Reply to Anonymous Referee #1

Comment:

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.

Reply

We thank the Anonymous Referee #1 for the valuable and in-depth comments on our manuscript. These comments and suggestions certainly help to increase its quality. We do agree that the manuscript is too long and therefore lacks focus. To this end, we have removed from the study some of the model simulations that do not directly relate to the main topic of the manuscript and thus provide a more concise story. This is clarified later in the reply, and highlighted in the revised version of the manuscript.

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

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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.

Reply

The question regarding which size of GIS yields the best model-data comparison is answered in the form of three tables added in the Supplementary material and mentioned in the revised version. The three tables contain the RMSD values between the three different datasets used in this study (CAPE Last Interglacial Project Members, 2006; Turney and Jones, 2010; Capron et al., 2014) and the simulations with different GIS configurations calculated at different time slices and for annual mean and local summer. We have decided to add tables rather than fully include it in the manuscript for two reasons. Firstly, due to the large amount of data, creating model-data comparison maps for each simulation would results in a too long manuscript that would again lack focus. Secondly, the purpose of this manuscript is not to determine which GIS size yields the best model-data agreement, but to determine the influence of GIS changes on global climate during the LIG. The reason for choosing only one reduced GIS configuration in the model-data comparison of the original manuscript is that both proxy datasets from CAPE Last Interglacial Project Members (2006) and Turney and Jones (2010) indicate a significant warming in the northern high latitudes, therefore we have considered to take the reduced GIS simulation which indicates the strongest warming in order to increase the model-data agreement. From the tables is also clear that the main conclusion did not change since the proxy-based temperature anomalies by CAPE Last Interglacial Project Members (2006) indicate the best agreement with the simulation with preindustrial GIS (LIG-ctl), while the Turney and Jones (2010) dataset fits best to the simulation with reduced GIS and changes in albedo (LIG-1300m-alb). For the new proxy-based dataset that is included in the revised version of the manuscript (Capron et al., 2014), we find the best model-data agreement for summer at 125 kyr BP in the LIG-1300m-alb simulation. However, this result is not conclusive with respect to the size of GIS because we do not have other GIS configuration simulations for this time slice.

In the revised manuscript, we have reorganized the Results and Discussion sections in a more clear and focused manner. The description of the results has been shortened. We have also included in the revised discussion the possible mechanisms that lead to changes in surrounding temperatures due to

changes in GIS. The warming in the northern high latitudes during winter can be explained by a delayed response to a warming occurring in October (Fig. R1) which is caused by positive sea-ice-albedo feedbacks. The mechanism behind the warming in the southern high latitudes is explained as well in the Discussion section and later in the reply.

Whether the model-data improvement is for the right reason we cannot say for sure. Other factors like glacial memory effects on 130 kyr BP are not considered in this study as such effects are not well represented in the models and cannot be fully reproduced.

We have mentioned all these main topics in the conclusions of the revised manuscript.
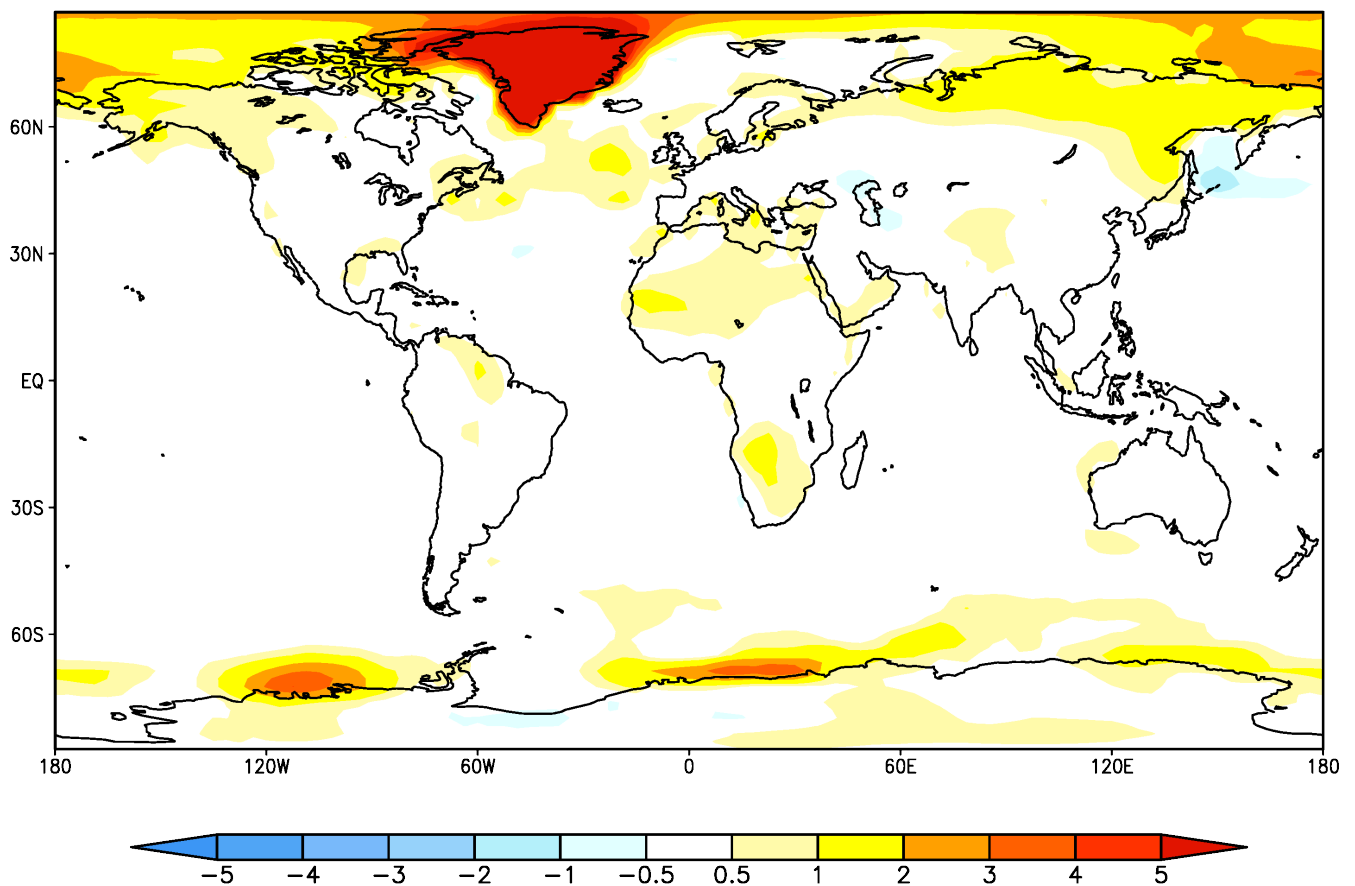
Figure R1. *Effect of Greenland Ice Sheet elevation and albedo in the 130 kyr BP simulation. October mean surface temperature (TS) anomalies (in °C) for simulations LIG-1300 m-alb minus LIG-ctl.*

