**Review of the manuscript "Greenland Ice Sheet influence on Last Interglacial climate: global sensitivity studies performed with an atmosphere–ocean general circulation model" by M. Pfeiffer and G. Lohmann**

## 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 it.

I have three main comments:

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

- 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 ?

- 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.

-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.

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.

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:

- 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.

- 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.

- 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.

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. <u>Specific comments:</u>

- **Abstract:**

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.

**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.
**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.

- **Introduction:**

**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.

**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)".

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.
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.

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

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

**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).

- **Section 2: Data and Methods**

**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.

**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.

**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.

- **Section 3: Results**

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.

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.

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).

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

- **Section 4: Discussion**

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

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.

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.

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.

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).

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).

- **Conclusion**

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.

## 3. Stylistic and typographic comments

**P934.**
- Abstract: Add a sentence of perspectives at the end.
- 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).
- line 8: to assess
- 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.
- line 13: "leads to an ADDITIONNAL warming..."
- 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 ?).

**P935.**
- line 26: see previous comment for line 1, P934.
-line 18: add the Turney and Jones (2010) paper in the list of reference.
-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.

**P936.**
- line 18: "....ice core data proposes only a modest change, I.E. EQUIVALENT TO A CONTRIBUTION IN SEA LEVEL OF ABOUT 2 m".
- 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.

**P937.**
-line 7: "...to a pronounced warming OF ABOUT XX"..." please, provide a quantitative estimate.
-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".

**P938.**
-line 14: "....of transient simulations of the entire LIG (GIVE TIME INTERVAL)".

**P940.**

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

**P944.**
-line 4: replace chapter by section.
-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"...
- line 16: "...LIG-x0.5 RELATIVE TO LIG-CTRL."

**P945.** line 4: "...the Sea of Okhotsk (WESTERN PACIFIC OCEAN)"

**P959.**
-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.

**P968.**
-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.
-line 19. Please be more specific and add an example: "a reduced GIS of XX m", " the warming by YY°C",

## 4. Tables and figures

**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."

**Figure 3.**
-Please reformulate first sentence such as:
"Effect of Greenland Ice Sheet elevation and albedo on SAT at 130 kyr BP".

**Figure 4.**
-the violet dashed line is hard to see.

**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).

## 5. References


Bakker, P., Renssen, H., 2014. Last interglacial model-data mismatch of thermal maximum temperatures partially explained. Clim. Past 10, 1633-1644, doi: 10.5194/cp-10-1633-2014.

Capron, E., Govin, A., Stone, E.J., Masson-Delmotte, V., Mulitza, S., Otto-Bliesner, B., Rasmussen, T.L., Sime, L.C., Waelbroeck, C., Wolff, E.W., 2014. Temporal and spatial structure of multi-millennial temperature changes at high latitudes during the Last Interglacial. Quaternary Science Reviews 103, 116-133, doi: 10.1016/j.quascirev.2014.08.018.

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Loutre, M. F., Fichefet, T., Goosse, H., Huybrechts, P., Goelzer, H., and Capron, E., 2014. Factors controlling the last interglacial climate as simulated by LOVECLIM1.3, Climate of the Past, 10, *1541-1565*.

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Otto-Bliesner, B. L., Rosenbloom, N., Stone, E. J., McKay, N. P., Lunt, D. J., Brady, E. C., and Overpeck, J. T., 2013 How warm was the last interglacial? New model – data comparisons, Philos. T. R. Soc. A, 371, 1–20.