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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 #2

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This manuscript evaluates the role of insulation and sea surface temperature changes on the Greenland temperature during the Eemian. This work is interesting and valuable as it could offer insights into (i) the drivers of sea level changes during that period, (ii) the drivers of climate change and (iii) the reasons for the discrepancy between modelled and reconstructed Greenland temperature. The work carried out is sound and well described (apart from a few minor clarifications that need to be made), but the implications of the results are not sufficiently well presented and some of the analysis needs to go a bit further. This paper could have a lot more impact with a little bit of adjustment to the manuscript and a little bit more analysis of the result. I therefore suggest the manuscript to be accepted after some corrections and clarification. These

would be a bit more than minor revisions, but i don't anticipate they would require too much work.

Overall the manuscript does a good job at describing the changes associated with SST and insulation forcings, but does discuss the reasons of these changes. In particular, I would like to see some explanation of the role of insulation on precipitation seasonality.

There is some mention of 'non-linearity' effect, but this is very much glanced over. It needs more description of what that means, how strong the non-linearity is and what causes it.

In the discussion and introduction, clarify that part of the SST changes are caused by insulation and that this study focuses on the direct effect of insulation vs ocean temperature changes. Also, there should be a discussion of how well the model simulates modern Greenland temperature and how that would impact interpretation of the results. For example, some GCMs have difficulties simulating Arctic cloud processes. Could that affect the sensitivity of the model to changes in insulation/SSTs ?

The conclusions of the manuscript are a bit underwhelming. The start of the manuscript suggests that this study could shed light on the reasons for model-data discrepancy regarding Greenland Eemian temperature. The paper concludes that changes in ice sheet topography are to be blamed, but that is precisely a factor that the paper was not including. Is there nothing to be learned about the model's sensitivity to insulation and SST changes ?

Finally the mass balance calculations are really interesting and valuable, but the results are a bit lost in the manuscript which is a real shame.

Other minor comments: Line 20: "While the ice core air content only suggests limited elevation changes at the NEEM site (45$\pm$350 m higher than present ice sheet elevation), the NEEM ice core temperature reconstruction has been corrected using the surface elevation change estimate from the ice core air content (NEEM commu-

nity members, 2013)." A lot of repetition in this sentence which I find a bit difficult to understand, so I suggest modifying it.

Section 2.3 page 2, line 20. Reference for the SST and sea ice boundary conditions. Is this from Pedersen et al. (2016b)?

Section 2.3 line 27: clarify, what the impact of insolation on SST changes is based on ? is this again from Pedersen et al. (2016b) ?

Page 5, line 29: "The simulated responses reveal that the ice sheet topography is important for the precipitation changes: Figure 4 30 reveals several examples of contrasting snowfall changes on the east and western side of the ice divide." I understand what is meant here, but I would suggest clarifying this statement as the readers may confuse (i) the control that topography has on the pattern of climate change observed, with (ii) the effect of topographical changes not included here.

Figure 6 add label for "effect of SST" "effect of insolation" above the subplots to help the reader understand the results.

Page 6, line 31. This paragraph needs more discussion. The second sentence is not enough to justify the non-linearity. I suggest formalising slightly more the factor decomposition to calculate the interaction between ocean and insulation forcings (see Stein and Alpert) or at least state that adding the two effects does not give the full temperature change. Also, add a discussion of the reasons for this. Why is this non-linearity different for precipitation-weighted and absolute temperature difference? Can you explain the processes that lead to the non-linearity ?

Stein, U., Alpert, P., 1993. Factor Separation in Numerical Simulations. Journal of the Atmospheric Sciences 50, 2107–2115.

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