Authors' response to comments by Reviewers

We are very grateful to Shawn Marshall and the anonymous reviewer for inspiring and very constructive reviews. Before addressing specific comments, we would like to comment on two points which were brought up by both reviewers.

Incomplete sampling. Both reviewers are perfectly right that more model parameters could be perturbed further and wider ranges of model variability could be explored. Unfortunately, the number of permutations increases as the square of the number of selected parameters. In our discussion paper, we present results of 270 simulations, each 250,000 years long, i.e., simulations totaling over 67,5 million model years. This only was possible because of the extremely high computational efficiency of the modeling tool we developed and the availability of a high performance computing cluster. Unfortunately, we are not in the position to increase the number of experiments by orders of magnitude. This is, however, not crucial since we did not claim that we explore the whole range of model uncertainties. In fact, we show that even modest variations of a single model parameter lead to large uncertainties in the Eemian GIS ice volume, which challenges some previous, over-optimistically accurate estimates of the GIS contribution to Eemian sea level rise. More importantly, confirming previous results by Tarasov and Peltier (2003), we demonstrated that Eemian simulations are still useful for constraining model parameters, which is also important for future climate change projections.

That being said, Shawn Marshall's proposal to include higher levels of warming was a very good idea and we have performed 90 additional simulations with a paleo factor value of 2.5. These simulations along with additional discussion will be included in the revised manuscript. In short though, the original conclusions of the discussion paper remain largely unchanged, because most simulations with higher boundary warming fall outside of the constraints.

Mass balance partition constraint. Neither reviewer was enthusiastic about our idea to use the mass balance partition criterion as the principal observational constraint, instead of the observed ice margins. This is not surprising, as this approach is at odds with the common practice of GIS model tuning to the observed "geometry". We wish to use this opportunity to advocate for our approach.

If we had a perfect GIS model, it would be able to simulate the ice sheet's geometry correctly together with the correct surface mass balance. However, the current models used for studying the long-term evolution of the GIS have a number of limitations. In particular, they do not resolve (and do not have right physics to describe) the so-called "fast" processes, i.e. ice streams and outlet glaciers. Some of the misfit with our model versions can be attributed to biases in the precipitation field produced by REMBO. But a large part of this must also stem from ice sheet model imperfections.

Figure R1 shows how a realistic ice sheet mask should look on a 20km model resolution grid (derived from the Bamber et al. (2001) data). It shows that only about 10% of the ice margin grid points of the ice sheet neighbor the ocean and that most of them are located in the northern, low accumulation areas. For such a modeled ice sheet geometry without fast enough flow through these points, the ice discharge into the ocean would represent a small fraction of the accumulation, i.e., in equilibrium, the simulated GIS surface mass balance should be close to zero. However, zero surface mass balance implies that the ice sheet is unstable (this was proposed by Gregory et al. (2006) and confirmed by our own analysis). Therefore, such a GIS would already be unstable for Holocene climate conditions which, fortunately, appears not to be the case. In reality, around 50% or more of the accumulated snow is discharged into the ocean through processes which, simply, cannot be resolved by our (and similar) models.

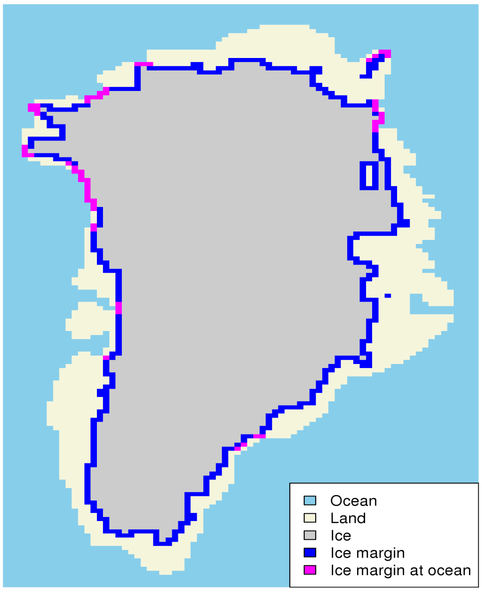

Figure R1. 20 km resolution land-ice mask derived from the present-day topography (Bamber et al., 2001).

