Reply to comments of Referee #2

We thank referee #2 for an extensive and constructive review. Below we discuss all concerns and questions, and give a perspective of how we will address these in a revised manuscript. A more in-depth reply to the comments will be provided with the revised manuscript.

Referee #2: Understanding the stability of the Greenland Ice Sheet (GrIS) during higher than present temperatures is essential, in particular for deriving better projections of future ice sheet mass loss. The reconstruction of the extent and volume of the GrIS during the Last Interglacial period (LIG) has remained a challenge. Significant differences in published results are attributed to differences in applied climate forcing and the methods for coupling the ice sheet and the climate models.
This study adds to the growing literature of ice sheet melt and sea-level contribution reconstructions from Greenland Ice Sheet melt during the LIG. However, it does not offer an improvement over previous estimates because of the limitations of the applied methodology. The approach employs SICOPOLIS, a thermomechanically coupled ice sheet model using the Shallow Ice Approximation (SIA) and the Norwegian Earth System Model (NorESM) without fully coupling the two components. Ice sheet melt is estimated using PDD modeling with temperatures adjusted to elevation changes by using spatially and temporarily constant lapse rates.

Referee #2: Although widely used for investigating ice sheet evolution in millennial to multi-million year time scales, the approach has several caveats. Most importantly, PDD factors of snow and ice are not constant in space and time and therefore the PDD method has a poor performance for reconstructing of melt-rate histories. Instead of the PDD method, the authors should have used one of the more realistic and accurate approaches. Such methods do exist, including the insolation-temperature-melt model from Robinson et al., (2011) or the SMB gradient method, introduced by Helsen et al. (Helsen et al., 2012, Coupling of climate models and ice sheet models by surface mass balance gradients: application to the Greenland Ice Sheet. The Cryosphere, 6(2), 255–272. http://doi.org/10.5194/tc-6-255-2012).
Reply:
Indeed, the PDD method is widely applied in paleoclimate research. The main reason for this is its simplicity. Yes, we agree that the parameterization is very simple, but that also prevents over-tuning of the climate forcing. As we show in the manuscript, even for recent past warm time periods such as the last interglacial, the climates models simulate very different climates. High resolution regional climate models will give better spatial resolution, but these are only as good as the global models that force them.

We appreciate the reviewer's suggestion of using alternative approaches. The first suggestion, the ITM model, includes an additional factor which is direct related to insolation. However, the added value of including insolation as a direct forcing for surface melt also has its challenges due to the added uncertainty of the transmissivity of the atmosphere (an essential parameter in the ITM scheme). This transmissivity is taken as a linear function of elevation (Robinson et al., 2010), but this again (like in the PDD scheme) assumes that cloud characteristics remain constant over time. In the second method, the SMB gradient method of Helsen et al. (2012), the SMB is adjusted locally depending on elevation changes. The necessary SMB gradients are first derived from a regional climate model, applying linear regressions to SMB and elevation data within a small zone surrounding each grid point. This approach is quite sensitive to uncertainties in elevation, where an elevation uncertainty of ±100 m translates to an uncertainty of 1.2 m on the simulated ~2.1 m of last interglacial maximum sea level rise, dominating the total uncertainty (Helsen et al., 2013).

We realize that our results are likely to depend on the melt scheme applied. As suggested by the reviewers, we will investigate the impact of the different schemes and will adjust our manuscript accordingly.

Referee #2: Another main limitation of the study is that rather than evolving the ice sheet surface with the changing climate, the present-day ice sheet topography is used for the climate simulation. Relatively high elevations during the LIG likely cause an underestimation of the warming in the LIG, which could result in low estimates of ice loss. The authors dismiss the potential for performing a fully coupled model experiment as computationally too expensive at the present. However, this challenge is no longer prohibitive, as demonstrated by the first coupled ice sheet/climate model, which is currently in discussion in Past Climate (Goelzer, H., Huybrechts, P., Loutre, M. F., & Fichefet, T. (2016). Last Interglacial climate and sea-level evolution from a coupled ice sheet- climate model. Climate of the Past Discussions, 0, 1–34. http://doi.org/10.5194/cp- 2015-175).
Reply: While the work of Goelzer et al. presents a great technical effort, its results are limited by the relatively low resolution of the Earth System Model used (3 levels of approximately 5-6° spatial resolution in the atmosphere, compared to 26 levels and 3-4° spatial resolution in NorESM-L), the intermediate complexity model LOVECLIM, requiring an even larger amount of downscaling/interpolation of the simulated climate fields. Furthermore this study also applies the PDD method to simulate melt over the ice sheets. As discussed on page 15, lines 1-5, we refer to the study of Stone et al. (2013), who use a similar set-up as we do, but additionally tested how a smaller GIS affects the climate simulated by their GCM. It is unlikely that the GIS became ice-free during the LIG, and a small reduction of the ice sheet does not have a large effect on the large-scale climate as simulated by the GCMs.
We will further clarify this in the revised manuscript.