General comments:

Comment:

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

Reply

We have shortened the Results section and made it more focused. We also give a short description of what is to follow. The purpose of different sensitivity simulations is clarified in the revised version of the manuscript.

Comment:

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?

Reply

The reason for including a simulation with different CH_4 values is indeed not clear in the original manuscript, but an explanation is added in the revised version. The LIG-1300m-alb-CH_4 simulation has been performed in order to have one LIG simulation that has identical GHG concentrations as the PI simulation (Wei et al., 2012) which was run with concentrations as proposed by PMIP2. This simulation is needed in order to be able to quantify the combined as well as separated effects of insolation and changes in GIS and albedo on global climate, without any changes in GHG concentrations since this is not the focus of this study. The effects of different CH_4 values are displayed in Fig. S2 in the Supplementary material of the initial manuscript, but this figure is removed from the revised version since is indeed not of relevant importance to the main story. However, the LIG-1300m-alb-CH_4 simulation is not used in the model-data comparison because all other LIG experiments with reduced GIS do not have identical GHG values like PI simulation from Wei et al. (2012). Therefore, in order to be consistent in the model-data comparison of the proxies with different LIG simulations, we use the simulation LIG-1300m-alb since it has identical GHG concentrations with those used in the

other simulations that consider a reduction in GIS, as well as in the LIG-ctl simulation. We therefore keep the LIG-1300m-alb-CH$_4$ simulation in the revised manuscript in order to be able to quantify the exclusive effects of insolation and changes in GIS configuration on the global climate.

The LIG-GHG simulation was run according to PMIP3 protocol and while this simulation indeed does not contribute to the main topic of the study we decided nevertheless to include it in order to show that the effects of lower GHG concentrations used in the LIG-GHG simulation do not have a large scale influence on the global surface temperature when compared to our LIG control simulation (LIG-ctl). But since it is not part of the main story, we have kept this figure in the Supplementary material (Fig. S1).

The LIG-125k simulation was included in order to see whether changes in insolation would play a major role in the model-data comparison agreement, and also to be able to perform a comparison with results from Otto-Bliesner et al. (2013), who also conduct model simulations for the 125 kyr BP time slice and compare these results to the proxy-based dataset used also in our study. In the revised version of the manuscript, additionally a comparison with the proxy dataset by Capron et al. (2014) is performed for both time slices, namely 130 and 125 kyr BP. We keep the results from the LIG-125k simulation in the Supplementary material since the focus of this study is on the 130 kyr BP time slice.

The GI simulation was included as a "side story" with respect to changes in insolation but for simplifying the story we have removed it in the revised manuscript since it does not add any relevant contribution to the main topic. The HOL-x0.5 simulation is also removed for the same reasons.

The HOL-tr transient simulation indeed does not contribute to the main topic of the paper, namely the influence of GIS on the surface temperature. However, we decided to include it as a temperature evolution reference with respect to the LIG temperature evolution.

Comment:

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.

Reply

We have indeed failed to provide a clear explanation behind the decision to include transient simulations in our study. However, we hope that the revised manuscript presents the reasons more

clearly. The reason for including LIG transient simulations in our study is to to be able to calculate the maximum LIG warmth with respect to summer and annual mean, and use these results in the model-data comparison since proxies from the CAPE Last Interglacial Project Members (2006) and Turney and Jones (2010) datasets are considered to indicate summer and annual mean signals, respectively, at the maximum LIG warmth. These results indeed lead to an increase in the model-data agreement. The proxy dataset from CAPE Last Interglacial Project Members (2006) indicates best agreement with local summer (warmest month) at the summer maximum LIG warmth (in the LIG-ctl-tr simulation), while the proxies from Turney and Jones (2010) compilation fit best to annual mean at the annual mean maximum LIG warmth (in the LIG-1300m-alb-tr simulation). This way we are able to tackle one uncertainty in the proxy data interpretation.

We have decided to display in a figure the temperature evolution during the LIG in order to give the viewer a feeling on how these results look, before including them in the model-data comparison. However, for a better flow of the story, we have kept only the figure with temperature evolution in the northern high latitudes (Fig. 5) and moved the figures with middle and low latitude averages in the Supplementary material (now Figs. S2 and S3). We also shortened the parts that cover this topic in the Results and Discussion sections for a better readability.

Comment:

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.

