Reply to Emilie Capron

1. Summary and general comments

M. Pfeiffer and G. Lohmann present a sensitivity study that aims at quantifying the contribution of the height and extent on the Greenland Ice Sheet (GIS) to the Last Interglacial (LIG) warmth based on a wealth of snapshot and transient simulations performed with the Community Earth System Models (COSMOS). They confront the simulated Surface Air Temperature (SAT) with reconstructed SAT based on marine and terrestrial records and they discuss the observed model-data mismatch. They argue that this mismatch can be reduced when taking into account the seasonal bias of the proxy records and the bias due to uncertainties in the proxy record chronologies and, subsequently, the LIG maximum warmth timing.

This sensitivity study is an interesting contribution with implications relevant to the climate and paleoclimatic communities (both model and data): Evaluating the performance of Earth System Models under the warmer-than-present-day LIG and better constraining the role and the configuration of the Greenland Ice Sheet under such context are key issues of particular relevance in the context of our current and future warming world.

The authors have run numerous simulations and provide a very thorough description of the new simulations. I really appreciate the huge amount of work that this represents. Unfortunately, it results in a very long paper which is difficult to read while other aspects of the paper also need improvements and clarifications. As a result, I can only recommend the publication of this manuscript in Climate of the Past after some major revisions. I will be happy to read the next version of the manuscript and I have listed below comments and suggestions that the authors should consider when preparing

Reply

We thank Emilie Capron very much for the detailed and valuable comments on our manuscript, which help us to increase the quality of the paper. We agree that the manuscript in its published form is too long and lacks focus. Therefore, we have shortened it in the revised version and removed some analysis that was not directly related to the main topic of the paper.

Note: We have corrected in the whole manuscript the terms “surface air temperature (SAT)” and replaced it with “surface temperature (TS)”, as in our analysis we actually use surface temperature (TS) which is a combination of land surface temperature and sea surface temperature.

I have three main comments:

Comment:

1. As suggested in the title and in the introduction, the purpose of the paper is to quantify the contribution of the GIS to LIG warmth. However, my feeling is that at the end of the paper, the reader is not left with a precise message answering the purpose of the paper.

Reply:

We have tried in the revised version of the manuscript to write it in a way that the message becomes more clear by the end of the paper.

Here are some suggestions that should participate in resolving this issue:

Comment:

- Up to 15 simulations have been run leading to model outputs presented in 11 figures in the main manuscript and 19 figures in the Supplementary Material. I think that the authors should re-consider if all the simulations and outputs they show are necessary and participate in improving our understanding of the climatic processes during the LIG and the role of the Greenland ice sheet. In particular, I am not sure I understand why the simulation testing the methane effect is relevant in the context of this study (see comments in the "specific comment" section). Also, is it really necessary to leave the simulations for 115 ka since, as far as I understand, they are hardly discussed in the manuscript?

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.

We have removed some of the simulations that do not directly relate to the main topic. The GI (115 kyr BP) and HOL-x0.5 (6 kyr BP) simulations are removed in the new version of the manuscript.

Comment:

- This big number of simulations results in a Result Section which is too long and too descriptive. I find it hard to read and difficult to extract the key messages. A big effort of synthesis would be necessary to propose a more concise description of the results (i.e. the authors could focus on similarities and differences between simulations in some key regions). I think it would be very useful if the authors could provide a more critical point of view on the various simulations they present and discuss in a clearer way for instance which extent and height to the Greenland ice sheet leads to the results the closest to the data and also what should be the most appropriate simulation to represent the LIG climate.

Reply

We have shortened the Results section and structured it in a more concise way.

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 discussed 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 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 in the original manuscript is because both proxy datasets from CAPE Last Interglacial Project Members (2006) and Turney and Jones (2010) indicate a significant warming in the Northern Hemisphere, therefore we 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 comparison 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.

It is not easy to indicate what should be the most appropriate simulation to represent the LIG climate. The preindustrial configuration of GIS in the LIG-ctl simulation is not considered the most "realistic" since there is strong evidence that the GIS elevation and extent were lower during the LIG than the PI. The main question is what was the real height of GIS during the LIG, a subject that is still under debate. In our study, we decided to consider simulating a more dramatic change, namely about half its preindustrial elevation. The most "realistic" simulation with GIS reduction is LIG-1300m-alb because the albedo is adjusted accordingly where the ice is removed, though one must be cautious since as we already mentioned the reduction of GIS in this simulation is dramatic. The other two simulations with a different representation of the GIS were run with ice albedo everywhere above Greenland, though there were ice-free areas during the LIG. We use a rather simplistic representation of the GIS in our model simulations, as our main interest is to quantify the effects of changes in GIS on the global climate rather than local.

