We thank the reviewers for their comments. In general, we have added more discussion on the implications of our findings, by addressing in more detail how our results differ from earlier ice sheet modeling studies, and by extending the section where we compare our results with marine proxies and other Eemian climate reconstructions or models. With this, we think that the manuscript has been improved considerably, for which we thank the reviewers.
Below, we answer to each of the comments. We printed our answers in blue, to distinguish them from the reviewer's comments in black. Use of italics refers to quoted text from our revised manuscript.

Referee #1: Anders Carlson

I think this is a great paper that should be published, but it can also be improved by the authors. Most notable is the absence of a discussion of the implications of their study. Since this is the most "thorough" attempt at modeling the LIG GIS, I think including such implications would greatly improve the paper beyond what is at present a thorough modeling paper. I outline these below.

The authors should add a section on why they get different results than previous modeling experiments. They mention that previous attempts used a single forcing or PDD methods downscaled from a GCM, but I think further discussion is needed if the reader is to believe their approach is superior or "more right". One could argue that layering models on top of models propagates uncertainty in each model and so such a discussion would improve the impact of the papers results.

We have included a separate section in the discussion that addresses the differences with previous modeling experiments, and some underlying causes:

5.1 Comparison to other ice sheet simulations

In comparison with previous simulations of Eemian GrIS retreat, we have employed a different approach to force our ice sheet model. As the SMB forcing strongly influences GrIS response, it is not surprising that our results differ from those earlier studies. Here we attempt to identify some key characteristics of our Eemian ice sheet – climate simulation, to clarify these differences.
As shown above, high accumulation along the southeastern margin prevented disintegration of the southern dome, notwithstanding substantial retreat of the southwestern ice margin. This contrasts with earlier work that resulted in higher (2.7 – 5.5 m) GrIS Eemian SLR estimates (Cuffey and Marshall, 2000; Huybrechts, 2002; Tarasov and Peltier, 2003; Lhomme et al., 2005). The (near-)absence of ice in southern Greenland in these previous studies is likely caused by underestimated accumulation in the southeast, as these studies used the accumulation map from Ohmura and Reeh (1991), which is drier in this region. More recent studies, all using different GCM products as forcing fields, resulting in more ice cover in southern Greenland, and different magnitudes of ice sheet retreat in the north (Robinson et al., 2011; Fyke et al., 2011; Born and Nisancioglu, 2012; Quiquet et al., 2013; Stone et al., 2013). The northern region is vulnerable because of the low accumulation. Small changes in SMB result in large differences of ice sheet volume

response. One of the major issues concerns the calculation of the ablation, which is often based on a PDD model approach. Van de Berg et al. (2011) showed that it is problematic to correctly reproduce north-south gradients of ablation in different climate states. Therefore, Stone et al. (2013) suggested that a more physically-based representation of insolation-driven ablation in the Eemian should result in a stronger northern ice sheet retreat than their own results. However, with a regional climate model as forcing we find the opposite, i.e. less northern retreat. This discrepancy can probably be attributed to cooler conditions over northern Greenland in our regional climate model, compared to their set of GCM simulations. The latter might be due to a-priori assumptions of their Eemian ice sheet topography, lacking ice cover in northern Greenland (Stone et al., 2010; 2013).
In general, our coupled regional climate – ice sheet modelling approach offers important advantages over previous simulations. Firstly, the high-resolution (18 km) of the regional climate model results in an explicit, physical and high-resolution representation of the precipitation pattern. Moreover, the resolution is very similar to the resolution of the ice sheet model, which circumvents the need of downscaling. Next to that, the ablation is calculated using a physically-based multi-layer snow model and energy balance calculations, with explicit albedo-melt feedback, circumventing the use of temperature – melt relationships such as a PDD model. Lastly, the 2-way coupling strategy allows feedbacks between topography and atmosphere. Ideally, the coupling interval would be smaller than 1.5 ky, but this was not possible due to computational costs.

The authors do not discuss the impact that GIS runoff would have on ocean circulation. What is the Sv discharge? What is the total runoff (i.e., not sea-level rise component but the total freshwater flux to the ocean, which would be larger)?