Response to individual reviewer comments: Reviewer 1 (Shawn Marshall)

I can think of a couple of areas where the sensitivity tests/bounds do not bracket the full realm of possibility. In particular, Eem summer warming in the Greenland region in the large-scale model (with two grid cells over Greenland) is tested for 1.7 to 3.4 degC, but it is possible that peak Eem warming exceeded this (e.g. CAPE reconstructions of up to 5 degC). While warming of that extent may be unlikely, or may implicitly include internal feedbacks from albedo and elevation that are included in the modelling of this paper, it cannot be ruled out. Other parameters in the modelling, such as sliding and the melt model, have similar questions attached to them. There is a wide range of phase space explored here, but within the parameters of the models/parameterizations; other approaches to modelling basal flow and melt (e.g. a full energy balance within an RCM) might give sensitivities outside what is explored here. This warrants a comment I think. The main conclusions on GIS retreat during the Eem are thoroughly supported by the tests presented here, but they are not a complete sampling of what is possible and they are probably not the final word.

Response: We agree with the reviewer that although 5°C summer warming around Greenland is unlikely, the upper range of temperature can still be above the range of temperatures applied in our simulations. We therefore have now performed simulations with a higher value of the paleo factor (f_p=2.5, corresponding to boundary warming of 4.25°C) that will be included in the revised manuscript. While some interesting information can be obtained from these new experiments, our conclusions largely remain the same. As mentioned by Reviewer 2, the upper bound is controlled by the minimum allowed summit elevation and this threshold is reached in our simulations. This means that a constraint on the minimum summer Eemian warming would be most useful now for constraining both model parameters and the GIS contribution to Eemian sea level rise.

I do question one of the fundamental premises and constraints that the authors employ, the amount of modern surface melt/runoff predicted by the model. (Or more precisely, the fraction of accumulation that this makes up). By taking a fraction, the authors may mask large biases in the modelled melt, e.g. it could be biased to be both too melty and too wet. This would lend uncertainty to predictive skill going forward or backward in time, as there is no reason to expect such offsetting errors, if they exist, to balance out in the same way under a different climate regime.

Response: When we refer to the right mass balance partition, this also includes an assumption about the correctness of the absolute values. Precipitation over the GIS is solely controlled by REMBO, which was initially tuned to produce the correct total accumulation over the ice sheet (see Robinson et al. 2010). Furthermore, melt and runoff largely agree with estimates from RCMs, but uncertainty remains. In the current study, we primarily change the surface melt by changing parameter *c* in the melt scheme. The mass balance partition serves as a useful metric of how sensitive the surface mass balance is. Therefore, the correct "partition" simply implies the correct components of the GIS mass balance. We will clarify this issue in the revised manuscript.

Also, as noted in the attachment, it is not clear to me that the ice extent, which the authors argue we should ignore, is a less robust predictor of model skill than the fraction of runoff/accumulation. The authors argue that missing fast-flow physics and poor resolution and representation of the ice margins make for a poor prediction of ice extent unless melt rates are turned artificially high. (Sidenote: the authors should add to this list the poor representation of ice-ocean interactions and ice sheet losses at marine margins; these are processes that are not well-represented in all ice sheet models, but also compromise model skill at predicting ice sheet extent and mass balance). I agree with this but it is hard to isolate how the missing model physics affects ice extent vs. mass loss via calving. Both are compromised from the lack of model skill at ice-marginal and ice-ocean processes. Some areas that experience heavy melting in the model presented here would likely lose their mass 'first' through ocean melting and calving, in reality, if these processes were better captured in the model. This means that constraint that is applied, on surface melt totals, is not totally robust. Melt may be overestimated because some ice does not reach the ocean when it should, or does not melt from below or calve. If this occurs, it leaves 'room' for higher melt rates in other parts of the ice sheet, e.g. in some of the terrestrially-terminating regions of Figure 4 where too much ice is predicted, while still falling within reasonable amounts of total modelled melt.

Response: This comment is mostly addressed above. We want to add here that although the spatial distribution of surface melt for the simulated GIS is not identical to that diagnosed for the observed GIS topography, the total proportion of the components of mass balance (accumulation and runoff) are rather similar, which implies that mass partition criterion that we used is sufficiently robust. We agree in so far with the reviewer that total amounts are not necessarily unique. But we think that the overestimation of melt (locally or totally) is not the main point, if the mass balance criterion is used. We rather have the situation that the ice discharge is underestimated if we apply an area partition criterion; this is indeed a first order problem inherent to all classical ice modeling approaches. The point here is that a classical ice sheet model requires more contact with the ocean at the ice margins than a future, improved (super) ice sheet model would – one which includes fast flow, proper treatment of ice-ocean processes and basal hydrology, sufficient spatially resolved and physical representation of small outlet glaciers, etc. – in order to model enough ice discharge into the ocean. Still, we do not claim that our mass balance criterion is the ne plus ultra; but we do propose that such a criterion leads to results that are overall more consistent than an area distribution criterion, if fast model ice flow is missing. And, we do propose the usage of the mass balance partition criterion for such cases.