Comment:

-This comment applies as well for the discussion section. It should be shorter and more to the point. But in addition, I think the paper would be improved with a more critical (rather than descriptive) comparison with other published works in order to better highlight its added value.

Reply:

We have shortened the Discussion section and provide a more concise discussion of our results.

Comment:

2. My second comment concerns the comparison of their model results with existing LIG data

synthesis. The authors neither use or mention the recent data synthesis for the LIG from Capron et al. (2014) combining ice core and marine sediment records covering the high-latitude regions (latitudes above 40°). This new data synthesis is the first one providing a coherent temporal framework between records and thus accounting for the non-synchronicity between records from different regions during the interval 130-115ka rather than presenting one single snapshot representing the LIG maximum warmth such as in previous work. These time series represent appropriate targets for transient simulations. In this paper, we also built 4 time slices at 115, 120, 125 and 130 ka describing SAT and that represent also improved target for snapshot simulations for these time periods.

The authors should consider using this improved data synthesis to discuss their climate simulations. I might be missing information but from what I can extract from their conclusions, the main outcomes of the studies seem to be rather similar to the ones from previous studies, i.e. although a reduction in GIS elevation and extent improves the agreement between model and data, the simulated SATs underestimate the temperature changes indicated by the proxy reconstructions. I think that confronting the simulations with the new datasets (for the high latitude regions) could add an additional dimension in the novelty proposed in this paper. In addition, it provides information about Greenland and Antarctica from ice cores while at the moment, the authors do not discuss these regions in term of model-data comparison.

The authors should not hesitate to contact me. I will be happy to answer to any questions they could have regarding this new data synthesis.

Reply:

We thank Emilie Capron very much for providing us with the new proxy-based dataset (Capron et al., 2014). It is now included in the new version of the manuscript. However, due to the large amount of data we have chosen to include only the temperature anomalies for the 130 and 125 kyr BP time slices in our model-data comparison. Since the focus is on the 130 kyr BP, we have included the 125 kyr BP in the Supplementary material. However, we find a best agreement for 125 kyr BP, though we cannot conclude whether a reduction in GIS contributes to the agreement, since we do not have other simulations with different GIS configurations for this time slice. We can only compare different GIS changes for the 130 kyr BP time slice. In these simulations, the best fit occurs when the marine proxy-based temperature anomalies are compared to LIG-ctl simulation. A reduction in GIS leads to a small warming in the North Atlantic Ocean due to an increase in the Atlantic Meridional Overturning Circulation (AMOC) which transports more heat northwards. Since most of the records are located in

the North Atlantic Ocean and most show a negative anomaly with respect to the present, a reduction in GIS does not improve the model-data comparison. However, the differences in TS between the preindustrial GIS and reduced GIS simulations are rather small in the North Atlantic Ocean. In general, COSMOS seems to simulate cooler temperature anomalies as compared to the CCSM3 and HadCM3 climate models (Capron et al., 2014). This is probably caused by the GHG concentrations which are higher in the CCSM3 130 kyr BP simulation than in our COSMOS 130 kyr BP simulations. However, HadCM3 indicates as well warmer anomalies than the COSMOS, though the GHG concentrations are smaller in HadCM3. One factor that may counteract the effect of GHGs is the vegetation which is dynamic in our simulations. We find a similar response in our transient simulations when we compare LIG temperature evolution from two simulations with and without dynamic vegetation. Though the GHG concentrations are predominantly smaller in the LIG-GHG-tr simulation than in the LIG-ctl-tr, the former simulation gives higher temperatures than the latter. The only difference between the two simulations, besides GHGs, is the vegetation. When used dynamically, the vegetation can lead to a cooling in the North Atlantic Ocean.

We have included also model-data comparison of LIG trends between 125 and 115 kyr BP from Capron et al. (2014) and from our COSMOS LIG-1300m-alb-tr and LIG-ctl-tr simulations, but because the main focus here are anomalies and due to the large amount of data, we have decided to include it in the Supplementary material. The comparison with the ice cores is provided in the text only, in order to limit the number of figures.