The total freshwater flux (FWF) is larger than the sea-level rise contribution, since FWF is the sum of the meltwater run-off (R) both from ice sheet and tundra (seasonal snow) and the calving flux, whereas the contribution to sea level depends on the total mass balance (MB):
FWF = D + R
MB = SMB - D
We have computed FWF (tundra excluded) and show it in a revised version of figure 5b:

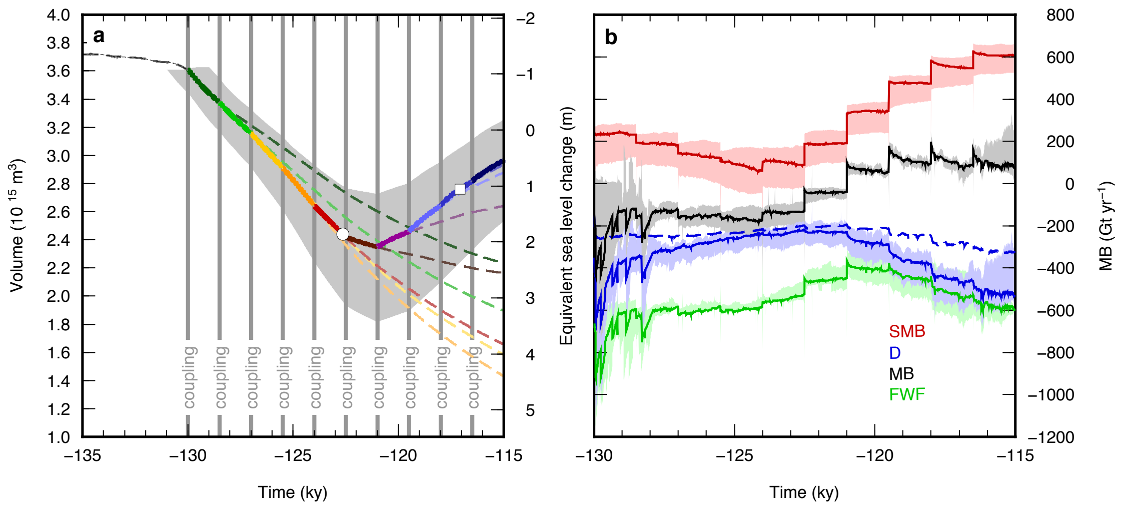

Fig. 5. Eemian ice volume changes and mass balance components. (a) Simulated GrIS volume change and equivalent sea level contribution. The colours indicate different climate forcings. Dashed lines show ice volume evolution without additional coupling. Circle (square) indicates the timing of ice margin retreat (readvance) at Dye-3. (b) Time series of SMB (red), D (ice discharge, blue), MB (black) and FW flux (green). D and FWF are shown as negative numbers to express mass loss. The blue dashed line represents the calving flux along the southeastern margin, south of 68°N (see text). Background shading represents the uncertainty estimate.

We added a paragraph in section 4.2 about the freshwater flux:

Shown in green in Figure 5b is the freshwater flux (FWF), which is the sum of the ice discharge and the ice sheet meltwater run-off (FWF = D + R), and therefore larger in magnitude than MB. A maximum FWF of ~800 Gt y^{-1} (0.028 Sv) occurs at the start of the coupled simulation, but FWF decreases due to the reducing influence of D, as the ice sheet margin retreats increasingly on land. A relatively stable FWF of ~600 Gt y^{-1} (0.021 Sv) prevails between 128 and 124 ky BP, but note that the FWF from the tundra is not included here, which likely is an increasing number due to the expanding tundra area. This simulated Eemian value is lower than the present-day FWF from Greenland (Bamber et al., 2012), which can be explained by the much smaller role of D in our simulation. The maximum Eemian FWF contribution from ice sheet run-off (350 ±76 Gt y^{-1}) is of the same order of magnitude as the modern-day value.

How does this compare with climate simulations that suggest small amounts of LIG discharge from the GIS or the Arctic could cause local ocean cooling near the GIS (Cottet-Puinel et al., 2004; Born et al., 2010; Sanchez-Goni et al., 2012)? Indeed, a paper the authors should look at suggested cooling of the Labrador Sea through much of the LIG from GIS melt (and increased Arctic freshwater export) that also may have reduced deep overturning: Winsor et al. (2012, G3). How would their results be impacted by such a local feedback? Would this then make their retreat results a maximum estimate if this feedback was included? There is a hint of this with their varying the melt rate of marine margins, but this could be further developed in light of these studies.

First, we thank the reviewer for his suggestions of this relevant literature. With regard to the impact of GrIS melt on the Atlantic meridional overturning circulation (AMOC), it is important to stress that our modeling approach only covers feedbacks between ice and atmosphere, no oceanographic feedbacks are included. This prohibits a detailed analysis of ice-ocean interaction. Nonetheless, we added the following paragraph in section 5.2:

