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Climate of the Past Discussions

# *Interactive comment on* "Greenland warming during the last interglacial: the relative importance of insolation and oceanic changes" *by* Rasmus A. Pedersen et al.

**Anonymous Referee #1**

Received and published: 16 May 2016

The manuscript presents an interesting analyses of a long standing question 'how can we explain the large amount of reconstructed LIG Greenland warming relative to climate model simulations?'. By presenting multiple simulations with different forcing and boundary conditions a clean investigation of the role of different forcings can be made. Although I think the manuscript should be clearer at some points, and more in depth at others, I deem it suited for publications with minor revisions.

Main comments: In its current form it is not clear to me why calculations of SMB are included. This part of the manuscript should be better introduced and embedded. Moreover, is it possible to use the SMB calculations to provide a rough estimate of the amount of surface elevation change that would result during the LIG? This would be



a great addition to the presented work and clearly show the added value of including SMB calculations.

In the presented study, a fixed seasonal calendar is used. Although common practice in palaeoclimate modelling, it seems to me that in this study the biases that this assumption introduces might be of importance. Several publications have shown that the impact of using a fixed seasonal calendar rather than a fixed angular calendar has largest impact during LIG NH autumn. The results presented in this study have a strong focus on seasonal changes, thus it should be explained what the possible impact of using a fixed seasonal calendar could be.

The presented study uses oceanic boundary conditions from a previous coupled simulation. Although it is outside the scope of this study to discuss in detail what caused the imposed changes in ocean surface temperatures and sea-ice cover, a short description of the relevant changes, causes and uncertainties is important to interpret the presented results. More specifically I'm thinking about the changes in the AMOC strength, sea-ice changes and changes in deep convection in the North Atlantic region. What drives those? Are there indications from palaeo climate reconstructions that such changes are realistic and how does that impact the presented results? The pre-industrial sea-ice cover in EC-Earth seems rather extensive, does that impact the LIG results?

It is described that the simulated changes at NEEM are not in accordance with reconstructions. Related to that, how does the EC-Earth simulation compare to previous model results (for instance Lunt et al, 2012, Landais et al. 2016), could these differences partly explain the mismatch between EC-Earth LIG NEEM temperatures and reconstructions?

Minor comments: In the manuscript the terms direct and indirect are often used, are they in this context the same as forced response and feedbacks? Please clarify and consider updating in the manuscript.

Page 1 lines 12-14: The link of this section to the title of the manuscript is not clear. Please clarify.

Page 2 line 1: Consider including Landais et al. 2016 at this point since that study also compares model results specifically for Greenland.

Page 2 line 20: Is the suggested elevation change really limited. The given uncertainty could also indicate that in fact the changes where on the order of 300m, implying a temperature effect on the order of 2 degrees.

Page 2 line 24: 'Local conditions' is a little vague, what are you referring to, please clarify.

Page 3 lines 28-29: 'and consequently..' this is perhaps too obvious. Consider removing.

Page 4 line 20: 'Ice sheets. . ..' This has been mentioned before, consider removing.

Page 5 lines 9-11: Are these areas of snow cover changes over the ice sheet or over vegetated land, and related, how large is the albedo change that likely caused these patches of strong warming?

Page 6 lines 3-4: What kind of non-linear processes are causing this behaviour? Please clarify.

Perhaps not so much a comment as a question: it appears that precipitation and snow have an almost one-to-one relationship, higher temperatures result in more snow fall, is that correct? Is that simply because warm air can hold more moisture, or is there also an effect from changes in sea-ice cover and moisture availability?

Table 2: The sum of the iL+oP and iP+oL experiments is quite different from the iL+oL result. Is this because of non-linear effects and if so, what are they? Internal variability?

Technical comments:



Throughout the text double brackets are found in references. Perhaps a latex issue, but consider using only single brackets.

The word 'thus' is used a little often, consider replacing with synonyms or restructuring some sentences.

Page 3 line9: remove 'the' before GrIS.

Page 3 line 12: remove 'the' twice in this sentence.

Page 5 line 7: Remove 'the' in front of 'peak'.

Page 5 line 7: perhaps 'but also the central, high. . .'?

Page 5 line 13: Confusing sentence, please rewrite.

Page 5 line 29: Please rewrite because from this it could be understood that topography changes are driving precipitation changes, while no such topography changes are applied in this study.

Page 6 line 4: should this be eastern GrIS?

Page 8 line 1: 'appear', are they or are they not in your experiments?

Figure 1: the curve for 80.2N has a spurious kink between August and September, is this real or somehow an artefact of the calculations or plotting?

Figure 2: the direction labels are unreadable. Perhaps the figures can be made larger in general because the sea-ice extents are also difficult to see.

Figure 3: why are only changes larger than 0.5K shown, it appears to me that they are shown in white.

Figure 3 and 4: Perhaps in both figures the sum of iL+oP and iP+oL can also be shown to compare to iL+oL in order to highlight the non-linearities? Or the difference between the sum and iL+oL?

Figure 4 and 5: Why are the ocean areas masked out?

Figure 5: It seems the annual mean temperatures are even more non-linear when considering the individual and combined forcings. Please discuss.

Figure 6: remove ‘iP+oP’ from titles in line with the other figures.

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