Comment:

3. My third comment relates in a more general way to the form of the paper: I find the manuscript long and unfortunately, too much information leads to the blurring of the main findings and makes it difficult to extract the most important results and their implication. I think that it originates from the three following reasons which should be fixed in the revised version:

Reply

We thank Emilie Capron for the suggestions. We have considered them in the revised version of the manuscript.

Comment:

- Some sections have excessive details, in particular in the Results and Discussion Sections. Specific

paragraphs are highlighted and suggestions to shorten the text are given in the Specific comment" section of this review. I think the authors should keep this comment in mind for the whole manuscript when preparing the revised version.

Reply

We have removed some of the details and simplified the story and the text in the Results and Discussion sections of the revised manuscript.

Comment:

- It is also related to my first main concern related to the number of simulation outputs presented. I think that not all simulations and shown model outputs should necessarily be kept or if the authors really think they are all necessary, then, a strong effort of synthesis needs to be done.

Reply

As mentioned above, we have removed some of the simulations that do not relate to the main topic (GI and HOL-x0.5). We have also reduced the part of the paper that covers the evolution of temperature during the LIG and moved the figures with middle and low latitude temperature evolution to the Supplementary material. Since we use the transient simulations for calculation of the maximum and minimum LIG TS, we have kept the figure with the northern high-latitudes temperatures as an example figure, in order to give the reader a feeling on how these transient changes in temperature look like.

Comment:

- The manuscript is also long because of some redundant information in some sections (e.g. introduction and discussion). I indicate them in the "specific comment" section. Overall, the revised manuscript should be written in a more concise way.

Reply

We have aimed in the revised manuscript to write the story in a more concise way and remove redundant information.

I detail below specific remarks mostly related to my comments above and also some technical corrections that should be taken into account when preparing the revised version.

2. Specific comments:

Abstract:

Comment:

It needs to be re-written to clarify the main results of the study and make it more to the point. In particular, the authors should better highlight what new insights are provided by their study. In its current form, some information remains vague and sometimes unclear. Some of their conclusions are also similar to previous studies (e.g. problem of proxy seasonality, and chronology issues of the paleo-data). This is absolutely fine, however, they should try and better highlight why this is still of interest in the context of their new simulations (e.g. the fact that for the first time the height and the extent of Greenland is tested) and which results are specific to their work.

Reply

We have rewritten the abstract as suggested.

Comment:

P934, line 12: The sentence starting with "Reducing...." needs to be more specific. For instance : "...reducing the height by XX m...". Similar comment for "....leads to a warming of several degrees": Please, provide at least a temperature interval.

Reply

We have rephrased to "Reducing the height by ~1300 m and the extent of the GIS leads to a warming of up to +5°C [...]".

Comment:

P934, line 17: "with respect to the pattern". When reading the abstract, the reader may wonder if the authors mean a temporal pattern or a spatial pattern or both. Please, reformulate.

Reply

We have added "warming pattern".

Introduction:

Comment:

P936, line 13: this paragraph should be written in a more concise way. Although the sentence starting line 21 is slightly more specific, it is redundant with the sentence starting line 15.

Reply

We have rephrased part of this paragraph.

Comment:

P937, line 3: reformulate this sentence to : “Existing studies on the effects of a reduced GIS during the LIG have been centred mostly on the Northern Hemisphere and focused on implications related to sea level rise (Stone et al. 2013) and Atlantic Meridionnal overturning circulation (AMOC) (Bakker et al. 2012)”.

Reply

We have replaced the sentence as suggested.

Comment:

Also, please, don’t repeat twice the Bakker et al. (2012) and Stone et al. (2013) in the same sentence. In the same paragraph, two sentences later, the authors mention again these two studies. I think this paragraph could be shortened and still provide the same amount of information.

Reply

We have shortened this paragraph and wrote it in a more concise way.

Comment:

In this paragraph the authors should also add references to Loutre et al. (2014) who present some transient simulations for the LIG with an EMIC, as well as the study by Bakker and Rensen (2014) discussing the possible bias linked to the synchronicity hypothesis and that is cited later in the discussion in the current manuscript.

Reply

We have added these two references in the paragraph.

Comment:

P938, line 12 to line 18: Please, shorten the text to avoid redundancies.

Reply

We have rephrased and reorganized this part of the paragraph.

Comment:

P937, line 25: Papers by Capron et al. 2014 and Govin et al. 2012 discuss these issues more

extensively.

Reply

We have added these references.