The same Eirik Drift sediments also point to reduced Labrador Sea subsurface temperatures and reduced salinity between 130 and 122 ky BP (Winsor et al., 2012). They attribute this to an increased FWF from Greenland, and associated reduced Labrador Sea convection, although the influence of sea ice anomalies are not ruled out. These findings are supported by climate models of intermediate complexity, pointing to a locally weakened deep convection in the Irminger and Labrador Seas, as a result of surface water freshening during the Eemian (Cottet-Puinel et al., 2004; Bakker et al., 2012; Sánchez Goñi et al., 2012). Without additional FWF forcing, this feature is also present in a combined ECHO-G – RACMO2 Eemian climatology with the present-day ice topography: cooling over the Labrador Sea occurs, due to a local sea ice expansion and less entrainment of warm Atlantic water (Van de Berg et al., 2013).
Our simulated FWF (Figure 5b) is probably not large enough to significantly impact overturning circulation, although Bakker et al. (2012) suggest that a FWF of 0.039 Sv can locally weaken deep convection in the Irminger and Labrador Seas. Although our annual mean FWF values are lower, summer peak values are much larger. However, our modelling approach does not include oceanographic feedbacks, which precludes a robust analysis of the impact of ice loss on ocean circulation, and vice versa.

The question on how such local feedback would have influenced our results is not trivial. It is too simple to argue that including an ocean circulation feedback would result in less ice sheet retreat. The direct effect of ice-ocean interaction is small in our experiment, due to the quick retreat of the ice sheet to land. Changes in ocean circulation imply a redistribution of heat, but can also be associated with changes in precipitation patterns, with obvious consequences for the SMB forcing. Our simulation is forced by ECHO-G, which does show lower Eemian near-surface temperature over the Labrador Sea, when compared to the preindustrial run (as written above), so this feature seems quite well represented in our climatology.

Similarly, I think it would be great if the authors would include a figure or two of the regional climate model anomalies. Such high-resolution simulations for an important region during the LIG would be a great addition to the paper. I note this because I think the authors could better discuss some of the complexities of LIG climate in this region. First, the CAPE project glossed over much of the LIG nuance and showed negative anomalies (e.g., work by Bauch and others) as no change. Similarly, they used many records that do not have chronology beyond being assumed to be from the LIG. Where they fell in terms of timing in the LIG was not considered. Records that had a chronology were also still lumped together into one time period. There are many regions of the North Atlantic that were no warmer than present/the Holocene during the LIG, including regions close to Greenland (Bauch et al., 1999; 2012; Van Nieuwenhove et al., 2011; Winsor et al., 2012). Likewise, the Baffin Island chironomid record the authors reference has been revised and shows temps during the LIG that were no warmer than peak Holocene temps (Axford et al., 2012). Such nuance beyond the CAPE reconstruction would benefit from the publishing of the regional climate model results.

We thank the reviewer for these valuable remarks about LIG climate reconstructions. We now refer to several of these suggested studies at different places in the new manuscript, to give a better picture of LIG climate in this region:

Introduction:
Regionally, Eemian temperature anomalies were lower or even negative, in particular sea surface temperature (SST) over the Labrador Sea and Nordic Seas (Bauch et al., 1999, 2012; Winsor et al., 2012).

Section 4.3.4:
We can further compare simulated July temperatures in Baffin Island with proxy records, as this is an area within the RACMO2 domain. We find 3 to 4 K higher July temperatures here, which is close to, but slightly lower than, the 4 to 5 K higher summertime temperatures as reconstructed from a compilation of paleoclimatic evidence (CAPE-Last Interglacial Project Members, 2006), and also in agreement with proxies for lake water temperatures (Axford et al., 2011). The Eemian optimum climate from RACMO2 does not show uniform warming over the entire domain, local cooling also occurs. For example, near-surface temperatures over the Labrador Sea show a distinct negative anomaly (down to 4 K) (Van de Berg et al., 2013). Evidence for local oceanic cooling in this region is also found in Eirik Drift sediments and by paleoclimate modelling (Winsor et al., 2012; Sánchez Goñi et al., 2012). Overall, the comparison with the available paleoclimatic evidence yields confidence in the quality of the Eemian climate from ECHO-G.

Section 5.2:
Marine proxies from the same Eirik Drift site point to reduced Labrador Sea subsurface temperatures and reduced salinity between 130 and 122 ky BP (Winsor et al., 2012). They attribute this to reduced Labrador Sea convection, due to increased FWF from Greenland, although the influence of sea ice anomalies are not ruled out. These findings are supported by climate models of intermediate complexity, pointing to a locally weakened deep convection in the Irminger and Labrador Seas, as a result of surface water freshening during the Eemian (Cottet-Puinel et al., 2004; Bakker et al., 2012; Sánchez Goñi et al., 2012). Without additional FWF forcing, this temperature anomaly is also present in the combined ECHO-G – RACMO2 Eemian climatology with the present-day ice topography: cooling over the Labrador Sea occurs, due to a local sea ice expansion and less entrainment of warm Atlantic water (Van de Berg et al., 2013).
Our simulated FWF (Figure 5b) is probably not large enough to significantly impact overturning circulation, although Bakker et al. (2012) suggest that a FWF of 0.039 Sv can locally weaken deep convection in the Irminger and Labrador Seas. Albeit simulated annual mean FWF is lower than this value, summer peak values

exceed this threshold easily. However, our modelling approach does not include oceanographic feedbacks, which precludes a robust analysis of the impact of ice loss on ocean circulation, and vice versa.