Reply

One possible mechanism behind the changes in the Southern Hemisphere caused by a reduction of GIS is related to an increase in the AMOC, which transports more heat from the downwelling areas in the northern high latitudes towards the Southern Hemisphere (Fig. R2). One possible explanation for an

enhanced AMOC may be an increase in the salinity in the northern North Atlantic Ocean of up to +1 psu (Fig. R3), increasing thus the density of the water in the downwelling locations. Changes in AMOC due to a reduction of GIS can be additionally explained by an increase in the atmospheric flow displayed in Fig. 11 of the revised manuscript. The low pressure system over Greenland and the high pressure system above Europe become more extreme, enhancing the north-eastward air circulation. However, convection cannot be the only explanation for the southern high latitudes warmth, since the heat would be dispersed towards the Southern Hemisphere. We however note a large scale warming in the subsurface of the Southern Ocean which is probably caused by positive feedbacks. This warming may be related to changes in the water stratification. We observe an invigorated vertical mixing in the northern North Atlantic Ocean (Fig. R4a) and a suppressed vertical mixing in the Southern Ocean (Fig. R4b), the latter causing the heat at subsurface to be preserved. The Southern Ocean has a large heat capacity leading to a long memory of the system. Lags of up the three months occur in the surface layer including sea ice (amplifying factor via positive ice-albedo and ice-insulation feedbacks), while long-term lags occur in deeper levels below the summer mixed layer that store seasonal thermal anomalies (Renssen et al., 2005).

The explanation of these mechanisms are included in the Discussion section of the revised manuscript.
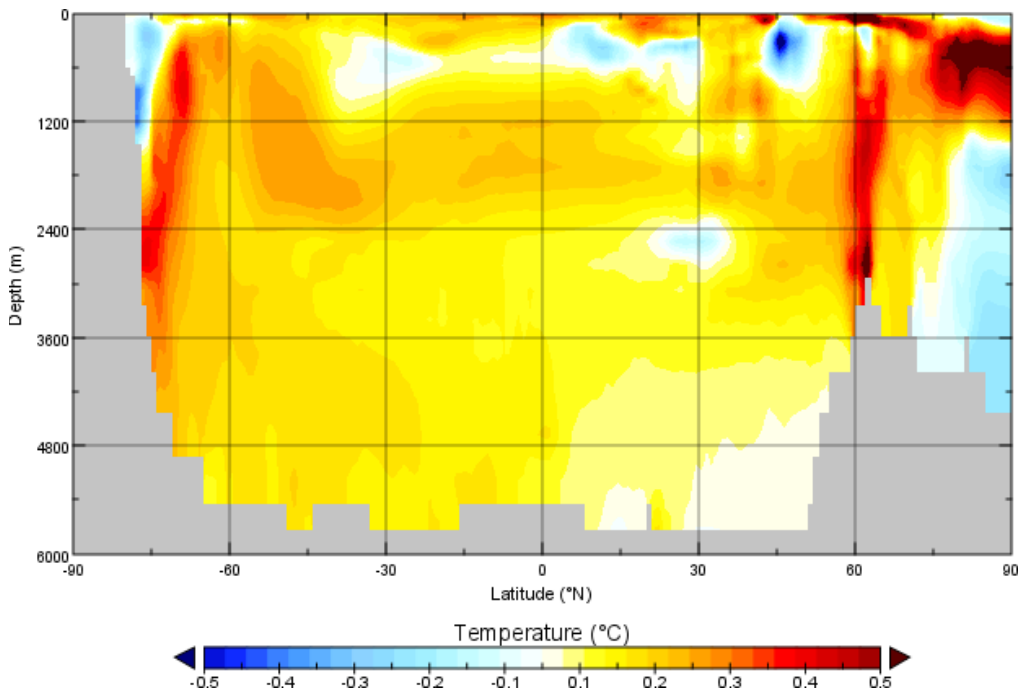
Figure R2. Effect of Greenland Ice Sheet elevation, insolation, and albedo in the 130 kyr BP