Comment:

P937, line 25: “On cause of the model-data...". This paragraph needs to be reformulated as the model-data is firstly related to the fact that the LIG synthesis the authors refer to represent one single snapshot on the LIG maximum warmth, and thus they imply that maximum warmth occur synchronously across the globe. Once the authors have said this, they should add a sentence explaining that the reason of such an approximation is linked to the difficulty to combine time series from different types of paleoclimatic archives since they do not benefit from robust absolute timescale allowing precise temporal comparison between regions and between archives. This issue is widely discussed by Capron et al. (2014).

Reply

We have reformulated and added the information as suggested.

Section 2: Data and Methods

Comment:

P940, line 12: What is the specific interest to focus on the CH_4 effect rather than the CO_2 effect? I am not sure that the simulation testing the effect of CH_4 is particularly necessary and it doesn’t seem to me that the effect of methane on climate is very much discussed later on. The authors should consider removing it.

Reply

We have provided the explanation for using higher CH_4 concentrations in the reply of the first main comment. The changes in GHGs are not the main interest in this study, but since it was necessary to include a simulation with increased methane concentrations, we also looked at those results. However, in order to shorten the story, the figure with the effect of an increased atmospheric methane concentration on the TS is removed from the Supplementary material of the revised manuscript.

Comment:

P940, line 13: The simulation with GHG prescribed such as LIG-PMIP is an important simulation and

very appropriate for comparison with existing simulations that also follow PMIP recommendations. That's why the authors use it in the discussion. Thus I don't understand why it appears in the Supplementary Material.

Reply

The simulation with GHG concentrations as suggested by PMIP3 protocol (LIG-GHG) is actually not of particular relevance to the main topic of this study. We have included this simulation in the Supplementary material (now Fig. S1 in the revised version) in order to show how large is the impact of lower GHG concentrations compared to concentrations used in our LIG sensitivity simulations. We only want to show that there is not a large difference between using relatively lower and larger values when simulating the 130 kyr BP time slice. Thus, assuming linearity, the results of the LIG simulations with reduced GIS should be similar even when GHG concentrations as proposed by PMIP3 would be used. One has to take into account also that in Fig. S1 we see not only the effect of lower GHG concentrations but also of the vegetation, which in case of the LIG-GHG is fixed to PI, while in the LIG-ctl is computed dynamically.

Comment:

P941, line 13: The authors perform statistical tests to evaluate the significance of their results. Those tests highlight variations from one simulated parameter to the other in the total area that can be/cannot be interpreted and also in the geographical regions: My question might be naive but where does this come from? Why the significance of the results varies from one simulation from the others? this may deserve to be shortly discussed somewhere in the revised manuscript.

Reply

The statistical significance t-test between two simulations vary according to how large the anomalies are. The larger the anomaly the more likely it is significant.

Section 3: Results

Comment:

Some descriptions need to be removed in this section. At the moment, it is too long and I think it is easy to get lost into the details.

Reply

This section is shortened in the revised manuscript.

Comment:

Section 3.1: One way to shorten this section would be to present global SAT, Northern Hemisphere SAT, Southern Hemiphere SAT with annual, winter average etc... for the different simulations, in a Table to avoid the long text. In the text, the authors could only highlight the most relevant patterns and refer to the Table.

Reply

We have removed the detailed description of the TS averages, but we do not consider an extra table necessary since the absolute values of the global, Northern Hemisphere, and Southern Hemisphere TS averages in all equilibrium simulations calculated for annual, summer, and winter mean are given in Table 2 and the differences can be calculated from there. Instead, we focus now in the text only on the main pattern and differences between the simulations with reduced GIS.

Comment:

Section 3.2 needs to be shortened too and again with a focus on the important patterns for some specific key regions. However, I think the authors should highlight more clearly here that their simulations show that the timing of the maximum warmth is different between the winter signals and the summer signals (as seen in Figure 6).

Reply

We have also shortened this section, especially that we have moved to the Supplementary material, the figures with averages of middle and low latitudes TS evolution. We have also highlighted the differences in the timing of the maximum warmth between summer and winter.

Comment:

Section 3.3: This section is too long and need to be shortened as well.

Reply

This section is also shortened and more synthesized in the revised manuscript.

Section 4: Discussion

Comment:

This section should be shortened and should proposed more synthesized and critical discussions.

Reply

In the revised version we aim for a more synthesized and critical discussion.