Next to this, we would like to stress that the focus of this manuscript is on ice volume changes in the Eemian, whereas the Eemian climatology is discussed in separate papers (e.g. Van de Berg et al. 2011, 2013). The most relevant (anomaly) field for ice sheet changes is the SMB. Van de Berg et al. (2011) presented this, and also showed differences in atmospheric summer temperatures at 500 hPa. In our opinion, an anomaly plot of near-surface temperature (T_{2m}) outside the area of the ice sheet is less relevant for our manuscript, as it does not directly influence ice sheet evolution. However, we agree with the reviewer that a high-resolution climate simulation is valuable, also for comparison with proxy data. Therefore, we would like to point out to Figure 4a in Van de Berg et al. (2013, Clim. Past Discussions), that shows T_{2m} anomalies with respect to the preindustrial climate. Most notable are the changes over the ocean, where T_{2m} is determined by changes in water temperature. Regional cooling (down to -4 K) over the Labrador Sea is simulated, due to increased sea ice extent and less entrainment of warm Atlantic water. We added a remark about this pattern in the manuscript, as quoted above.

The timing of their minimum GIS extent/maximum sea-level contribution could also be further developed. Although not well dated, the ~121 ka minimum agrees with the inferences of the minimum GIS extent from Eirik Drift sediment records (Carlson et al., 2008; Colville et al., 2011). How does this timing compare with estimates of the timing of peak LIG sea level (e.g., Dutton & Lambeck, 2012; Kopp et al., 2009)? Are there any variations in retreat that could explain sea-level volatility suggested by some records (e.g., Rohling et al., 2008; Thompson et al., 2011)?

We extended the discussion on the contribution to the sea level highstand, section 5.4, first paragraph:
Our sensitivity experiments combined with the available geological evidence from ice (NGRIP members, 2004; Willerslev et al., 2007; NEEM community members, 2013) and marine cores (Carlson et al., 2008; De Vernal and Hillaire-Marcel, 2008; Colville et al., 2011) strongly suggest that the GrIS contribution to the Eemian sea level highstand was between 1.2 and 3.5 m, with a best estimate of 2.1 m (Figure 5). The timing of the minimum ice volume at 121 ky BP also compares well with marine proxies, that suggest reduced input of meltwater from Greenland after ~120 ky BP (Carlson et al., 2008; Colville et al., 2011). Kopp et al. (2009) show an earlier eustatic sea level maximum, at 124 ky BP, followed by a secondary peak at 118 ky BP. This contrasts to Dutton and Lambeck (2012), who infer a later eustatic sea level maximum, but mainly stress that the current limited availability of far-field data and dating uncertainties make the reconstruction of a eustatic Eemian sea level curve uncertain. We find a single minimum in GrIS volume, which is an obvious result from the gradual transition between Eemian Optimum and Glacial Inception in our forcing fields, and the lack of possible oceanic feedbacks. Our results therefore also lack any indication for millennial-scale sea level oscillations, as found in several coral stratigraphies (Rohling et al., 2008; Thompson et al.,

2011). These may also register variations in Antarctic ice volume, which is not considered here.

Minor comments by page/line

1736/21: This 4-5 K warmer is highly selective, I would note the nuance discussed above. The CAPE project also didn't consider transient climate evolution so a 4 K peak at 128 ka was grouped with another 4 K peak at 118 ka, which is clearly not right.

First sentences of introduction are now as follows:

Eemian summer temperatures over most Arctic land reached peak values of 4–5 K higher than during the preindustrial (CAPE-Last Interglacial Project Members, 2006; Overpeck et al., 2006). Regionally, Eemian temperature anomalies were lower or even negative, in particular sea surface temperature (SST) over the Labrador Sea and Nordic Seas (Bauch et al., 1999, 2012; Winsor et al., 2012).

1737/29: I wouldn't say "very well" as this is not quantifiable

Here we disagree: the performance of RACMO on reproducing SMB is quite well-quantified with observations, as can be seen in Ettema et al. (2009): 265 SMB observations were compared with modelled values, with a correlation coefficient of 0.95.

1738/7: Change to "Eemian interglacial period". Interglacial is an adjective, not a noun.

OK

1738/8-12: This is an odd sentence. I would reword: "With this approach we not only take advantage of the improved...., but also the two-way coupling, which ensures a ...... and the climate forcing, yielding more confidence in the SMB reconstruction".

