

Interactive comment on “Greenland Ice Sheet sensitivity and sea level contribution in the mid-Pliocene warm period – Pliocene Ice Sheet Model Intercomparison Project PLISMIP” by S. J. Koenig et al.

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RESPONSE TO REVIEWER A. Carlson (Referee)

My co-authors and I wish to thank the referees for their careful reviews. We believe we have addressed most (if not all) of the points raised. Point-by point explanations and responses to the individual points raised are given below.

This paper presents the ice-sheet model results of the PLISMIP study; a major step

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for- ward in our understanding of Greenland ice sheet evolution under mid Pliocene warm conditions with implications for future Greenland ice sheet behavior. The authors find that the climate forcing via a GCM is a larger source of uncertainty than any one ice- sheet model's physics. I like the paper and it should be published with some minor revisions (namely, the references have some problems).

> We highly appreciate rev1's recommendation that the manuscript should be published with "minor" revisions.

After reading this paper and returning to it a few days later to write this review, I'm left feeling that the discussion is somewhat thin; this is a large endeavor and can any insight into new directions be given beyond that the GCM is a larger uncertainty? Such a large international endeavor, I think, should start banging the drums on what we don't know to push for further international efforts and also give some reason for say IODP drilling that can help not only with ice-sheet reconstructions, but also the needed climate (the PRISM data set is actually quite poor/absent around the Greenland - the actual 'dated' data that comes from this 1/8 of the world is on the other side of ocean fronts from Greenland; exception Eirik Drift which they've cut out from the more recent summaries).

> We agree with the notion of rev1 that new, high quality data close to the proximity of Greenland are key for advancing the understanding of Pliocene Greenland. This is particularly true when applying a combined approach of modeling and data reconstruction.

The very last sentence of the "Conclusion" section argues in favor of that. It is well positioned at the end of the manuscript to "bang the drums" for the inherent need to corroborate our modeling results with new data in the proximity of Greenland.

Page 2824 9 - Add in Robinson et al. 2012 Nature Climate Change and Levermann et al. 2013 PNAS to this list of GrIS response to anthro changes 17 - Add in Robinson et al. 2012 here again, they show hysteresis. 22 - this reference list has problems.

Rasmussen et al., Dahl-Jensen et al. and Johnsen et al. are all ice-core studies of $\delta^{18}\text{O}$ change that are not over the last few interglacials. NGRIP and NEEM should be referenced for this. Overpeck et al. is really just a summary of insolation and sea level data. I would cut. Cuffey & Marshall are a modeling study that should be moved to the next reference list. The authors neglect actual records of ice-sheet extent and should add these here - Colville et al. (2011, Science) and Reyes et al. (2014, Nature) - that constrain the G_{1S} during MIS 5e and 11 (actually only studies to do this so far on margin constraints).

> Done.

Page 2825 8-13 - the last summary of the Kap Kobenhaven concluded it was ~ 2.4 Ma or younger in age (Funder et al., 2001). It is thus in the Quaternary, not the Pliocene. The authors may confuse the fact that up until ~ 5 years ago, the Pliocene extended up to 1.8 Ma. It now ends at 2.64 Ma. Regardless, current understanding would argue that this is wrong to include the KK in this summary. Likewise, the Ile de France is really really poorly dated and if correlative, is not from the Pliocene, but earliest Pleistocene (again, redefined Pleistocene). Willerslev et al. dated their only recoverable DNA from the base of Dye 3 to mid Pleistocene age (400 ka - 1 something Ma); this is not evidence for Pliocene forests. de Vernal & Hillaire-Marcel only presented high-res Pollen records back 1 Ma, not to the Pliocene. So remove the references to the poorly dated sites that are thought to be early Pleistocene. Also remove the Willerslev et al. and de Vernal & Hillaire-Marcel references as these are clearly Pleistocene stories. the place to get some info on the records is the cruise reports in ODP results volume 105 in 1989; de Vernal & Mudie for Baffin Bay site 645 and them again for site 646 on Eirik Drift.

> Removed the concerned references and added the review paper of De Schepper et al. 2014. The focus of the para now lies on the marine proxies.

Page 2826 1-3 - there is no reference given here and I know of no evidence/data that would say there were either substantial fluctuation in G_{1S} cover of Greenland during

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the Pliocene or intervals of little or no ice. The existing IRD records are the only actual proxy dated to that period and they just show IRD presence. I would remove this sentence.

> Done.

5 - Why not mention here the GIA effects that drove the Raymo et al. paper? Giving this range and not mentioning that it really is largely an effect of the ensuing glacial periods is quite important.

> We agree and point to the Raymo paper.

Page 2828 25-30 - How is precip lapsed? Not indicated.

> The precipitation fields are not adjusted with respect to orographic changes in the ice sheet simulations. Whereas temperature changes relatively linearly with height, precipitation does not. There are many factors that impact precipitation levels of which orographic height is only one. For example, one of the key parameters within precipitation fields is the circulation of the atmosphere, which cannot be simulated within an offline coupling of climate and ice sheet. Precipitation parametrization have to rely on a range of different climatological, many of which are simulated significantly less well than temperature and therefore it is considered that introducing such parametrization would introduce more uncertainty.

We also refer rev1 to the publication of Dolan et al. (2012) in Geoscientific Model Development, where the authors detail the experimental design. There they state:

Page 970: “Currently, there is no similar simple relationship [in comparison to temperature] between precipitation and altitude. Where downscaling methods do exist, the ratio of precipitation change with temperature change is poorly constrained. Therefore, no correction for precipitation is specified within the experimental design. If, however, modelling groups already prescribe a precipitation correction as standard within their ISM, this will be documented during the analysis of results.”

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Page 2830 11 - What about Bamber et al. (2013, The Cryosphere) updated topography and ice volume? This should be mentioned here and how the update may affect results as compared to modern.

> In order to be consistent with the experimental design for PLISMIP (as presented in Dolan et al., 2012) the results are compared to the earlier version of the Bamber et al. (2001) modern-day ice sheet reconstruction. Nevertheless, in the present manuscript we use Bamber et al 2013 in Table 2 as an additional measure for the calculation of mean and standard deviation. Please note that the estimated volume of the Greenland Ice Sheet differs by only $0.03 \times 10^6 \text{ km}^3$ (Bamber et al., 2013 vs. 2001).

Page 2835 15-16 - So, these vegetation reconstructions mentioned, at least sum, don't date to the Pliocene (i.e., Funder et al. and Willerslev et al.) - rather the early to mid Pleistocene. Revise and remove the Pleistocene references.

> Done.

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