Referee #2: The authors argue that by using present-day observations and paleo ice core reconstructions to evaluate a range of model results corresponding to different parameter selection (basal sliding, PDD factors and lapse rate) they could obtain a robust estimate of LIG GrIS lost, despite the significant model simplifications. However, this appears to be contradicted by the fact that using the same methodology with similar parameterization but a different climate model that features warmer sea and air temperatures in N Greenland, Born and Nisancioglu (2012) have obtained a completely different pattern of LIG mass loss during the LIG with a strong ice sheet retreat in NE Greenland.
Reply: There are several large differences between the study of Born and Nisancioglu (2012), hereafter BN2012, and the one described here. Most important differences are: 1) Indeed the climate forcing was different (different models were used, as well as only 2 time slices), which has a major impact; 2) BN2012 initialize their LIG simulations with present-day ice and bedrock topographies; and 3) different data was used to constrain the model simulations. The combined effect of this is that BN2012 simulate a higher sea level contribution (see Figure 1). We will more clearly discuss the difference in the revised manuscript.

Referee #2: Detailed remarks: Page 2, lines 15-19: the ice penetrating radar mapping only detect the ice that is still present. However, Eemian ice could have thinned and melted in southern Greenland and therefore no longer present.
Reply: Correct, we will rewrite.

Referee #2: Page 5, lines 3-9: The modification of the global heatflux estimate from Pollack et al., 1993 by Greve (2005) results in an improved match of basal temperatures measured in the boreholes, but still provides an unrealistic distribution of geothermal heatflow. Rogozhina et al. (Rogozhina, I., Hagedoorn, J. M., Martinec, Z., Fleming, K., Soucek, O., Greve, R., & Thomas, M. (2012). Effects of uncertainties in the geothermal heat flux distribution on the Greenland Ice Sheet:

An assessment of existing heat flow models. Journal of Geophysical Research, 117(F2), F02025. http://doi.org/10.1029/2011JF002098) have shown a spatially uniform geothermal heatflow distribution provides the best fit to the borehole temperature observations.
Reply: Thank you, we were not aware of this study. We will test what effect a different heatflux pattern has on our results and include in the revised manuscript.

Referee #2: Page 5, lines 12-22: using the techniques mentioned above (ITM or SMB gradient) instead of PDD would eliminate the need of relying on the traditional lapse rate assumptions, thus improve the estimate of surface melt
Reply: The ITM method still relies on surface temperatures, but indeed for the SMB gradient method we could eliminate the need of the temperature lapse rate. Please see our discussion above concerning the implementation of the different methods. We will test the impact of using ITM in the revised manuscript.

Referee #2: Page 5, lines 22-25: what is the source of this particular parameterization – please include reference
Reply: This default parameterization in SICOPOLIS is based on Marsiat (1994); a reference will be included.

Referee #2: Page 7, lines 7-12: the motivation of varying the basal sliding parameter is not clear. I assume that the defaults SICOPOLIS value was selected to provide a good match for the PI GrIS. The basal sliding parameter, which is usually inferred by parameter assimilation, varies spatially and temporarily. It is not clear what improvement is offered by employing different, constant basal sliding parameters.
Reply: The basal sliding parameter is a tuned constant parameter in SICOPOLIS that relates the basal sliding velocity to the basal shear stress. This is commonly taken as a constant value, as the distribution of sediments and bedrock irregularities is not well known for the entire ice sheet. Parameter assimilation or inversion could give a spatially varying parameter, but without indications how this will vary over time. In order to avoid unknown details for longer simulations the parameter is kept constant. However, we assess the effect this constant value has on our ice sheet results. We will better clarify this in the revised manuscript.

Referee #2: MacGregor et al., 2015 reference is missing
Reply: We will add this.

Referee #2: Figure 2: why not using ERA-interim or an observation-based reconstruction (e.g., Bales, R. C., McConnell, J. R., Mosley-Thompson, E., & Csatho, B. (2001). Accumulation over the Greenland ice sheet from historical and recent records. Journal of Geophysical Research: Atmospheres (1984–2012), 106(D24), 33813–33825. http://doi.org/10.1029/2001JD900153)
Reply: We prefer to use the 40 years long reconstruction of ERA40, because we compare this data product to a pre-industrial climate simulation that does not cover the current climate warming, and is in an equilibrium state. We will better explain this in the revised manuscript.

Referee #2: Figure 3: why is the surface elevation in NorESM-L compared to ETOPO1? ETOPO is not used in the study – should it be Bamber et al., 2013?
Reply: On this scale (20 km grid of SICOPOLIS) the ETOPO1 and Bamber et al. (2013) topographies are very similar. However, to be more consistent we will adjust the comparison to use the Bamber topography in the revised manuscript.

Referee #2: Figure 5: Units of PDD factors should be added
Reply: We will include the units.

References:

MacGregor, J. A., M. A. Fahnestock, G. A. Catania, J. D. Paden, S. P. Gogineni, S. K. Young, S. C. Rybarski, A. N. Mabrey, B. M. Wagman, and M. Morlighem (2015), Radiostratigraphy and age structure of the Greenland Ice Sheet, J. Geophys. Res. Earth Surf., 120, 212–241, doi:10.1002/2014JF003215.

Marsiat, I. 1994. Simulation of the northern hemisphere continental ice sheets over the last glacial–interglacial cycle: experiments with a latitude–longitude vertically integrated ice sheet model coupled to zonally averaged climate model. *Paleoclimates*, **1**, 59–98.