We rephrased this sentence:

This approach offers two mayor improvements with respect to earlier Eemian GrIS simulations: Firstly, we take advantage of the improved performance of regional climate models over GCMs, and secondly the two-way coupling ensures a physically realistic representation of the feedbacks between the continuously changing ice sheet topography and the climate forcing, yielding more confidence in the SMB reconstruction.

1739/1: I would use a ";" not a colon, this could be changed in a lot of places in the text.

Thanks, we replaced colons through the text where appropriate.

1740: Somewhere such non-linear feedbacks of the GIS on ocean circulation/temperature needs to be mentioned, which aren't included in this simulation because the GCM is not coupled. For instance, even if an ice margin is land terminating, what effect does a cold surface ocean have on SAT?

Indeed, this should have been mentioned more clearly. We have included this in the discussion, section 5.2 (see above).

1741/19-21: I would say "on land during our Eemian simulations", since this is model result; actual data on whether ice retreated from the sw coast is lacking at present.
OK

1743/11-14: Okay, so even starting at 129 ka, one still must include the Laurentide, which didn't fully disappear until 128-126 ka (Carlson, 2008; Carlson & Winsor, 2012). This ice sheet would definitely impact the Lab Sea (Carlson et al., 2008) and could influence Greenland climate (LeGrande & Schmidt, 2009). I think the authors need to further discuss this influence that they have not included in their simulations. What happens if one starts the LIG simulation at 126 ka, when the Laurentide is fully gone?
Indeed, (remnants of) the Laurentide ice sheet had an influence on early LIG climate. This is not captured in our results, as the Laurentide ice sheet was not present in the ECHO-G simulations. We discuss this with an addition to section 2.2:
Note that no other than modern ice cover is present outside Greenland in our climate model simulations. In particular (remnants of) the Laurentide ice sheet have probably had influence on atmosphere and ocean circulation in the early Eemian, but this difficult to quantify.
The question about what happens / changes if one starts the simulation when the Laurentide is fully gone is hard to answer, because to perform such a simulation would require knowledge of the state of the GrIS at 126 ky, which obviously would be much smaller then the GrIS at 130 ky. In principle, this GrIS size is neither known at 130 or 126 ky, but we did our best to initialise our ice sheet model as well as possible, by schematically forcing the ice sheet model over the entire penultimate glacial cycle (section 3). Obviously, the influence of the Laurentide ice sheet was even larger during this initialisation period, though hard to explicitly take into account.

1744/16-18: Okay, so here would be an example of a place where the reader is left wondering why this simulation doesn't have more NE GIS retreat, which is found in other simulations. Can the authors discuss further?
We added a separate section (5.1) about this in the discussion (see above).

1745/3-8: So, what happens when a cold Lab Sea would be included in response to GIS retreat? Would this reduce SW GIS retreat? Keep a calving margin?
Hard to say. As stated above, the Labrador Sea already has a cold anomaly in our simulation, without any freshwater forcing perturbation. But possible/potential feedbacks associated with changes in ocean circulation are not included, and we prefer not to speculate about this.

1745/28: This timing could be further developed as suggested above.
OK, this is now included in Section 5.4.

Section 4.3.1: Okay, so here's where the effect of the Laurentide needs to be further considered. It was likely still quite large up until ~128 ka, when ice over Hudson Bay collapsed (Carlson, 2008), and disappeared shortly there after by

~126 ka. So this simulation sensitivity should be expanded to see what happens when the simulation is started at say 126 ka so as to have no Laurentide influence.
The experiment suggested by the reviewer would assume that there is still a full-grown Greenland ice sheet at 126 ky BP. Although we do not deny the Laurentide influence on early Eemian climate, it is not expected that the Greenland ice sheet remained unaffected by Eemian warming while the Laurentide ice sheet collapsed during those first couple of millennia, as explained above. Therefore, we did not perform such an experiment. Furthermore, it should be noted that at 130 ky BP, simulated ice volume is 24% larger than today, i.e. 3.6 10^{15} m^3.
Rather than the aspect of the start of the Eemian warming, this issue is more related to the accuracy of our climate forcing. Therefore, we added some discussion on this in section 4.3.4:
Next to that, the lack of (remnants of) the Laurentide ice sheet in ECHO-G may lead to an overestimation of early Eemian warming.

1748/15: I would say "indicating in these simulations that the Eemian..."
Changed into:
... indicating that the simulated Eemian minimum is mainly determined by the accuracy of the SMB calculations.

1748/25: Sanchez-Goni et al. (2012) also simulated cooling around Greenland from GIS meltwater runoff.
We added a remark on this:
Evidence for local oceanic cooling in this region is also found in Eirik Drift sediments and by paleoclimate modelling (Winsor et al., 2012; Sánchez Goñi et al., 2012).

