and globally?

Line 21 page 947: What is the impact of the choice in alignment between the LIG and

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the Holocene. In other words, do the described differences between the two periods point towards differences in terms of the response of the climate to changes in the forcings, or do the differences appear because of the choices made in the alignment?
Line 21 page 947: are any of the results presented here discussed in the discussion section?

Line 6 page 948: are these temperature changes per ky? Per 10ky?

Line 7 page 950: This section is very long, perhaps use subheading to improve the readability.

Line 14 page 951 to line 17 page 952: Try to structure the description of the results, try not to jump back and forth between different geographical regions.

Line 10 page 953: What does this 0-10C range mean? Please clarify.

Line 16 page 953: what do the summer minimum and summer maximum LIG warmth mean? What is their relationship to the uncertainty in the interpretation of the proxy records?

Line 29 page 954: 'not as good'. Can the comparison for terrestrial data be considered as good?

Lines 15-27 page 955: In this methodology, do you consider every site individually when determining the season for which the simulated temperatures fit the reconstructions best? If so, is this realistic? Wouldn't one expect some kind of geographical pattern in the seasonal bias of the proxy records?

Lines 4-10 page 956: why is the orbital forcing not described earlier in the manuscript?

Lines 7-9 page 956: in which season did the low latitudes receive less insolation or is it an annual mean signal?

Line 8 page 956: shortly explain why the calendar shift only has minor impact on the results presented here.

Lines 22-24 page 956: 'hinting to'. Please shortly clarify this point. What kind of processes/feedbacks are involved. And is this true for both hemispheres?

Lines 14-15 page 957: how do the easterlies impact the Barents Sea? Please clarify.

Line 24 page 957: include a better description of the AMOC changes in the different

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experiments. In the LIG the AMOC weakens compared to PI? And the lowering of the GIS partly counteracts this weakening? Explain why the AMOC changes are simulated, especially since the authors connect the changes to important temperature changes in the high latitudes of the southern hemisphere.

Line 10 page 958: What could be the cause of the different response of the AMOC in the studies by Otto-Bliesner et al. (2006) and Bakker et al. (2012)?

Line 17 page 958: Bakker et al. (2012) find that a lowering of the GIS leads to a small additional weakening of the AMOC. Please discuss.

Lines 19-26 page 959: The description of the simulations that do and do not include interactive vegetation is confusing.

On line 12 of page 941 LIG-GHG simulation is said to be the only simulation with fixed PI vegetation. How does this relate to the simulations that are discussed here (LIG-GHG-tr and LIG-ctl-tr)?

Lines 12-14 page 960: Do they find a linear relation between temperature and insolation for all seasons and latitudes? Please clarify.

Lines 20-22 page 960: 'offer a bandwidth of possible temperatures'. Is that an aim of this study? If so please introduce it as such in the introduction.

Line 7 page 961: 'related to sea ice'. Or are the changes in sea ice related to the changes in temperature? Please clarify.

Line 18 page 961 (and also 19-21 page 962 and lines 12-15 of page 965): I don't think that determining which model performs best on a particular model-data comparison in a particular region, without discussion the mechanisms behind it, is scientifically relevant.

Lines 20-24 page 961: Not sure how this fits into the general topic of this section. Please clarify.

Lines 24 page 961 to line 25 page 962: This is an important section. Make clear what the results of this manuscript tell us about how changes in the GIS impact the model-data fit, how this compares to previous model results and how this compares to for instance ice core data.

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Lines 16-20 page 963: I'm not convinced that the results presented here actually allow you to make this statement. Please clarify.

Lines 25-29 page 963: What could such long-term feedbacks be for the LIG? Probably melting of the GIS is one of them, but what other processes do the authors suggest are missing in their simulations? More generally, what should be included in terms of forcings and long-term feedbacks in order to improve future model-data comparison for the LIG?