simulations. Annual mean ocean temperature anomaly (in °C) for LIG-1300 m-alb simulation minus LIG-ctl simulation.
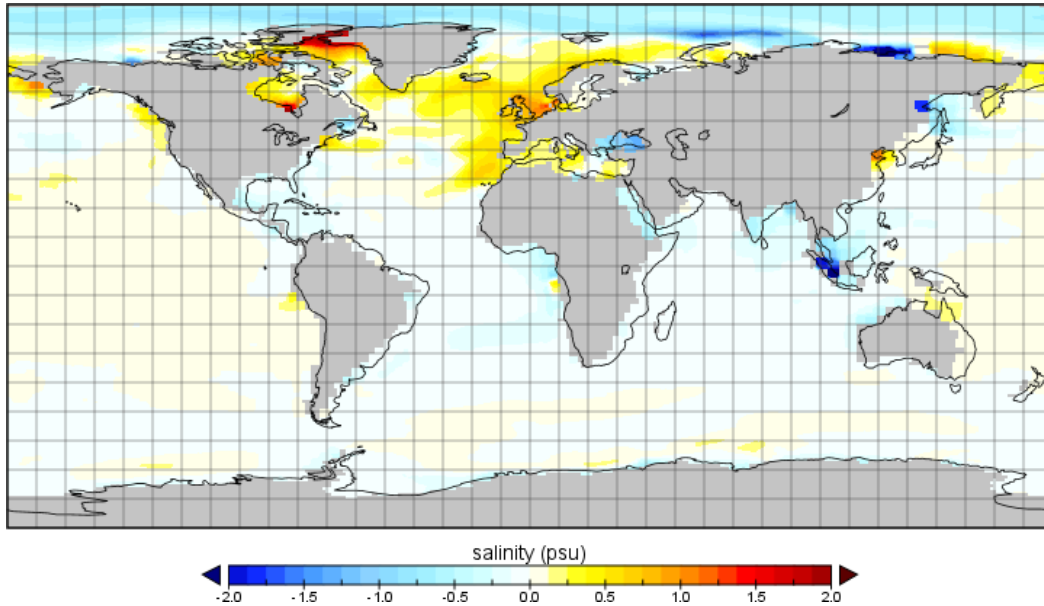
Figure R3. *Effect of Greenland Ice Sheet elevation, insolation, and albedo in the 130 kyr BP simulations. Annual mean sea surface salinity (in psu) anomaly for LIG-1300m-alb simulation minus LIG-ctl simulation.*
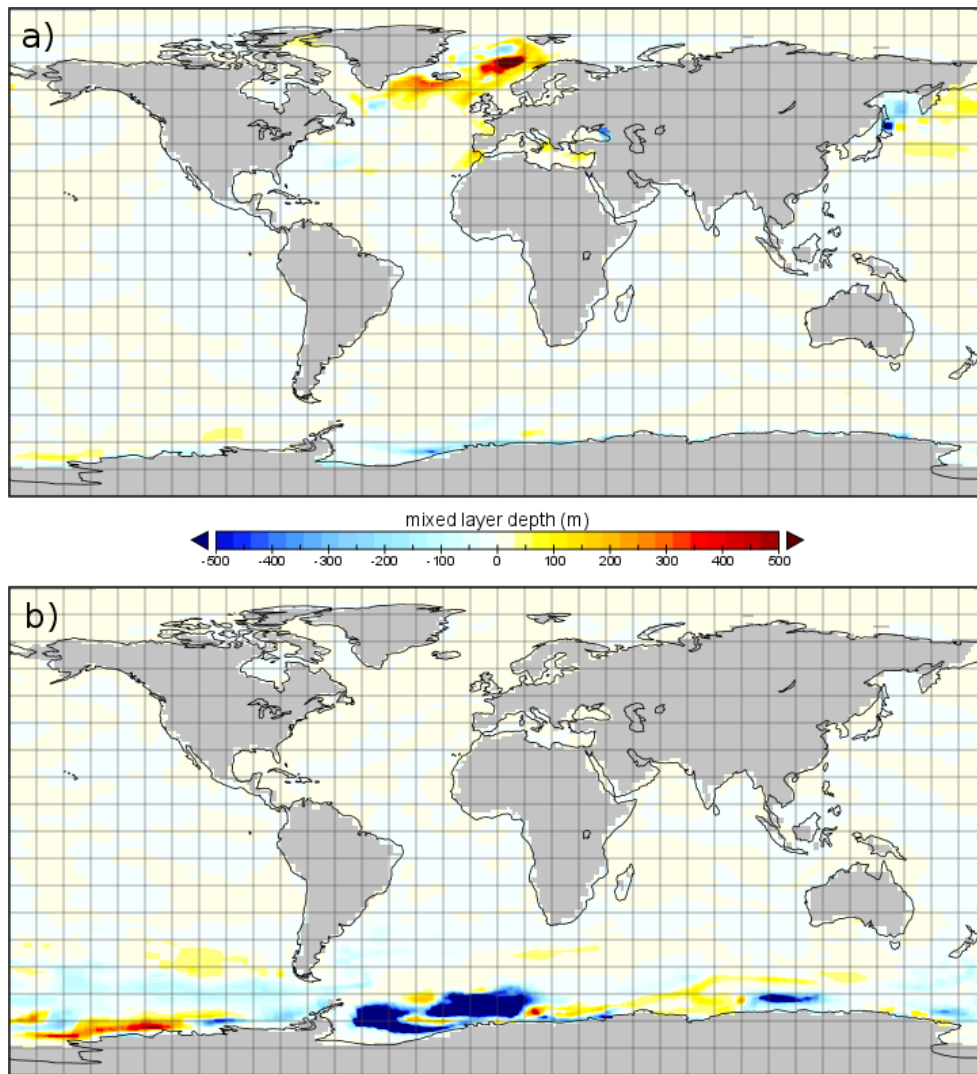
Figure R4. *Effect of Greenland Ice Sheet elevation, insolation, and albedo in the 130 kyr BP simulations. Mixed Layer Depth anomalies between LIG-1300m-alb simulation and LIG-ctl simulation for **(a)** December-January-February and **(b)** June-July-August.*

Comment:

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

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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?

Reply

Indeed, one dataset (Turney and Jones, 2010) agrees best with the simulation with reduced GIS, while the other dataset (CAPE Last Interglacial Project Members, 2006) fits best to the control simulation with preindustrial GIS configuration. Furthermore, the newly included dataset from Capron et al. (2014) fits as well best with the control simulation. One explanation is that a reduction in GIS has the strongest influence during local winter, while during summer the changes are very small (Fig. 3 in the manuscript). Therefore, for CAPE Last Interglacial Project Members (2006) and Capron et al. (2014) datasets, which contain a compilation of summer proxies, changes in GIS do not have a strong influence and thus do not improve the model-data comparison. The Turney and Jones (2010) dataset, on the other hand, represents annual mean which is influenced by winter changes, the season when a reduced GIS gives strong anomalies. Therefore, it fits best to the simulation with a reduction in GIS.

Large temperature anomalies caused by changes in GIS elevation are observed only in the southern high latitudes and northern high latitudes close to Greenland, therefore the model-data comparison in the middle and low latitudes is not affected by changes in GIS. Antarctica indicates a warming due to mechanisms and feedbacks mentioned and explained above and in the revised Discussion section, with heat being transported by atmospheric changes (not shown). A reduction in GIS leads to strong warming in the northern high latitudes, which improves the model-data agreement (Fig. 7ab in the revised manuscript). We have made this point more clear in the revised manuscript.