Comment:

Section 4.1: In its current form, I don't think this discussion is very useful. I don't identify what is new relative to previous studies. It would benefit from being a bit more quantitative in the following sentence:

P956,line 22: "...a global warming of up to XX°C in our LIG simulations...." If the purpose of the study is to quantify the possible contribution of reduced GIS elevation in combination with insolation forcing, I would have expected a discussion on the relative effect of the insolation versus the effect of the reduced GIS elevation.

Reply

We have added the exact contribution of insolation to global annual mean warming. However, since the focus is the contribution of GIS changes to the LIG climate, we have rephrased the first sentence of the Dicussion section for more clarity and we focus more on a discussion on the changes in GIS rather than insolation. We additionally give an overview on which forcing is dominant globally and in the hemispheres during summer and winter seasons and annual mean.

Comment:

Section 4.2: This section is too long. On one side, it should be shortened and less descriptive: the first paragraphs of the section are somehow a presentation of results again. But I think also that on the other side, results should be discussed more in the context of previous studies. At the end of this section, the authors should emphasize better, the outcomes specific to their study about the influence of Greenland Ice Sheet elevation on surface air temperature during the LIG.

Reply

This section is also shortened in the revised version of the manuscript and we have tried to avoid redundancies and to clarify the main message of this study. A discussion in the context of previous studies is also included.

Comment:

Section 4.3: The results should be also discussed in relation with the recent transient climate

simulations for the LIG performed by Loutre et al. 2014 using the LOVECLIM model.

Reply

The study by Loutre et al. (2014) is now included in the discussion of the revised manuscript.

Comment:

Section 4.4: Section is too long and needs to be synthesized a lot. The authors should also better highlight what their study provided compared to the previous simulations of Otto-Bliesner et al. (2006, 2013) and Lunt et al. (2013).

Reply

This section is also shortened and better organized in the new manuscript version.

Comment:

Section 4.5: The ideas developed in this section need to be re-organized.

P966, line 10: the issue of dating paleoclimate archives should be the first thing to write as this is the reason why defining the timing of the maximum warmth of the LIG is so hard to define and why it results in data synthesis that perform some temperature averaging procedure and produce only one snapshot on the data synthesis. The authors should discuss their results with the recent data synthesis by Capron et al. (2014).

Reply

We have re-organized the section as suggested.

Conclusion

Comment:

The conclusion should be more concise but should more clearly state the implication of the study. For instance, in the end, is it possible to tell the simulation that seems to be the most appropriate to explain the data (Which extent? which height for the Greenland Ice sheet?). A couple of sentences about more specific perspectives for future work should also be presented.

Reply

We have re-organized the Conclusions section as well. A reduction in GIS improves the model-data comparison if annual mean proxies are used, since the GIS changes strongly influence winter season rather than summer, therefore when summer proxies are used a reduction in GIS does not reduce the

dissagreement.

3. Stylistic and typographic comments

P934.

Comment:

- Abstract: Add a sentence of perspectives at the end.

Reply

We have added.

Comment:

- line 1: "(LIG, ~130-115 kiloyear before present)". Please add the "approximative" sign as these numbers can vary slightly from one paper to the other depending on how the LIG is defined. For instance, in the IPCC AR5, it is defined based on the sea level variations from and is given as 129-115 ka (Dutton and Lambeck, 2012; Masson-Delmotte et al., 2013).

Reply

We have added the "approximative" sign.

Comment:

- line 8: to assess

Reply

Done.

Comment:

- line 10: "whole LIG and Holocene": for each one, please give the exact intervals for which the transient simulations have been run, i.e. 130-115ka and 8-0 ka.

Reply

Done

Comment:

- line 13: "leads to an ADDITIONNAL warming..."

Reply

Done.

Comment:

- line 24: instead of writing “deficits”, the authors should be more specific and evoke that there are likely still some remaining processes that are missing in the model (and cite a couple ?).

Reply

Done.

P935.

Comment:

- line 26: see previous comment for line 1, P934.

Reply

Done.

Comment:

-line 18: add the Turney and Jones (2010) paper in the list of reference.

Reply

Done.

Comment:

-line 23: the sentence “Proxy records...” and the sentence line 18 starting with “The Last Interglacial...” should be combined as they convey a similar message with the the sentence starting line 23 being more specific.

Reply

The idea behind the order of these sentences was to first state that the LIG was in general considered warmer than PI and then continue with model and reconstruction studies on the LIG warmth. If we would combine those two sentences, we think it would create confusion, especially with respect to the references.