I think the constraint as applied by the authors is interesting and has some merits, but I don't fully trust it and would argue that ice extent should also be considered. I won't insist on this but like love to see a bit more discussion on this.

Response: We are very grateful to have the opportunity to discuss this point, since it is important, but we understand that we cannot fully convince the Reviewer with only verbal arguments. We have another paper in preparation that presents additional modeling arguments for our choice. As a final note, we can only compare our methodology with what has been done previously. Until now, most workers have chosen to modify/tune the climate and melt values in order to improve the present-day modeled ice geometry. We instead have chosen to tune the climate and melt to match the present-day as best as possible, and then coupled it to the ice sheet. What results with the latter approach is an ice sheet that is too large. Depending on the goals of the modeler, one approach may be more useful than another. It is clear to us though, it is not yet possible to model both the climate/surface and the ice sheet geometry correctly. This would require a new generation of ice sheet models.

Comments from the supplemental review (pdf comments)

It is not the isotopic composition as much as the total gas content that constrains this - there are some very enriched (warm) isotopic ratios in the Eem ice here

Response: Yes, this was incorrectly formulated and will be revised (see comments to the second Reviewer for more on this point).

This implies that the computational efficiency is the advantage over PDD models, whereas it is the improved physical basis that is most helpful.

Response: This is true. The goal with REMBO was to improve the physical basis over PDD models and to improve computational efficiency compared to more advanced approaches (RCMs, GCMs). This will be better formulated in the revised manuscript.

This is indeed an arguable premise, that ice discharge into the ocean can be accurately modelled without fast flow (resolution of ice flux in channelized fjords; ice-ocean interactions). For instance, the recent interannual variability in the fraction of GIS mass loss due to ice discharge can't be captured without these fast-flow and ice-ocean processes. I suspect this is a less rigorous constraint than ice sheet extent. Still important to look at, but I wonder how much weight this should be given.

That said, the way you incorporate this: based on the amount of modelled melt/runoff, is more robust I think.

Response: Please see the response above.

Rather, this indicates paleo-elevation (i.e. paleo-altimetry, associated with air pressure).

Also, I don't know whether 400 m is conservative wrt the total gas content data - I had the impression that the Eem elevation change at Summit could have exceeded this, e.g. up to 600 m, but best to speak with e.g. Raynaud or Bender or Cuffey on this.

Response: We agree that the gas content is related to elevation. The sentence will be reworded to leave out "conservative". Because there is high uncertainty in these values, we cannot say that 400m is conservative. To address this question more thoroughly, a new figure will be added that shows how the allowed Eemian contribution to sea level rise changes with different values of this constraint. For a realistic range between 300m and 600m, the maximum contribution to sea level rise changes by approximately 0.25m in either direction.

indeed should cite e.g. Fahnestock et al (2001) here, as evidence of likely regional anomalies or hot spots, at least in N Greenland.

Fahnestock, M., W. Abdalati, et al. (2001). High geothermal heat row, basal melt, and the origin of rapid ice how in central Greenland. Science 294(5550): 2338-2342

Response: Thank you for the reference, we will add the citation.

By meshing with the PDD coefficient, does this not assume (like PDD models) that all of the melt comes from T, and not from S? That is, once you have a more physical model here, it should partition melt between the different terms in (2), so the fraction of melt associated with T (PDD) should be less than in PDD models.

I guess that this effect is probably calibrated out with the negative value of c, such that this works fine.

Response: With the PDD model, there are two PDD coefficients (degree-day factors): one for snow and one for ice. The difference between these two factors implicitly accounts for the difference in corresponding albedos and, therefore, for the effect of the insolation. In our approach, the effects of albedo and insolation are accounted for explicitly. The fact that the value λ in Eq. (2) is the same as the standard degree factor for snow in PDD model is coincidental, but the fact that ITM is less sensitive (on average) than PDD to temperature changes alone is not. However, since both models are tuned to the present day climate, they both simulate rather similar melt rates. They do have, however, different sensitivities to climate change, as was discussed by Robinson et al. (2010).

