Response to referree #1. Original comments are in italic, our response in bold dark grey.

Roche et al. present a model description of their initial inclusion of a dynamical ice sheet model into an EMIC (iLOVECLIM). Implementation of ice sheet models into EMICs is a worthwhile enterprise, given the ability of these models to simulate long time-periods. As such, I believe this work is well-motivated scientifically. However, I do believe very major revisions are required for this manuscript before it is released from the Discussion stage of GMD. Very broadly, these revisions focus on:
1. description and justification of coupling procedures
2. robustness of model evaluation
3. omissions of important diagnostics of model performance
4. unexplained inconsistencies in model results
5. overly confident assertions of model ability to perform science simulations
English grammar and conciseness also needs a lot of work, but I would like to see the more important concerns I have addressed before worrying about grammar.
*I have indicated pages and line numbers for particular points, and prefaced general points with 'General:' The comments are in order of appearance, and I have highlighted comments which I consider most important with a '***'*
*******************Comments*******************
P5216,L8: 'for the Northern Hemisphere ice sheet (Greenland' -> 'Greenland'
Corrected as per suggestion.
P5216,L13: 'reasonable' is a subjective word. I think the authors get a 'reasonable' comparison, only because the simulated GrIS is unable to expand any further, because of the surrounding ocean in the model.
The GRISLI ice-sheet model used in the manuscript allows the simulation of dynamical ice-shelves. Therefore, the ocean by itself does not prevent the simulated ice-sheet to grow further. In fact, when simulating a colder than present climate, there is further expansion of Greenland through 1) ice-shelves expansion 2) grounding of ice on the ocean floor 3) further thickening of the ice-sheet. The first area to respond as such is the northeast part of Greenland where the narrow Nares Strait becomes filled with ice. We are far from having such an extreme configuration in our present-day ice-sheet. Moreover, there are some small regions where Greenland is not glaciated in our simulations, as presented in the revised version of the manuscript (see new Figure 11, shown below). The word « reasonable » has been removed from the text that now reads : « We also find an ice-sheet areal extent that is overestimated with respect to the observed Greenland ice sheet. »

****P5216L17: first sentence of manuscript discusses glacial/interglacial cycles, but no glacial/interglacial simulations are carried out here, only constant-climate equilibrium simulations. Suggest rewording this first sentence, so reader is not left with impression that the following work involves transient simulations, or simulations of past climates.*
The introduction has been completely re-written and now relates our work not only in terms of Quaternary glacial cycles but also in the frame of present-day and future climate perspectives.
P5216L26: 'In this framework': not clear to reader what 'framework' this refers to
The text has been reworded and now reads « ***Within that wide possible spectrum of climate models, we aim ...*** **».**
P5217: 'direct evolution': 'direct descendant'
Corrected as per suggestion.

****General: Please include more references to earlier efforts to couple ice sheets to climate models (other than LoveClim), and explore how the author's approach differs from these approaches. A few examples: -Ridley et al, 2005/2009 (10.1007/s00382-*

009-0646-0) -Charbit et al., 2008 (10.1029/2008GL033472) -Fyke et al., 2011 (10.5194/gmd-4-117-2011) -Vizcaino et al., 2008 (10.1007/s00382-008-0369-7) I also think the authors could refer to the body of literature which describes the role of climate forcing to influence ice sheet evolution. A few recent examples: -Quiquet et al, 2012 (10.5194/tc-6-999-2012) -Yoshimore et al., 2012 (10.1175/2011JCLI4011.1)
The introduction has been completly rewritten to take into account that aspect.

****P5219L1: The precipitation adjustment over the ocean occurs right around where Greenland is. Can the authors link the over-precipitation they see over GrIS to this broader bias (which is apparently important enough for oceanic processes to need to be fixed with a virtual precipitation pipe to the Pacific)?*
We have checked this aspect with respect to the climatology. The bias in Central Greenland is not linked to the overestimate of precipitation over the Nordic Seas. Rather, it is linked to the fact that not enough precipitation occurs at the coast of Greenland due to the low altitude seen by the atmospheric model and thus more precipitation occurs in Central Greenland where in reality the air mass has already dried out. This aspect is now mentionned in the manuscript.