1749/5: Okay, I wouldn't reference Francis et al. but rather their revised work in Axford et al. (2012), which shows <5 K of warming relative to the late Holocene over Baffin and Baffin LIG SAT that was no warmer than Holocene SAT.
OK

1751-1752: In general, I think the authors are making too big of a deal over the NEEM misfit. First, the model is only outside of the NEEM SAT uncertainty window at ~127 ka, when the NEEM record could have issues with the Laurentide fore bulge migrating through. Otherwise, it agrees. The authors should consider the uncertainty in these ice-core records. So this would remove the problem about underestimating the temperature change. The model gets it right for much of the LIG, assuming NEEM is "right".
We think this is an important topic, and so did other reviewers in the past. Note that the uncertainty in the ice-core record also allows a temperature deviation to the positive side (>+12 K), which would be in huge contrast with reconstructed elevation changes, as also reflected in our discussion.

1754/7: The authors should note that this inference from their model that Antarctica contributed a significant amount of LIG sea level rise is in agreement with the conclusions of Colville et al. (2011) based on those authors' observations from GIS discharged sediment records.

Yes, this is also implied by referring to Colville et al. (2011), a few lines above this statement.

1756/23: This publication name is incomplete. 1757/10: This publication has way more authors than listed.
Yes, we corrected this.

Referee #2: anonymous

1. Has it been shown that the asynchronous coupling between climate model and ice sheet model is satisfactory? A study by Calov et al. (2009) using an Earth System Model of Intermediate Complexity showed that an information exchange interval of 1000 years 9with the ice sheet model running continuously) leads to extreme reduction in simulated sea level drop. They argued that such an information exchange interval is too long for a realistic simulation of glacial inception. Obviously due to computational costs I realise that increasing the number of regional climate model simulations is not feasible but some discussion of the effects of the asynchronous coupling on the Eemian results would be useful, even if it is negligible.

   It is hard to prove that our coupling interval is sufficiently small, but by using the SMB gradients method in between model couplings, we in part account for a potentially too long coupling interval. Helsen et al (2012) showed that the SMB gradient method predicts SMB quite well after 1000 y.

2. A few more details on the experimental setup would be useful. For example, what bedrock topography is used in the ice sheet model? When ice is receding is it only replaced with tundra? What are the albedo values of this tundra? What are the orbital parameters used in the Eemian simulations? What greenhouse gas values do you use in your simulations?

We added this information:
(section 2.2) *Retreating ice on land is replaced by tundra, with an albedo of 0.18.*
(section 2.2) *Solar incoming radiation is adjusted for each Eemian time slice simulation to account for the influence of orbital forcing (Table 1, Figure 2, Berger and Loutre, 1991).*
An extra table provides details on the orbital and GHG forcing for each time slice:

Table 1. Climate model forcing parameters. Orbital parameters are calculated for the midpoint of each time slice (Berger and Loutre, 1991). Greenhouse gas concentrations between 130 and 125 ky BP follow the ECHO-G settings (Kaspar et al., 2007). The concentrations between 125 and 115 ky BP follow the observed concentrations in the Vostok ice core (Petit et al., 1999; Sowers, 2001).

time slice (ky BP)	eccentricity (-)	longitude of perihelion (°)	obliquity (°)	$[CO_2]$ (ppmv)	$[CH_4]$ (ppbv)	$[N_2O]$ (ppbv)
130.0 – 128.5	0.0385	60	24.2	270.00	630.0	260.0
128.5 – 127.0	0.0391	84	24.1	270.00	630.0	260.0
127.0 – 125.5	0.0396	107	24.0	270.00	630.0	260.0
125.5 – 124.0	0.0401	131	23.8	270.10	627.5	260.5
124.0 – 122.5	0.0405	155	23.5	270.70	612.5	263.5
122.5 – 121.0	0.0408	179	23.3	271.30	597.5	266.5
121.0 – 119.5	0.0410	204	23.1	271.90	582.5	269.5
119.5 – 118.0	0.0412	229	22.8	270.25	567.5	270.0
118.0 – 116.5	0.0414	253	22.6	268.15	552.5	270.0
116.5 – 115.0	0.0414	278	22.5	266.05	535.0	270.0

(section 2.3) *We use surface and bedrock topography from Bamber et al. (2003) as a starting point for our initialisation procedure (see section 3).*

3. Perhaps also including a plot of the spin-up of temperature from the regional climate model over Greenland either in the main manuscript or supplementary information would be informative to satisfy the reader that the regional climate model is sufficiently spun-up in 30 years of model time.

Spin-up time of atmospheric processes is in the order of days at most, so this is probably not an issue. The longest spin up time is associated with the subsurface temperature in the snow model. This can take a year or maybe two. Each simulation consists of 30 years, from which we only use that last 25 years. This assures minimal influence of spin-up problems.