Additionally, we have included the figures with the LIG control simulation (former Figs. S6, S7) in the revised version of the mnaucsript. For an easier comparison, these figures are merged with the corresponding maps that display results from the reduced GIS simulation (Figs. 6 and 7).

Comment:

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.

Reply

A description of changes in the atmospheric circulation is included in the revised manuscript, as well as the comparison with results by Merz et al. (2014a). The study by Merz et al. (2014a) indicates a rather localized change in the low level winds due to changes in GIS topography, with no major large-scale changes in the atmospheric circulation. Our study focuses rather on large-scale atmospheric changes. We observe an increase in air circulation west of Greenland and above northern North Atlantic Ocean as well as at other locations.

Minor comments:

Comment:

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

Reply

We have removed the word "notably" from the sentence.

Comment:

Line 21 page 934 (and also line 6 page 938): Why are the transient simulations used to investigate the possible impact of a seasonal bias in the proxy-records?

Reply

The temperatures extracted from the transient simulations are calculated as annual mean as well as summer and winter seasons. Annual and summer means are plotted on maps superimposed by the proxy-based temperatures and on scatter plots, while winter is included only in the scatter plots with the dataset from Turney and Jones (2010) and shows the range between the warmest average of 100 warmest months and coldest average of 100 coldest months. We want to investigate whether summer gives an improvement when comparing the proxy data to maximum LIG warmth, but we also want to provide a seasonal range for a more detailed view on the model-data comparison.

Comment:

Line 9 page 935: Past geologic timescales?

Reply

We have rephrased to "Past time periods".

Comment:

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

Reply

For the clarity, we have rephrased to "Past time periods provide the means for evaluating the performance of general circulation models".

Comment:

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

Reply

We have removed the word "also".

Comment:

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?

Reply

We have removed this part of the sentence since it is not relevant here.

Comment:

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.

Reply

We rephrased to "According to different studies, the GIS was lower [...]"

Comment:

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

Reply

We do not find this sentence misleading as we do not claim that the sea level was probably higher only due to GIS melting. Only that if GIS partially melted then this would lead to an increase in the sea level.

Comment:

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.

Reply

We rephrased to "studies based on reconstructions and climate models indicate that [...]".

Comment:

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.

Reply

We have summarized the studies that indicate sea level rise due to GIS melting, with a range of +0.3 to +5.5 m. In our simulations, the GIS changes would results in an approximately 3 m increase in the sea level.

Comment:

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.

Reply

We have split this paragraph in two. The first one covers other studies on changes in GIS during the LIG. It continues with model-data comparison studies that indicate mismatches when reduced GIS is considered. The second paragraph describes model-data comparison studies for the LIG but without changes in GIS, indicating as well a mismatch. Therefore, we consider in our study different boundary conditions in GIS elevation and extent as well as other possible factors that may improve the model-data comparison.

Comment:

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

Reply

We have removed this sentence.

Comment:

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.

Reply

We rephrased the paragraphs in a more clear and concise way, including the purpose of the model simulations.

Comment:

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.

Reply

To avoid confusion we have removed from the text "[...], allowing for applications of the model for climate states beyond present. [...]".

Comment:

Line 26 page 939: Include previous LIG studies.

Reply

We have included references to previous LIG studies.

Comment:

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 orbtital forcing described in the manuscript or depicted in the supplement?

Reply

A short description of the LIG orbital forcing is now included. The transient orbital forcing is not described in the manuscript nor the supplement, but the reference is given.

Comment:

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

Reply

The main focus of this study is to quantify the effects of changes in GIS on global temperatures, therefore we did not change the GHG concentrations to early LIG values. It does not follow the preindustrial GHG concentrations from the PMIP2 protocol, as the PI simulation (Wei et al., 2012) has been produced after we have performed our LIG simulations.

Comment:

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.

Reply

The reason for using increased CH_4 levels are given in the answer of the General comments no. 2). The difference in the CO_2 levels can be considered insignificant. We have moved this sentence at the end of the paragraph.

Comment:

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.

Reply

We have included a short description and a comparison with values proposed by other studies.

Comment:

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

Reply

We have moved the description of the transient simulations as suggested.

Comment:

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

Reply

We include a Holocene transient simulation as a reference with respect to LIG transient changes, for orientation purpose and to display the differences between the present and last interglacial.

Comment:

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

Reply

The "near-equilibrium state" refers to the adjustment of the climate system to the prescribed forcings. Is called "near-equilibrium" because the ocean needs a longer time to adjust than the time length of our simulations. The transient simulations are started using the sensitivity simulations analyzed in this manuscript, namely: LIG-ctl simulation was used for starting the transient LIG-ctl-tr simulation, LIG-x0.5 for LIG-x0.5-tr, LIG-1300m-alb for LIG-1300m-alb-tr, and LIG-GHG for LIG-GHG-tr. The forcings for all these equilibrium simulations are given in Table 1 of the manuscript. The forcings for the equilibrium simulation used for starting the HOL-tr transient simulation are not given because this equilibrium run is not included in the manuscript and thus we did not consider necessary to provide this information.

Comment:

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?

Reply

The first sentence refers indeed to the equilibrium simulations and the rest to the transient. We rephrased for clarity. The coldest and warmest 50 months from the equilibrium runs are calculated from consecutive years, as we always use only the last 50 years of the equilibrium simulations. In the case of the transient simulations, the 100 coldest and warmest months are calculated also from 100 consecutive

years, but as a running average. This method creates a series of subsets of 100 years (e.g. year 1 to year 100, year 2 to year 101, year 3 to year 102, and so on), and then calculates the average of each subset. The subset that shows the highest/lowest average is taken as the maximum/minimum LIG warmth. This method is used in order to filter out internal variability.

