

# ***Interactive comment on “Link between the Surface Mass Balance of the Greenland Ice Sheet and the North Atlantic Oscillation under preindustrial and last interglacial climates: a study with a Coupled Global Circulation Model” by Silvana Ramos Buarque and David Salas y Melia***

**Anonymous Referee #2**

Received and published: 8 May 2018

The manuscript studies the connection between the NAO index and SMB of the Greenland ice sheet using a set of experiments with an AOGCM for different orbital configurations. The model uses a configuration of increased spatial resolution over the region of interest, which improves the representation of some atmospheric circulation features compared to the standard CMIP5 configuration. Correlation analysis reveals spatial and temporal patterns of correlation between the NOA and SMB. Despite improved resolution, the representation of surface melt is poorly represented in the model. With

this, I feel there is limited confidence that the model is the right tool to study the NAO-\*SMB\* relationship. I have made two suggestions in the general comments below how I see the study may be modified to circumvent this problem, both requiring a substantial reworking of the material, i.e. major revisions.

— General comments —

The model does not perform well in simulating surface melt and runoff, which is an important component to the surface mass balance at present, it will become more important in the future and likely was important during the Eemian warm period. This implies an important caveat for interpreting links between the NAO and SMB as put forward in the manuscript. As it stands now, the shortcomings of the model in terms of melting are also not well presented, with contradicting statements (see specific comments below). I was wondering if the authors could focus on precipitation changes (instead of SMB) and their relation to the NAO as a more robust feature of the model. Another possibility may be to look at a precursor of melt, like the 700 hPa temperature, which appears to be a good predictor for surface melt according to Fettweis et al. (2013a).

While it is recognised in the manuscript that earlier research has shown that "changes in atmospheric circulation" are responsible for a large part of the summer warming in Greenland (citing Fettweis et al., 2013), an important distinction put forward by Hanna et al. (2013) is not further discussed: they find that "Greenland coastal summer temperatures and Greenland Ice Sheet (GrIS) runoff since the 1970s are more strongly correlated with the Greenland Blocking Index (GBI) than with the NAO Index". In the context of the present paper concerned exactly with the relation between atmospheric circulation and GrIS SMB, it seems in place to also discuss the Greenland Blocking Index. Possibly the model in this study does not represent the GBI nor the relation to Greenland SMB very well. In that case, this should be clearly presented and discussed as another limitation of the model.

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The correlation analysis is an important element of the manuscript and reveals important spatio-temporal differences in the relationship between NOA and the Greenland SMB (or at least precipitation, see above). What I miss in the paper is a step beyond the correlation analysis to help the reader understand what this work really implies. Should we expect a stronger influence of the NAO on Greenland in the future or during the Eemian? What would that imply for a possible distribution of melt and precipitation changes? Does the seasonal difference in the relationship play an important role now and how will that change in the future?

In the introduction, the study is fully motivated with a perspective on the future. Given the different forcing mechanism between the Eemian and the future (orbital vs. GHG), one could question if the chosen experiments (130 and 115 kyr BP orbital configuration) are really a good choice to learn something about future changes. In my opinion, the future perspective could be a much less important element in this paper and more focus be placed on understanding the Eemian climate itself. While the idea to learn something about the future by looking at the past is one of the well established and accepted motivations in paleo research, the opposite perspective can also be rewarding and should probably be added for a more balanced view. The fate of the GrIS during the Eemian e.g. remains a scientific problem of high relevance, which could be mentioned and discussed.

— Specific comments —

L22: Distinguish between Fettweis et al., 2013a and 2013b in the manuscript.

L42: Could you please clarify the term "surface temperature feedback". Often a feedback is named mentioning two components that have mutual dependencies like SMB-surface elevation feedback or surface temperature - albedo feedback.

L61: "Better the link between NAO \*variations\* and ..."

L62: The terminology "warm and cool phase of the Eemian" may not be correct. I

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would refer to the studied time slices as "the warm climate of the Eemian" and "the cold climate of the penultimate glacial inception" or similar.

L67: The MAR model could be introduced much earlier, e.g. when discussing results of Fettweis et 2013 (L35ff).

L78-81: This is a confusing description. As long as there is no coupling to an ice sheet model, it is standard for an AOGCM to operate with a fixed surface topography over land. As far as I can see it, this has nothing to do with technical requirements of the snow pack model as described here. It would be interesting to describe instead if and how the snow-pack model differs from other GCMs and from the MAR model, which I suspect has a full physical solution to the problems you are describing.

L81-83: Since there is no ice-dynamical process in this model at all, it seems strange to evoke the idea of a calving flux. I think it would be far simpler to say that all precipitation over ice-covered land is equally distributed over the ocean north of 60N, while the snow pack evolution is calculated diagnostically, without contribution to the mass budget. It should be clarified that the instantaneous relocation of this mass (freshwater?) as an additional forcing does not have any influence on the ocean response.

L86: A resolution of 40-50 km is still relatively low compared to the resolution of state-of-the-art regional climate models (MAR at 15km, RACMO at 11km). This should be mentioned here.

L120: If model bias and climate change signal are combined, how do we tell them apart? Is there maybe another experiment that could separate these two factors?

L124ff: Is this discussion really important for the GrIS? Consider discussing the biases for Greenland in more detail instead.

L169: Is it elevation or surface slope that has an important impact on precipitation amounts? Clarify.

L178: Clarify if this masking includes ice caps and glaciers in the periphery of the

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Greenland ice sheet.

L185: You attribute most of the underestimation of melt to the albedo limit. Why is that limit in place? Are there other shortcomings of the snowpack model worth mentioning? How does the snowpack model compare in complexity and included processes to the one in MAR? If resolution is an important limitation, how does the model compare to low resolution versions of MAR (Franco et al., 2012).

L194: Could add a few references after "Greenland" as a reminder.

L191: I strongly disagree with this statement. The model is clearly not reproducing the melting well and therefore shows considerable shortcomings to represent the SMB. The statement is in clear contradiction to the description L184 and L312.

L195: Could you please clarify if the NAO index is here calculated based on the normalised PC as described at line L156? In other words, is the NAO index definition the same for the ERA-based correlation with MAR SMB as the CNRM-CM5.2 correlation with CNRM-CM5.2 SMB?

L215: Are "changes in precession" meant compared to pre-industrial or to other times during the Eemian?

L246: Could you find a better word instead of "node"? This is the first time this term is used. Maybe 'region'?

L310: Again, I think this statement may be true for accumulation, but clearly not for melting.

L317: There is "another hand" missing in this sentence or somewhere in the following.

L331: Not sure what "nibbled" means, please revise. Interesting to speculate on the impact of the Greenland ice sheet during the Eemian, extend if possible.

L344: This final statement may raise the suspicion that the findings in this paper are not yet established to be robust and may be subject to change. Maybe just a question

of formulation. Revise.

Figure 2 Precipitation is defined positive. Maybe adjust the colour scale accordingly?

Figure 4 This figure clearly shows that ablation and SMB are very poorly represented in NPS-0k.

Can you show the sublimation  $E$  subtracted from  $P$  to get accumulation  $C$  in the top panel (maybe as a supplement)? It seems to have a large impact on the resulting  $C$ . It also seems to have large spatial variability. Is that expected?

— References —

Franco, B., Fettweis, X., Lang, C., & Erpicum, M. (2012). Impact of spatial resolution on the modelling of the Greenland ice sheet surface mass balance between 1990–2010, using the regional climate model MAR. *The Cryosphere*, 6(3), 695–711. <http://doi.org/10.5194/tc-6-695-2012>

Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2018-12>, 2018.

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