4. Anomaly plots of temperature and precipitation patterns over Greenland for your control simulation at the time when the Greenland ice sheet shows minimal extent would be useful since it is these patterns which have been used to explain the Eemian northern retreat of the ice sheet in recent studies. How does yours compare? Furthermore, does the regional model for modern day tend to overestimate, underestimate precipitation in any specific region compared with observations? This may indicate where biases exist.

Probably what is meant are plots at the time of maximum retreat, not the time at minimum extent. This occurs at ~125 ky BP. We would like to stress that the focus of this manuscript is on ice volume changes in the Eemian, whereas the Eemian climatology is discussed in separate papers (Van de Berg et al. 2011, 2013). The most relevant (anomaly) field for ice sheet changes is the SMB. Van de Berg et al. (2011) presented this, and also showed differences in atmospheric summer temperatures at 500 hPa. In our opinion, an anomaly plot of near-surface temperature (T_{2m}) outside the area of the ice sheet is less relevant in our manuscript, as it does not directly influence ice sheet evolution. Moreover, changes in T_{2m} are dominated by topography changes that not only occur on the ice sheet, but also on the tundra due to glacial isostatic rebound. However, we agree with the reviewer that a high-resolution climate simulation is valuable, also for comparison with proxy data. Therefore, we would like to refer to Figure 4a in Van de Berg et al. (2013, Clim. Past Discussions), that shows T_{2m} anomalies with respect to the preindustrial climate. Most notable are the changes over the ocean, where T_{2m} is determined by changes in water temperature. Regional cooling (down to -4 K) over the Labrador Sea is simulated, due to increased sea ice extent and less entrainment of warm Atlantic water. We added a remark about this pattern in the manuscript, as quoted above.

5. The summary and conclusions of this manuscript are very brief. I think it would be very beneficial to include a comparison detailing the similarities and differences with other recent modelling studies (e.g the lack of northern retreat) and why your result might be more robust. The authors should also address the implications of their work and what the next future steps might be.

Yes, we agree and we included a comparison to other recent modeling studies, section 5.1. Moreover, we extended our discussion about the comparison to marine records, section 5.2, and the implications for sea level reconstructions, section 5.4. These new sections are all quoted above.

Minor/Technical corrections
P1737, line 15: Please also include the following reference: Stone, EJ, Lunt, DJ, An- nan, JD Hargreaves, JC 2013, Quantification of the Greenland ice sheet contribution to Last Interglacial sea level rise. Climate of the Past, 9, pp 621-639. Please also include in any other relevant sections in the manuscript.
Done

P1738, line 18: Please change a-synchronous to "asynchronous"
Done

P1742, line 21: Perhaps insert "interglacial" after "Holocene" for clarity
Done

P1743, line 4: What is the justification for the lapse rate value you chose? A different value of this parameter could alter the results.
We have chosen this value based on the lapse rate in RACMO2. This has now been made clear in the text:
Following Helsen et al. (2012), the temperature forcing as derived above is translated into a climatic elevation change ($\Delta H_{climate}$) using an atmospheric lapse rate γ_{atm} = -7.4 K km^{-1}. This value is obtained from a regression of elevation and RACMO2 surface temperature.

P1748, line 23 onwards: What simulation (i.e. which interval during the Eemian) do you use to compare with proxy data? A plot showing this or more details in the text would be useful as the CAPE data only represents the maximum Eemian warmth.
We used the Eemian climate reconstruction as described by Van de Berg et al. (2011; 2013), hence a solution that is calculated with the present-day topography of the Greenland ice sheet for maximum Eemian insolation, at 125 ky BP. ECHO-G boundary conditions are also calculated for this interval. We added some details on the near-surface temperature anomaly pattern in the text, reflecting that we find regional variability and even local cooling, rather than a

uniform Eemian warming.

P1753: line 24: An explanation of why 2.1 m is the best estimate should be included. It is not clear to me why this is the case.
We added: *... since this is the solution of the coupled model*

Table 2: Please give more detail in the caption of the sensitivity experiments. It may also be useful to include the actual numbers after the description e.g. sliding halved, late start etc.
We added the actual numbers and changed the caption into:
Summary of sensitivity experiments. Each experiment was carried out with one perturbed parameter. Resulting minimum ice volume and associated SLR are given.

Figure 4: The inset on Fig.4b is very difficult to see. I suggest making this substantially larger.