Comment:

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?

Reply

The CAPE Last Interglacial Project Members (2006) temperature reconstructions are a compilation of summer proxy-based temperatures selected by the authors of the respective paper, as they wanted to focus on summer during the LIG. Each proxy site is published by different authors and all the references are given in Tables 1 and 2 in CAPE Last Interglacial Project Members (2006). Turney and Jones (2010) is as well a compilation of records published by other authors, but of annual mean proxy-based temperatures.

Comment:

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?

Reply

The lapse rate is actually negligible, the "climate effect" being the dominant one (Fig. R5). We have calculated the "climate effect" by extracting the temperature from the simulation with reduced GIS (LIG-x0.5) at the height of the preindustrial GIS, for each given grid cell. From this interpolated temperature we have extracted the surface temperature from the simulation with preindustrial GIS (LIG-ctl). The temperature over the glacier boundary layer is increasing with height until a specific elevation after which it is decreasing. The increase in temperature with height is larger in the simulation with reduced GIS than in the control simulation.
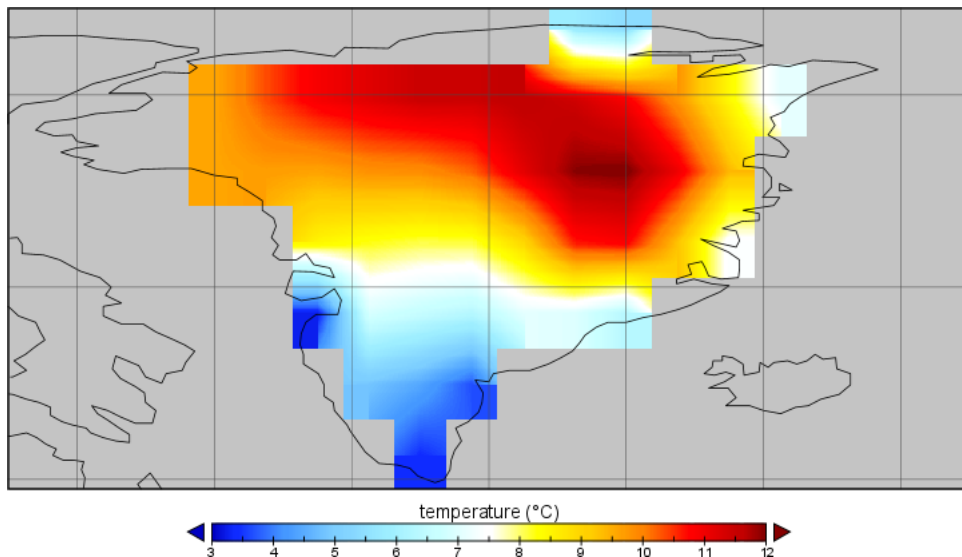
Fig. R5 Temperature anomaly representing the "climate effect". Temperature derived from the simulation with half GIS (LIG-x0.5) interpolated at the height of preindustrial GIS minus the surface temperature derived from simulation with preindustrial GIS (LIG-ctl).

Comment:

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

Reply

The 0.5 Sv change can be considered minor.

Comment:

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

Reply

The 0.2 Sv change is negligible.

Comment:

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

Reply

Yes, the average is the same for Northern and Southern Hemispheres and globally.

Comment:

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

Reply

The choice in alignment is somewhat arbitrary. Differences between the two interglacials are caused by the climate's response to changes in the prescribed forcings.

Comment:

Line 21 page 947: are any of the results presented here discussed in the discussion section?

Reply

These results were also discussed in the Discussion section of the original manuscript, but for simplicity and a more concise story we have decided to remove these results and discussion from the revised version of the manuscript and describe only shortly the importance of these simulations in the story, namely to determine the maximum LIG warmth used in the model-data comparison.

Comment:

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

Reply

The trends have been calculated per 15 kyr.

Comment:

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

Reply

We have introduced subheading in the revised version of the manuscript.

Comment:

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.

Reply

The description of the results is structured based on time slices rather then geographical regions. First, we present results from the 130 kyr BP simulation, describing the comparison in some key regions. Afterwards, the model-data comparison focuses on TS anomalies at maximum LIG warmth, again

presenting results from some key regions. However, for more clarity we have rephrased parts of the paragraphs.

Comment:

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

Reply

The CAPE Last Interglacial Project Members (2006) proxy data compilation does not contain fixed temperatures for most sites but rather temperature intervals. We extract from these specific intervals, the temperatures that fit best to the simulated temperatures.

Comment:

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?

Reply

The summer minimum and summer maximum LIG warmth are calculated from the respective transient simulation. First, we have calculated the warmest month of each model year between 130 and 120 kyr BP. Then, we have calculated the running average with a window length of 100 model years and selected the warmest average of 100 warmest months which represents the summer maximum LIG warmth. For the summer minimum LIG warmth, we take the coldest average of 100 warmest months average. We use this method because the CAPE Last Interglacial Project Members (2006) proxies are considered to represent summer at the peak LIG warmth and we want to determine whether this approach increases the model-data agreement. The minimum summer LIG warmth is additionally calculated in order to have a temperature interval for the comparison.

Comment:

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

Reply

We have rephrased.

Comment:

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?

Reply

The simulated temperature is extracted at the location of each given proxy. A geographical pattern is indeed expected, though in some regions is more difficult to determine. Other studies on model-data comparison that consider seasonal biases have the same assumptions that there are regions that have rather a mixed signal (e.g. Lohmann et al., 2013).