P5219,L4: How are the coupled ice-sheet/climate models LOVECLIM1.2-AGISM, and iLoveClim/GRISLI) different?
There are many differences between the two coupled systems, already in the treatment of the ice-sheet dynamical equations between GRISLI and AGiSM. AGISM consists of two 3D thermo-mechanical ice-sheet models for both Antarctica and Greenland. While the Antarctic component describes ice-shelf and grounding line dynamics, the Greenland model only deals with grounded ice (Huybrechts et al., 2011), contrary to the version of GRISLI we are using. Leaving aside the dynamic of the ice-sheet, the coupling procedure is different as well. One obvious difference is that we use a absolute forcing while LOVECLIM-1.2-AGiSM is coupled through an anomaly mode approach.
Moreover, there are some differences in the treatment of the surface mass balance. Huybrechts et al are using a PDD approach as well, but the treatment of refreezing of melt is different. We are using the PDD version of Fauso (Fausto et al., 2009), with a parametrization of refreezing with altitude, cf. Charbit et al. (2013, section 2.1) for a discussion. For the coupling in LOVECLIM1.2-AGISM, the refreezing scheme is the one of Janssens and Huybrechts (2000), which takes into account the energy (latent heat) released by the refreezing to melt the ice and additionally a condition on water supply from the melting snow to generate runoff.
Finally, we do not consider the fact that liquid precipitations may refreeze while this is taken into account in Huybrechts et al., (2011).
We have accounted for these in the revised version of the text which now reads : « ***The two coupled systems are different both in the coupling method and in the ice-sheet model itself. The Greenland ice-sheet model used in LOVECLIM1.2 (AGISM) do not account for ice-streams whereas the GRISLI model in iLOVECLIM does. In the coupling method, the main difference is that LOVECLIM1.2 (AGISM) uses an anomaly mode for the coupling where we use absolute fields. Finally, there are some differences in the refreezing schemes used for computing the surface mass balance (compare Fausto in Charbit et al. (2013) and the scheme in Janssens and Huybrechts (2000)). The latter differences are expected to be of less importance than the differences arising from the use of an anomaly coupling method versus direct forcing fields method.*** **»**

P5219,L4: elaborate on how the model decides where to put ice streams. A map of where these occur over the simulated GrIS would be useful, or at least a verbal description.
We guess that the reviewer means P5220 ? We have added the following on how the ice-streams

are determined:
« The location of the ice streams is determined by the basal water head, with ice streams regions corresponding to areas were the sediment layer is saturated. »

***P5219,L6: How does this calving scheme work for GrIS? What does it generate? the authors say 'no significant ice-shelf areas are expected', but, what does the model actually do?*
The scheme is already explained at P5220 where it is said : « Calving at the ice shelf front occurs when two criteria are met: (a) the front grid point has a thickness below 150 m and (b) ice coming from an upstream point fails to maintain the thickness above that threshold. »

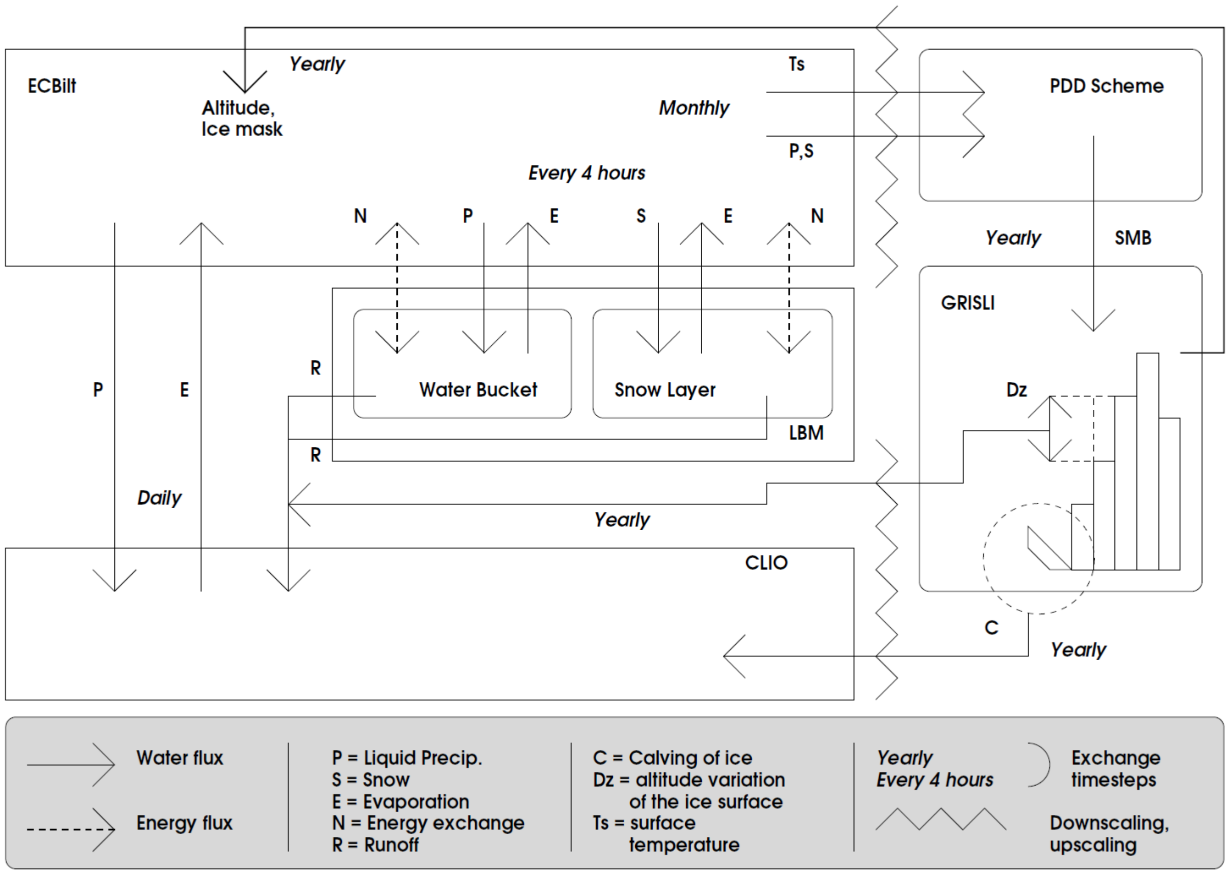
P5221,L2: I appreciate that the authors are adopting a non-bias-corrected approach, despite the difficulties it introduces in reproducing reality.
We are thankful for this appreciation.