Line 29 page 964 and lines 1-7 page 965: Make more clear how the presented data support the notion that the comparison of the proxy-data compilation of Turney and Jones with the COSMOS LIG climate simulations is best when simulated annual mean temperatures are used. How certain are the authors on this point? This results appears to be in large contrast to previous studies, but if indeed the case, an important finding. Please clarify.

Line 7 page 967: following on the previous point, isn't 'in fact' too strong a statement?

Line 1 page 976: In this section as well as in the conclusions, it is discussed how certain simulations and seasons provide the best model-data temperature comparison. What is the benefit of describing how one scenario fits one location while another scenario fits another location. They can't all be true! For instance if the extent of the GIS changed, so did the albedo in those locations. And especially considering GHG changes, we know they changed so doesn't an improved model-data comparison in case GHG changes are neglected indicate an improvement for the wrong reason? Please elaborate.

Lines 3-11 page 968: It appears that even if one takes into account a large number of uncertainties, the model-data comparison is still rather poor.

Lines 5-10 page 969: It is concluded that a reduction in the GIS elevation and extent improves the agreement between model and data. How conclusive are the results? Especially since in the next line they mention that in 1 out of 2 data sets that are used, the opposite is found.

Lines 21-23 page 969: Where does this statement on climate sensitivity come from?

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Is it discussed at all in the manuscript? How can one expect to be able to study climate sensitivity in a model experiment in which CO₂ is not even changed?

Lines 24-25 page 969: 'Better representation of the climate models'? Please clarify.

Line 27 page 969: Is it useful according to the presented results to perform transient simulations including transient changes in GIS elevation and extent?

Table 1: are the simulations LIG-GHG, LIG-125k and GI mentioned at all in the manuscript?

Table 2: How are summer and winter defined? Please repeat this information in the caption.

Figure 2: Why are the results of the LIG-ctrl simulation not shown for comparison?

Figure 2 (and others): I find the color-sceme that is used (blue to red) a bit misleading. It nicely shows the difference between positive and negative, but the differences between the different shaded of blue/red are very small and make, for instance, the model-data comparison in figures 8 and 9 look much better than figure 10 shows. Please clarify.

Figure 5: Which one of the presented simulations does not include interactive vegetation changes? How large is the impact?

Figure 5: 21 model years so 210 orbital years? Please mention in caption.

Figures 5-7: why is there no focus on the SH when the transient results are discussed?

Figures 8, 9 and 10: Is the LIG-1300m-alb or the LIG-1300m-alb-CH₄ simulation presented here?

Figure 10: I find this caption rather confusing. Is (b) about annual means and (c) about the seasonal range? What do the vertical bars and the gray bars indicate?

Figure 11: Why is the period 130-120 used?

Figure 11: Why are the proxy locations depicted?

Supplementary information: Where can one find the figure captions?

Technical comments:

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Line 6 page 934: make clear that these are equilibrium simulations.
Line 2 page 935: 'are the projections'
Line 7 page 935: change to "needs to be tested (e.g. Braconnot et al....)"
Line 14-16 page 935: Please rephrase.
Line 13 page 936: 'during the LIG compared to PI'
Line 13 page 938: equilibrium simulation.
Line 14 page 938: Clarify what is considered the 'entire LIG'.
Line 18 page 938: 'physical characteristics' sounds a bit critical. Consider rewording.
Line 26 page 938: 'timing uncertainty'?
Line 6 page 949: Not sure whether the word realization is appropriate when discussing different simulations with different forcings rather than different ensemble members forced by the same scenario.
Line 19 page 950: Isn't Great Britain part of Europe?
Line 2 page 951: 'the sign is generally comparable'. This sounds strange since the sign can only be the same or not.
Line 8 page 956: high latitudes of the Northern Hemisphere.
Line 10 page 956: 'in the early LIG'
Line 9 page 963: 'presents as well' perhaps 'also presents'.
Line 12 page 963: capture at most or simply remove the word 'mostly'.
Line 17 page 963: is the hyphen supposed to be there?
Figure 11: It appears there is a space in Turney.

Interactive comment on Clim. Past Discuss., 11, 933, 2015.

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