This is an excellent test to add to the sensitivity studies. I wonder, however, whether they should go further, up to a warming of as much as 4-5 degC, since the CAPE reconstructions suggest warming of up to 5C was possible in this region? While that may or may not have been realized or sustained, the tests performed here do not bracket the full range of possibility.

Response: We agree with this comment and have added additional simulations to account for higher warming (see also initial comments). The new simulations provide interesting results, confirming that better constraints on the temperature anomaly during the Eemian would greatly help to constrain model parameters and the GIS contribution to Eemian sea level rise. A new figure will also be added showing the temporal evolution of sea level and GRIP temperature during this period. For higher temperature anomalies, only low sensitivity models are accepted, resulting in an earlier peak in GIS mass loss. This also indicates that if improved estimates of sea level peak timing can be produced, this would also help to constrain the GIS during this period.

This seems excessive and reaffirms the idea that present-day extent might offer a helpful constraint.

I understand the concerns in the paragraph below, that ice extent may not be a good constraint because the model lacks a good treatment of fast flow and calving, so mass losses through these processes are under-represented, so the modern modelled ice extent is too high, and should be unless melting is tuned to be artificially high. But it is really hard to isolate how this affects ice extent vs. mass loss via calving. Both are compromised from the lack of model skill at ice-marginal and ice-ocean processes.

Some areas that experience heavy melting in the model presented here would likely lose their mass first through ocean melting and calving, in reality, if these processes were better modelled, so this means the constraint that is applied, on surface melt totals, is not totally robust either. If overestimated due to the reason above, it would leave 'room' for higher melt rates in other parts of the ice sheet, e.g. in some of the terrestrially-terminating regions of Figure 4 where too much ice is predicted.

Response: Please see the initial main comments. Because we are confident that we have improved the representation of surface mass balance compared to simpler PDD approaches (based on the tuning of the model), we must conclude that a large part of the positive bias in ice volume and extent results from a lack of representation in the ice sheet model. This is not to say that perhaps some climatic changes during the Holocene (and/or Eemian) also occurred that are not accounted for here. But without additional information, we cannot apply anything different than anomalies to the present-day fields. The partition criterion applied here allows us to constrain the sensitivity of the mass balance model without applying additional tuning of the climate. This is important for studies of the stability of the ice sheet.

It is worth emphasizing that this is not a 'regional sea-level climate signal' - rather, an amplified version of this including the change in elevation and albedo when the ice sheet thins or disappears from the landscape.

Response: We will clarify this in the revised manuscript.

Response to individual reviewer comments: Reviewer 2

p. 1554, line 5: mention that summer temperatures ‘at the margin’ are what is required.

Response: We agree, and it will be corrected.

p. 1556/1557: it is mentioned that SICOPOLIS v. 2.9 includes ‘a physically-based treatment of the temperate layer at the base of the ice sheets via explicit calculation of the water content of the temperate basal ice’. Is this at all relevant for Greenland, is such a layer actually occurring?

Response: Such a layer does occur but we didn’t test how important is this feature of

SICOPOLIS for our results. This additional comment about the ice sheet model will be removed from the manuscript.

p. 1557, line 27: REMBO does not assume changes in relative humidity at the Greenland borders. What is the implication of that assumption for precipitation changes over Greenland during the last glacial cycle and during the Eemian? That is an important issue because the central ice thickness responds quite strongly to accumulation changes in addition to the changes in extent driven by marginal melting. Fig. 1 should include a curve on the precipitation evolution (e.g. as a ratio wrt present) as those can also be compared to constraints from ice cores.

Response: An assumption of constant relative humidity can affect the results, but its impact should be rather small compared to the impact of temperature variations (~20°C between glacial and interglacial states). For example, in CLIMBER-2 simulations, variations in the relative humidity in the grid cells corresponding to Greenland are less than 5% over whole glacial cycles. And particularly, the difference in relative humidity between Holocene and Eemian conditions is negligible. We will add, as suggested, the accumulation curve to the Fig. 1. It shows that the agreement with empirical estimates is rather good. For example, at the LGM, accumulation at Summit decreases by a factor of four.

p. 1558: first paragraph. More details are required on how the ice sheet extent during glacial times depends on the sea-level forcing. How far out on the continental shelf can the ice sheet expand?