P5221,L8: Perhaps clarify what the authors mean by the 'seasonal cycle perturbation bias' argument. Would this go away if the authors used a monthly bias correction?
The paragraph was not well written indeed. We have re-written that part for clarity. It now reads : « ***Moreover, instead of using anomaly fields with respect to the simulated present-day climate as inputs of the ice-sheet model, we use absolute fields from ECBILT for precipitation and precipitation. Actually, in order to remove potential biases of the climate model, the perturbation method is sometimes used in studies based on climate models including an interactive ice-sheet component (e.g, Vizcaino, 2008, Huybrechts et al., 2011). However the perturbation method relies on the strong assumption that model biases prevailing in a given climatic context are of the same order of magnitudes as those of the present-day one. Using absolute fields is therefore an important requirement to consistently use the model in different climatic contexts.*** **»**

***P5221,L14: the authors use a PDD-based scheme in the coupled model. Can you quantify how much this scheme disregards conservation of mass/energy, where it is employed?*
There is no issue with the conservation of mass involved in the PDD scheme. Accumulation is given and melting / runoff is calculated from the temperatures provided to the PDD scheme. We have furthermore taken great care of mass conservation within the coupled system ECBilt – GRISLI – CLIO. For energy, there is no strict conservation indeed, since the atmospheric model provides a temperature field and not energy fluxes. However, quantifying how much is disregarded is complicated : it would involve implementing an energy conserving scheme first and then diagnose the implied flux differences between the PDD and the energy method. While desirable, this is beyond the scope of our present development. To give us confidence on what is done in the current study, we have compared the pattern of accumulation diagnosed in the simplistic snow model in ECBilt and the upscaled accumulation pattern from the PDD on the ECBilt grid. The result (not shown in the manuscript for reasons of length and clarity) is that the two methods agree very well. The simplistic method in ECBilt is based on a crude conservation of energy, thus giving us some confidence that we are not completly out of the range of what would ideally be the goal. Ultimately, we do want to develop an energy conserving scheme, as is stressed already in the manuscript (previous version, p.5232, L.7-10).

***P5221,L14: How is snow simulated elsewhere in iLoveClim? Is this default snow scheme operational over the GrIS, and if so, what happens when this snow melts over the GrIS in this scheme? Particularly, if ice is exposed, can the bare-ice surface warm above 0C? Does this exposure occur consistently with what occurs in the PDD scheme, as far as melt rate coefficients are concerned?*