Comment:

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

Reply

We have moved the description of orbital forcing to the Data and Methods section.

Comment:

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

Reply

In the annual mean, the effect of obliquity on insolation in the tropics is minor. Yet, there is still an effect of obliquity on the tropical climate (Bosmans et al., 2015).

Comment:

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

Reply

The calendar shift has a minor effect here because we calculate the summer and winter seasons by extracting the warmest and coldest month rather than June-July-August and December-January-February averages.

Comment:

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?

Reply

Here, we refer to positive feedbacks such a sea ice-albedo feedbacks, which have an influence in both hemispheres.

Comment:

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

Reply

Actually, Barents Sea does not fit in that sentence. We have rephrased for clarification.

Comment:

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

Reply

The AMOC during the LIG is indeed weaker than the PI, but changes in GIS decrease the difference between last interglacial and preindustrial AMOC values. This mechanism is explained in the answer to Comment no. 4) from General comments.

Comment:

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)?

Reply

Both studies consider, in addition to changes in GIS, a relatively strong freshwater flux into the North Atlantic Ocean, a factor that is not included in this study. Such a freshwater input would lead to a weakening of the AMOC.

Comment:

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.

Reply

A short explanation is added in the discussion of the revised manuscript.

Comment:

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)?

Reply

In the Data and Methods section, it is indeed written that the only simulation with fixed PI vegetation is LIG-GHG, but it refers to the equilibrium simulations only, since the transient simulations are not yet introduced. Later, when the transient simulations are presented it is written that the LIG-GHG-tr is the only simulation with fixed PI vegetation, and that refers to the transient simulations only (see Page 942 Lines 15-17 in the original manuscript). The equilibrium simulation LIG-GHG was used for starting the LIG-GHG-tr transient simulation, and both have a fixed preindustrial vegetation. LIG-ctl-tr (and all the other transient simulations were run with dynamic vegetation). However, we have rephrased in order to avoid confusion.

Comment:

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

Reply

Bakker et al. (2013) find a linear relation between changes in insolation and temperatures for both summer and winter and for all latitudes. There are however some exceptions. In northern high-latitudes, the winter temperature changes result mainly from sea-ice related feedbacks and are described as highly model-dependent. In southern middle to high latitudes, winter temperatures are strongly affected by changes in GHG concentrations.

Comment:

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.

Reply

This is not a particular aim of our study, rather an additional result.

Comment:

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

Reply

It is difficult to unravel these effects in a coupled climate model, due to the fact that both influences interact simultaneously.

Comment:

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.

Reply

The sentence from Line 18 page 961 does not refer to which model performs best but to the fact that COSMOS simulates much higher temperatures over Greenland than the ice core-based temperatures from CAPE Last Interglacial Project Members (2006) dataset. We have removed the other two sentences.

Comment:

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

Reply

We have removed this part since is not so relevant here.

Comment:

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.

Reply

We have reorganized this paragraph.

Comment:

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

Reply

For an easier comparison, we have included in a single panel the model-data comparison of simulation with reduced GIS (Fig. 7a, b) and preindustrial GIS configuration (Fig. 7c, d). This figure clearly shows that there are regions in the high latitudes that present an improvement in the model-data comparison when reduced GIS is considered. Moreover, the RMSD values are smaller in the case of comparison of the Turney and Jones (2010) proxy-based temperatures to the simulation with reduced GIS (Table S2 in the Supplementary material of the revised manuscript) than to the simulation with preindustrial GIS elevation.

Comment:

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?

Reply

The long-term feedbacks missing in our climate model refer for example to the state of the lithosphere which has not been yet implemented. A coupled ice sheet model and the biogeochemistry are already implemented in the COSMOS but are relatively new tools, and we did not include them in our LIG simulations because running for example the carbon cycle and the ice sheet into equilibrium would take a very long computational time. Additionally, other factors like glacial memory effect is not well represented and cannot be fully reproduced by the models.

Comment:

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.

Reply

In all considered cases (PI GIS, GISx0.5, GIS-1300m, and GIS-1300m and albedo, at 130 kyr BP, 125 kyr BP, and maximum LIG warmth) the best agreement occurs always when simulated annual mean anomalies are considered. These results are supported by the RMSD values given in Table S2 in the Supplementary material of the revised manuscript. The terrestrial proxies from Turney and Jones (2010) are described as representing annual mean at the maximum LIG warmth and we find indeed the best fit for simulated annual mean TS at maximum LIG warmth in the simulation with reduced GIS (LIG-1300m-alb). We have made the point more clear in the revised manuscript.

Comment:

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

Reply

We have removed "in fact" from the sentence.

Comment:

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 locatioins. 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.

Reply

This is a sensitivity study that considers only one factor rather than a full representation of the LIG climate. The model-data comparison is firstly performed in order to have a feeling on the order of magnitude of LIG temperatures. Future studies taking into account all climatic factors of the LIG should be considered.

Regarding the GHG concentrations, they indeed changed over time, but between the 130 kyr BP time slice and the maximum LIG warmth the differences are in the astronomical forcing which lead to an improved model-data comparison, independent on the size of GIS.

These changes like orbital and GHG concentrations are identical in all LIG transient simulations (except LIG-GHG-tr), meaning that if there is a difference in the model-data comparison between the

different simulations, the reason for this difference is the configuration of the GIS since all the other forcings are identical.

For a shorter and more concise story we have removed this section from the revised version of the paper, and also the part in the Conclusions that summarize the results of this particular section.

Comment:

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.

Reply

Indeed, taking into account several uncertainties does not completely solve the model-data disagreement but this way we manage to at least partly reconcile the model-data discord.

Comment:

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.