Response: Since SICOPOLIS does not include the treatment of shelf ice, the ice extent was limited to the area above the current (time-dependent) sea level. This will be added to the manuscript.

p. 1559: it is puzzling why the first constraint on mass balance partition is diagnosed for a fixed topography. Ice-sheet modellers are well aware that a modelled topography differs from an observed topography, especially concerning steepness of the margin and consequently the ratio of ablation area to accumulation area. Since a fixed topography is used to constrain behaviour obtained afterwards for a modelled topography an important systematic bias is introduced. A set of mass balance parameters that gives a reasonable partition for a fixed topography of the Greenland ice sheet will not do so for a model run, and this bears directly on the amount of ablation during the Eemian period. This problem should be carefully and convincingly addressed (see also further).

Response: We prefer to use the observed (fixed) GIS topography to constrain the mass balance model parameters, in order to eliminate other biases related to the ice sheet dynamics, uncertainties in the historic forcing, etc. However, it is important that the mass balance partition (and the absolute values of its individual components) is rather similar for both the simulated and observed GIS (which is the case for our simulations). If it were not the case, then the Reviewer's concern would be fully justified. We will discuss this issue in the revised manuscript.

p. 1559, line 16: is it really 'precipitation' that is meant here, or do the authors in fact mean 'accumulation'?

Response: Precipitation is meant here, but at Summit where no melt occurs, these two fields are very similar (in our model, they are identical, because we have no blowing snow, etc.).

p. 1559, line 18: what is the difference between 'calving' and 'ice discharged into the ocean'?

Response: We mean these terms synonymously. We do not represent calving explicitly, so we will change the manuscript to clarify this and only include the latter term.

p. 1559, section 3.2: the authors ought to provide stronger arguments why they consider the present-day modelled absolute elevation to be a useful constraint for the Eemian? The present-day elevation has virtually no memory of the Eemian. Can we really constrain the Eemian climate well enough to use a set of parameters constrained for the present day as a good model validation for the Eemian?

Response: We do not use the present-day absolute elevation to constrain the Eemian. We instead use the present-day GRIP elevation to constrain model parameters. In our case, the present-day GRIP elevation is most strongly affected by the geothermal heat flux, so it mainly helps to constrain this value.

p. 1560: total gas content has been contested as a good proxy for elevation changes. Referring to Raynaud et al. (1997) it is written that the gas content of the GRIP ice core indicates 'isotopically' warmer conditions. How is gas content related to isotopic composition? On what grounds do the authors interpret this as a surface lowering of maximally 400 m as the oxygen isotope record has equally recorded climate change? Assuming a large Eemian warming the central ice sheet could in fact also have been thicker. This argument needs sharpening.

Response: This point was poorly worded in the manuscript and it will be revised considerably. We consider that $\delta^{18}O$ measurements can provide information about temperatures. These data cannot say whether the temperature increase stems from general atmospheric warming or a decrease in elevation. Meanwhile, the total gas content is not a measure of temperature, but indicates changes in elevation, because the density of air is higher at lower elevations. Raynaud et al. (1997) found that although the timing of the gas samples was disturbed, the measured values for the Eemian were of the same range as present-day values, with the maximum values only slightly higher. This would indicate an Eemian GRIP elevation similar or lower than today. Cuffey and Marshall (2000) estimated a maximum lowering of approximately 350m, based on the variability of the measurements (that 0.01 cm^3g^{-1} variability corresponds to ~350m elevation uncertainty). Meanwhile, Otto-Bliesner et al. (2006) use the same data to say a maximum lowering of 500m is possible.

This, of course, means that it is only a rough constraint, so in the discussion paper, we chose to allow the elevation to decrease by 400m and we performed a sensitivity analysis by applying a constraint of 300m and 500m as well. In the revised manuscript, we will show a sensitivity analysis ranging from 0-1500m allowed elevation reduction. For a realistic range of 300-600m, only 0.25m of additional uncertainty in the level sea level rise is found. No simulation showed an increase in elevation at GRIP during this period.

Furthermore, we are not aware of publications contesting the use of total gas content as a measure of elevation changes. To the contrary, the results of Raynaud et al. (1997) have been cited many times thereafter, both by modelers and data experts (e.g., Cuffey and Marshall, 2000; Tarasov and Peltier, 2003; Otto-Bliesner et al., 2006; Landais et al., 2003).

Therefore, based on the available data, it seems unlikely that the Eemian GRIP elevation would have been much lower than the 400 m constraint chosen here, if at all, and for the estimated range of sea level contribution to be much different, a much more significant drop in elevation would be necessary.

p. 1562: why is so much importance attached to the geothermal heat flux as a perturbed model parameter? The geothermal heat flux controls the area of basal sliding and the temperature in the basal deformational layers, but many different values of the geothermal heat flux can give similar ice thicknesses with another choice of sliding coefficient and ice hardness (enhancement factor in the flow law). Besides, the geothermal heat flux has a high spatial variability and using a constant value may just be too simple to use this parameter as an influential parameter. This point needs more discussion.