P936.

Comment:

- line 18: “....ice core data proposes only a modest change, I.E. EQUIVALENT TO A CONTRIBUTION IN SEA LEVEL OF ABOUT 2 m”.

Reply

Done.

Comment:

- line 13: this paragraph should be written in a more concise way. Sentences starting line 15 and line 21 are repetitive with again the sentence from line 21 being more specific.

Reply

We have rephrased parts of this paragraph.

P937.

Comment:

-line 7: “...to a pronounced warming OF ABOUT XX”...” please, provide a quantitative estimate.

Reply

Done.

Comment:

-line 24: Please reformulate the sentence such as: “ The lack of accurate and independent age models for most paleoclimatic record during the LIG could be one cause for the observed model-data discrepancy”.

Reply

Done.

P938.

Comment:

-line 14: “....of transient simulations of the entire LIG (GIVE TIME INTERVAL)”.

Reply

We have removed this part from the sentence after rephrasing the paragraph for clarity.

P940.

Comment:

The authors should indicate clearly in the experimental setup section the time slices that are performed (mid-holocene, 130, 125 and 115 ka, etc...)

Reply

Done.

Along those lines:

Comment:

-line 5: Please reformulate " 3 equilibrium simulations covering the LIG are performed, using fixed boundary conditions for the 130 ka, 125 ka and 115 ka time slices".

Reply

We have reformulated as suggested without the 115 kyr BP time slice since this is removed in the revised version.

Comment:

-line 13: please reformulate : "An additional simulation is performed using VALUES for GHG concentrations proposed in the ....(PMIP3) FOR THE TIME INTERVAL XX ka (E.G. LUNT ET AL. 2012) AND CORRESPONDING TO 257ppm for CO2, 512ppm for CH4 and 239ppbv for N2O.....at 130 ka".

Reply

Done.

P944.

Comment:

-line 4: replace chapter by section.

Reply

Done.

Comment:

-line 7: it would be good to be consistent with the amount of digits given when providing quantitative estimate of SAT for instance, at the moment: "+11.1°C", "~2°C", +0.36°C"...

Reply

We agree that is is important be consistent. However, when we give approximations like "~2°C", we do not think is necessary to add digits. Similarly, when giving estimates like "up to +11.1°C" it depends on the case. In results from our study, we are able to provide one digit but not for estimates taken from other studies. We give the two digits when we calculate temperature averages or trends because in some cases the differences in the TS of different simulations are rather small.

Comment:

- line 16: "...LIG-x0.5 RELATIVE TO LIG-CTRL."

Reply

Done.

P945.

Comment:

line 4: "...the Sea of Okhotsk (WESTERN PACIFIC OCEAN)"

Reply

Done.

P959.

Comment:

-lines 9 to 15. Please be more concise. This is not necessary to describe again all this. The justification of the latitudinal band should not appear in the discussion section.

Reply

Done.

P968.

Comment:

-line 14. Please reformulate the first sentence to : "....general circulation model AND ASSESS THE INFLUENCE OF THE GIS ON GLOBAL CLIMATE. And "we employed..."sentence can be removed.

Reply

Done.

Comment:

-line 19. Please be more specific and add an example: “a reduced GIS of XX m”, “ the warming by YY°C”,

Reply

Done.

4. Tables and figures

Comment:

Figure 2.

- I suggest to remove here and in the rest of the captions for other figures the expression “...at the beginning of the LIG (130ka) and replace it simply by “...in the 130 ka simulation.”

Reply

Done.

Comment:

Figure 3.

-Please reformulate first sentence such as: “Effect of Greenland Ice Sheet elevation and albedo on SAT at 130 kyr BP”.

Reply

Done.

Comment:

Figure 4.

-the violet dashed line is hard to see.

Reply

We have changed the colorbar of all maps in order to distinguish easier between different shades. The violet dashed lines are therefore now easier to see. Furthermore, we could not find a better visible color.

Comment:

Figure 8.

I am not convinced that the values of RSMD should appear in the caption of the figure. Please consider providing a comparison with the recent 130 ka data time slice produced by Capron et al. (2014).

Reply

We have removed the RMSD values from the figure captions and created three tables in the Supplementary material of the revised manuscript, one table for each dataset: CAPE Last Interglacial Project Members (2006), Turney and Jones (2010), and Capron et al. (2014).

5. References

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