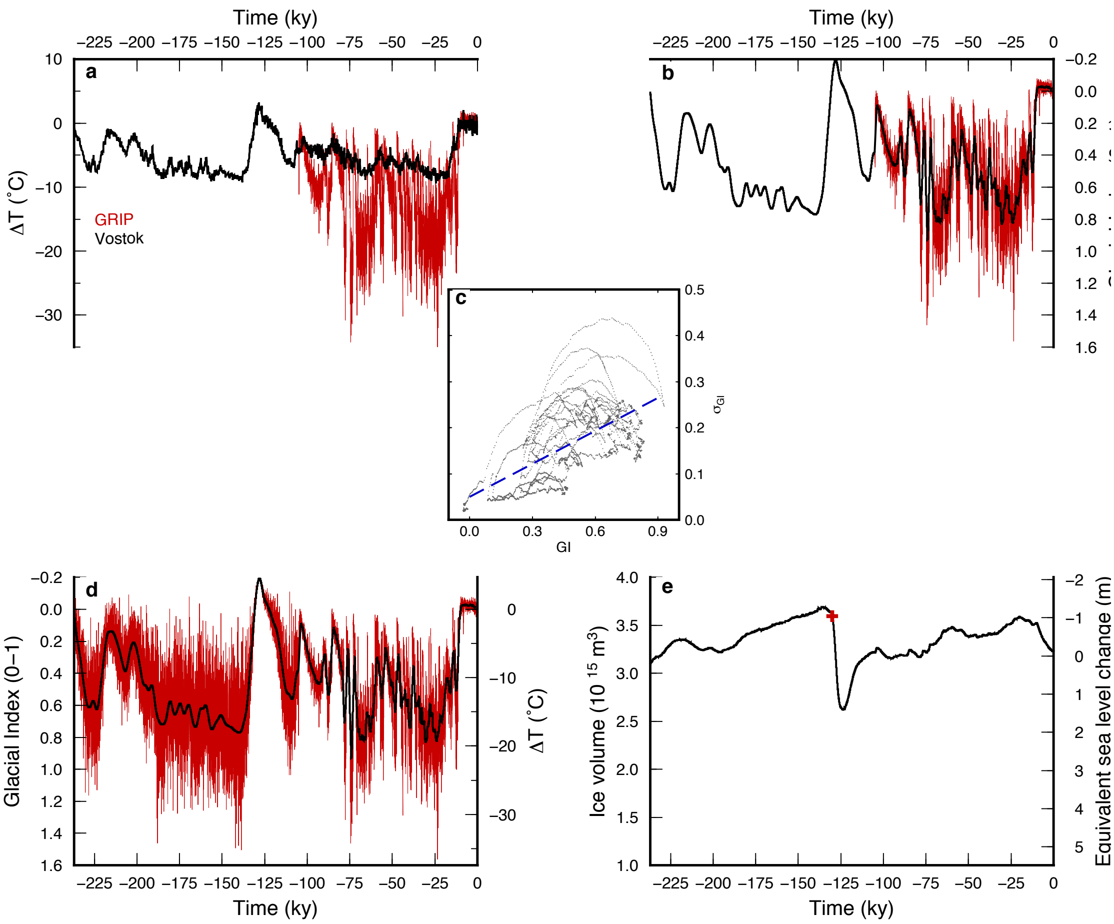

Fig. 4: Climate forcing used to initialise the ice sheet. (a) Near-surface air temperature proxies for the GRIP and Vostok ice cores. (b) Glacial indices derived from these ice core records, with 20 y resolution (red) and after applying a Gaussian filter (black); (c) Scatter plot of the standard deviation in the glacial index (GI) within a 3 ky bandwidth filter as a function of the mean GI. (d) Added high frequency variability of GI in the pre 105 ky BP period. Right axis shows corresponding climate record. (e) Resulting simulated ice volume using the climate forcing from (d).

Figure 6a: I suggest highlighting the feature at 72N, 50W with an open circle so that it is easily identified.
We think that a circle would mask some of the detailed information in the patterns, so we decided not to add the circle.

Figure 6b: As far as I can see there is no reference to this sub-figure in the text. Please include a reference to it.
Done

Figure 7: What does the grey shading represent? Is this the RMS between the sensitivity experiments and the control? Please clarify in the Figure caption.
Yes, this is indeed the RMS error. The figure caption is updated.

Figure 8: Please increase the size of the ice core labels. Although the shaded region is explained in the main text, also indicate in the Figure caption what the shaded regions represent.
The entire figure is enlarged to improve clarity, and the caption is extended.

References: All the references appear to have random (?) 4 digit numbers at the end of the citation in the reference list. I assume these should be removed.
These numbers are cross-references to the pages on which they appear. The journal added these.

References mentioned in review: Calov R, Ganopolski A, Kubatzki C, Clausen M (2009) Mechanisms and time scales of glacial inception simulated with an Earth system model of intermediate complexity. Clim. Past, 5, 245-258