Response: We agree that the actual *geothermal heat flux* field likely contains high spatial variability, but the existing data are insufficient to reconstruct the spatial pattern. Therefore, we prefer to use a constant value to reduce the number degrees of freedom in the model. We also agree that the geothermal flux affects the elevation in combination with (highly uncertain) sliding parameters, which we also perturb. Nonetheless, we do not consider our work as an attempt to find the right geothermal heat flux, but rather to have a realistic shape of modern GIS. This will be clarified better in the revised manuscript.

p. 1564-1565, section 4.5: apparently, a spatially uniform temperature anomaly is applied all over the model grid, identical to what was done in most previous studies. That is a major simplification that should be discussed more fully. For instance, GCMs usually indicate that temperature anomalies over the central ice sheet are larger than those at the margin. Secondly, changes in precipitation follow from a constant relative humidity. More information (a graph and/or discussion) should be provided on what this implies for precipitation changes. For instance, what is the precipitation change for a 20_C cooling over central Greenland, and what is it for a 5_ warming? Is it much different from the usual treatment based on the Clausius-Clapeyron relation?

Response: Indeed, a spatially uniform temperature anomaly around Greenland has been applied here, similar to many previous studies. This does not contradict the Reviewer's observation that *GCMs usually indicate that temperature anomalies over the central ice sheet are larger than those at the margin.* The temperature over the central part of GIS is computed by REMBO and can be significantly different from the prescribed temperature anomaly around the Greenland coast. Because REMBO computes the temperature daily, accounting for changes in elevation and albedo, the temperature changes over the ice sheet are more likely to be realistic compared to simpler approaches.

As far as precipitation changes are concerned, for a GIS geometry similar to the present one, they do closely follows Clausius-Clapeyron relation (also see the response to the comment about page 1557). However, when the GIS elevation/extent is considerably affected (as during Eemian), orographic effects also become very important and are accounted for by REMBO. Our approach cannot account for large-scale changes in atmospheric circulation (e.g., storm tracks) outside of Greenland, but this is not different from other approaches. Overall, changes in the surface mass balance of GIS during the Eemian are primarily controlled by temperature and insolation changes. A panel showing the temporal evolution of snowfall at GRIP will be added to Fig. 3.

p. 1566-1567: Figure 5: The colour scale does not allow to distinguish many details apart from the fact that elevation change is (not surprisingly) the main contributor to surface *temperature change. This should be revised.*

Response: We agree, and this Figure will be revised.

p. 1567, section 5. A figure comparable to Fig. 5 should be added for precipitation (or accumulation) anomalies/ ratios. Arguably, accumulation changes may be at least as important for ice thickness of the central dome than marginal melting during the Eemian.

Response: This is a good idea, and a second set of panels will be added to Fig. 5 to show the difference in precipitation from present day.

p. 1567, lines 16-19: I disagree on the role of fast processes and/or model resolution to produce a steeper modelled than observed ice margin (and therefore requiring higher surface melt for the same extent). Papers by Saito et al. (2007, AnnGlac 42) and Van den Berg et al. (2006, JGlac.) have clearly demonstrated that this is a numerical artefact in ice-sheet models due to the flux calculation at the margin. This is an important issue the authors need to clarify in terms of using a fixed topography to constrain mass balance model parameters.

Response: We are familiar with these papers, but we believe the problem here (producing a GIS that is too large) is additional to the issues outlined in the mentioned papers. More than half of the accumulation over the GIS is discharged into the ocean, primarily via ice streams and outlet glaciers, rather than by melting. Whatever problems exist with the calculations of marginal points in existing ice sheet models, if more of these marginal points are not in contact with the ocean (as is shown in Fig. R1 above), there is no way to obtain the right amount of ice discharge in the ocean.

References: the first paper on modeling the behaviour of the Greenland ice sheet during the Eemian with essentially similar methods was published by Letreguilly A., N. Reeh and P. Huybrechts (1991) in Global and Planetary Change (The Greenland ice sheet through the last glacial-interglacial cycle), and deserves to be referenced and discussed.

Response: We apologize for not citing this important work, of which we are, of course, aware. The paper will be cited in the revised manuscript.

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