Reply

A reduction in GIS elevation and extent improves the agreement between model and data in the case of Turney and Jones (2010). We have rephrased for more clarity.

Comment:

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

Reply

That indeed is not a correct formulation. We have rephrased to "[...] interglacial climate change".

Comment:

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

Reply

Rephrased to: "a better representation of the LIG climate in earth system models".

Comment:

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

Reply

Transient simulations with transient changes in GIS are needed for a more realistic representation of the climate at any point during the LIG. Such studies would be useful for a model-data comparison of LIG temperature evolution.

Comment:

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

Reply

We have removed the GI simulation. LIG-GHG simulation is kept in the Supplementary material in order to show that the differences between the GHG concentrations that we have used in our simulations do not have large effects on TS. The LIG-125k is mentioned in the Discussion of the revised manuscript, when a comparison with Otto-Bliesner et al. (2013) study is included, and also a comparison with Capron et al. (2014).

Comment:

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

Reply

We have added this information in the caption.

Comment:

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

Reply

The results of the LIG-ctl simulation are already included in the comparison. Figure 2 displays TS anomalies between the simulations with changes in GIS (LIG-x0.5, LIG-1300m, LIG-1300m-alb) and the control simulation LIG-ctl.

Comment:

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.

Reply

We have kept the blue-to-red colorbar, but changed the colors in a way that it is easier to distinguish between different shades.

Comment:

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

Reply

The simulation with LIG-GHG-tr does not include dynamic vegetation changes. The impact is significant, as it counteracts the effects of the GHG concentration changes which are mostly lower than the fixed GHGs in the LIG-ctl-tr. Therefore, we expected lower temperatures in LIG-GHG-tr, but actually indicates warmer temperatures. The only difference between these two transient simulations, other than GHG concentrations, is the vegetation which is dynamic in the LIG-ctl-tr, meaning that the vegetation leads to a cooling in the Northern Hemisphere.

Comment:

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

Reply

We have added in the caption: "21 model years representing 210 calendar years."

Comment:

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

Reply

We have created figures only for the Northern Hemisphere because of the load of data and information that led already to a long manuscript. We have kept in the revised manuscript only the northern high latitudes for the same reason. Furthermore, the influence of GIS is the strongest in the Northern Hemisphere, so we decided to leave for the moment the Southern Hemisphere out of the story.

Nevertheless, the transient data from the Southern Hemisphere is used in the model-data comparison of Turney and Jones (2010) proxy compilation.

Comment:

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

Reply

In these figures, simulation LIG-1300m-alb is used. This information is already mentioned in the respective figure captions.

Comment:

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?

Reply

We have reorganized the caption for more clarity. In (b) and (c), the dots are identical representing annual mean. The only difference is that in (b) the vertical bars indicate the range between the maximum and minimum LIG TS with respect to annual mean, while in (c) they show the range between the maximum and minimum LIG TS with respect to summer (warmest month) and winter (coldest month), respectively. There are no gray bars, where it appears gray there is a displaying problem.

Comment:

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

Reply

We use these time interval because the maximum LIG warmth occurred within this interval, not after 120 kyr BP.

Comment:

Figure 11:Why are the proxy locations depicted?

Reply

We have removed the proxy locations, as indeed do not add any information to the story.

Comment:

Supplementary information: Where can one find the figure captions?

Reply

The supplement figure captions are in the file "Pfeiffer_and_Lohmann_supplement.doc" in the ".zip" file containing the supplementary figures.

Technical comments

Comment:

Line 6 page 934: make clear that these are equilibrium simulations.

Reply

Done.

Comment:

Line 2 page 935: 'are the projections'

Reply

We have rephrased to "is the computation of future climate projections".

Comment:

Line 7 page 935: change to "needs to be tested (e.g. Braconnot et al....)"

Reply

Done.

Comment:

Line 14-16 page 935: Please rephrase.

Reply

We have rephrased.

Comment:

Line 13 page 936: 'during the LIG compared to PI'

Reply

Done.

Comment:

Line 13 page 938: equilibrium simulation.

Reply

Done.

Comment:

Line 14 page 938: Clarify what is considered the 'entire LIG'.

Reply

We removed that part since it does not fit anymore to the sentence as it described equilibrium simulations.

Comment:

Line 18 page 938: .'physical characteristics' sounds a bit critical. Consider rewording.

Reply

The paragraph is rephrased and reorganized. We refrained from using "physical characteristics".

Comment:

Line 26 page 938: 'timing uncertainty'?

Reply

We have added "uncertainty".

Comment:

Line 6 page 949: Not sure whether the word realization is appropriate when discussing different simulations with different forcings rather then different ensemble members forced by the same scenario.

Reply

We have removed the word "realization".

Comment:

Line 19 page 950: Isn't Great Britain part of Europe?

Reply

We have removed "Great Britain".

Comment:

Line 2 page 951: 'the sign is generally comparable'. This sounds strange since the sign can only be the same or not.

Reply

We have replaced "comparable" with "the same".

Comment:

Line 8 page 956: high latitudes of the Northern Hemisphere.

Reply

We have added "of the Northern Hemisphere." The part of the paragraph describing the orbital forcing is now moved to Data and Methods section.

Comment:

Line 10 page 956: 'in the early LIG'

Reply

Done.

Comment:

Line 9 page 963: 'presents as well' perhaps 'also presents'.

Reply

Done.

Comment:

Line 12 page 963: capture at most or simply remove the word 'mostly'.

Reply

We have removed the word "mostly".

Comment:

Line 17 page 963: is the hyphen supposed to be there?

Reply

Yes.

Comment:

Figure 11: It appears there is a space in Turney.

Reply

The sentence containing this reference is removed because the circles on the maps have been also removed.

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