

## Response to reviewer#2

The authors analyze the role of sea ice and SST anomalies in the Labrador and Nordic Seas in controlling surface air temperature anomalies over the North Atlantic region (with a special focus on Greenland) during the Last Interglacial (LIG). Using the atmosphere component of CCSM4, a state-of-the-art climate model, a set of sensitivity experiments was performed to disentangle the influence of the Labrador Sea versus the Nordic Seas. The results were analyzed very carefully and in much detail considering heat and moisture budgets. It is found that sea ice retreat and warming in the Nordic Seas is crucial for the simulation of high Greenland temperatures during the LIG, which are evidenced by proxy records, whereas the role of the Labrador Sea is minor. The paper is well written and clearly structured. Although similar experiments and ideas have been published before by Li et al. (2010) with a focus on the last glacial, the results by Merz et al. are novel and show the importance of Nordic Seas ice cover for the LIG. As such, the study by Merz et al. is certainly of interest for the paleo-modelling community and suitable for Climate of the Past. However, the following points have to be taken into account before publication of the study.

We thank the referee for the careful review and the constructive comments. Please find the answers to all specific comments below.

1) p. 1, line 11: "Diabatic processes play a secondary role". This statement is confusing. The simulated SAT anomalies are ultimately caused by anomalous surface energy fluxes, e.g. sensible heating, which is a diabatic process. I think the authors refer to latent heating and radiative processes. Please be more precise.

We agree with the referee that our statement is confusing. What we meant is that the large-scale spreading of the warming is related to heat advection (of sensible heat) rather than to changes in condensation or radiation processes. We will revise the statement to clarify this issue

2) p. 1, line 23: In both models and data the LIG warming is mostly restricted to the extratropics, whereas the tropics show cooling in many regions. Again, please be more precise.

We agree and we will revise the introduction accordingly.

3) p. 2, line 13: The transient CCSM3 simulation used in this study was not part of the paper by Lunt et al. (2013). The CCSM3\_Bremen simulation in Lunt et al. (2013) is a time slice (125 kyr BP) run using the T31-version of CCSM3. It is different to the transient simulation by Varma et al. (2015). Please clarify.

Thank you for this accurate observation. We will clarify this issue, e.g. at the following passages: page 2, line 13 and page 6, lines 31-33.

4) p. 3, line 5: In addition to the papers by Li et al. (2005, 2010), cite the study by Zhang et al. (2014), which strongly supports the findings by Li et al., but in a fully-coupled setup.

We will add a respective reference.

5) p. 4, line 1: In addition to Varma et al. (2015), cite the studies by Bakker et al. (2013) and Govin et al. (2014), where the transient CCSM3 LIG simulation has been published first.

We will add the respective references.

6) p. 4, line 5: The two realizations do not only differ in horizontal resolution. Note that different greenhouse gas concentrations have been used as well as a different solar constant. Moreover, the transient character of the low-resolution run as well as the short integration time of the high-resolution time slice simulation should be taken into account. Please rephrase.

We add the following statement to clarify this issue:

*[Note that the two sets of EEM-PI realizations also use slightly different values for GHG concentrations and solar constant (Bakker et al., 2013 and Otto-Bliesner et al., 2013). Furthermore, the transient character of EEM lowRes is different from the time-slice approach of EEM<sub>highRes</sub>.]*

7) p. 5, line 26: How was the decision made on how far the sea ice margin is shifted - to the north? Is it based on the high-resolution LIG simulation or is it arbitrary? Please explain.

The character (direction and magnitude) of the shift was chosen to resemble the EEM-PI<sub>diff</sub> sea ice anomaly in the respective region (compare Figs. 4 and 5). We add a respective statement (page 6, line 4) to make this clear:

8) p. 6, line 31: The authors have not used the CCSM3\_Bremen simulation from Lunt et al. (see above).

As discussed in response to 3) we clarify this and revise the respective statements.

9) p. 7, line 1: As mentioned above, the difference is not only due to horizontal resolution. Different GHG concentrations have been used. In particular, N<sub>2</sub>O concentration is much higher in the high-resolution CCSM3 experiment than in the low-resolution run. Moreover, a higher solar constant (1367 W/m<sup>2</sup>) has been used in the high-resolution experiment.

Thank you for this correct observation. This will be accounted for as shown in response to 6)

10) p. 7, line 2: Vegetation is fixed (modern) in the transient CCSM3 low-resolution run.

True, this will be revised accordingly.

11) p. 7, line 11: As mentioned above, higher N<sub>2</sub>O and solar constant contribute to the warming in the high-resolution CCSM3. I agree that the ocean is also a likely candidate. In fact, as shown in Bakker et al. (2013) the AMOC in the transient low resolution CCSM3 simulation is relatively weak. Reduced oceanic heat transport would contribute to the relatively cool conditions in the North Atlantic. In addition, it should be noted that the pre-industrial reference run by Merkel et al. (2010) has much higher GHG concentrations than the transient LIG simulation (in particular CH<sub>4</sub>).

Thank you for this valuable comment. We will revise the following paragraph (p.7 line 9-16) to include the additional potential reasons for the diverse EEM-PI warming in the lowRes and highRes CCSM3 simulations.

12) p. 19, line 30: The study by Zhang et al. (2014) may be cited here, showing that processes are similar in coupled and uncoupled (Li et al., 2010) experiments.

We will add the respective reference and an additional statement to account for this valid observation. Your comment is very useful to further strengthen the credibility of our results.

13) p.36, Table 1: A reference is missing for the chosen GHG values.

The GHG values are chosen to correspond with Varma et al., 2015. We add a respective reference in Table 1.