Within the ECBilt/Land Surface Model (LBM) and when not coupled to the Ice Sheet Model, the snow bucket scheme is coupled to the atmospheric temperatures through an energy balance computation (see new figure 4). Thus, there is a snow layer computed in LBM that is valid on the ECBilt grid. When snow melts in ECBilt / LBM, the energy flux is modified in ECBilt and so is the atmospheric temperature. If ice is exposed, nothing is taken specifically into account since there is no specific ice surface type in ECBilt. Thus, the temperature can rise above zero, which is incorrect in reality. However, one should note that replacing that scheme with a scheme where the energy is consumed without ice being melted would be incorrect from the point of view of energy, since that would introduce an infinite energy sink in the model. In regions where the base ice is exposed, the temperature is thus overestimated in ECBilt.
From the point of view of the PDD model (cf. right hand side in new figure 4), the Surface Mass Balance is calculated independently. That is, the temperature from ECBilt is used to compute the Surface Mass Balance. The ice exposure may occur independently from what is going on in ECBilt, as correctly inferred by the reviewer. It shall be reminded that this is indeed how the PDD method was developped initially : using observations to tune a Surface Mass Balance computation ; in the observation used, there is no guarantee anyhow that the zones obtained for bare ice are coherent. This is an issue of the PDD method in itself, that we cannot solve at present albeit, again, by implementing an appropriate energy balance method.



New Figure : Coupling scheme between ECBilt, GRISLI and CLIO

P5222,L15: "bilinear interpolation considering a GRISLI grid point and the 15 surrounding corresponding ECBilt center grid points": it is not clear why the authors need 15 points to do bilinear interpolation? Why not just 4?

When interpolating with the neighbouring grid points, there is no reason a priori to limit oneself to the 4 neighbours. This can be done with any number of neighbours, with a decreasing weight with distance. We have implemented a scheme that can use 4, 9, 25 or 49 neighbouring points. However, it was set in the 4 neighbouring points mode in the current run, contrary to what we stated in the previous version. It is now corrected in the text. New text version reads : « the 4 surrounding points ».

****General: It seems like a basic figure of the SMB generated by the model is a very important missing piece of this manuscript. Please include a figure of the SMB field over the GrIS, as generated by the model, and compare this field to available observations or other simulations.*

The surface mass balance is now included in the manuscript and discussed in a dedicated paragraph, as requested, with new figure 11 :

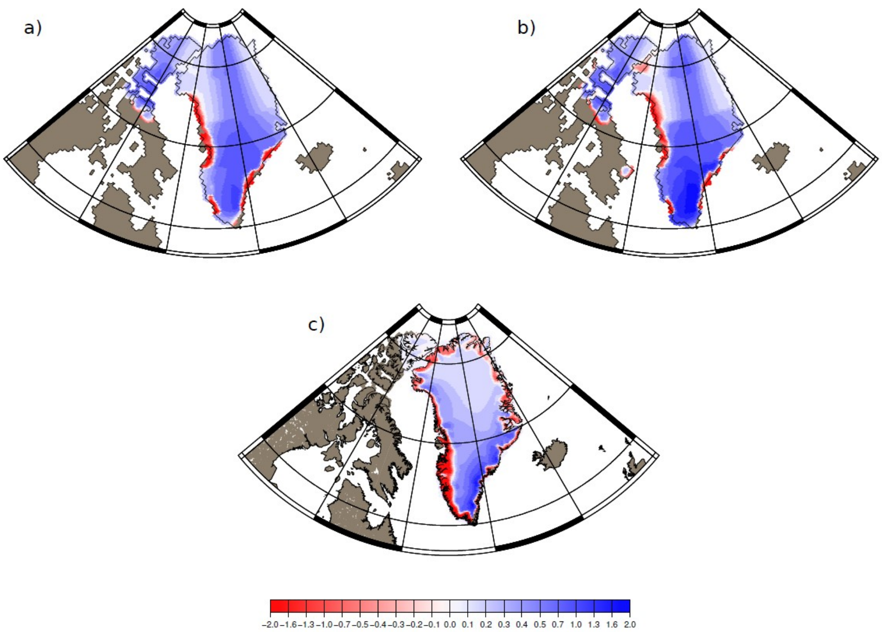


Fig. 11. Comparison of simulated Surface Mass Balance for the Greenland ice-sheet. a) is SNOW b) is PRECIP c) results from the MAR regional climate model at 5 km resolution forced by reanalyses over the period 79-88 from **?** doi:10.5194/tc-5-359-2011.

Also, does GRISLI also receive surface temperatures as a boundary condition from the climate model?
Yes, ECBilt provides the surface temperature as an input to the PDD scheme that calculates the Surface Mass Balance of GRISLI as shown in the coupling method figure, new figure 4.

P5222,L15: I suggest a re-write of the interpolation description, to first summarize the entire process of moving T/P from the ECBilt grid to the GRISLI grid, then describing in detail the separate downscaling processes. An additional flow-chart of the downscaling process would be a great addition.
We have re-written the paragraph following the suggestion. The text now reads :
« Due to the large difference in spatial resolution, two methods may be taken to obtain the temperature and precipitation at the fine resolution of the GRISLI grid, that is used to compute the SMB. The first method is to perform a simple interpolation for each GRISLI cell, using the neighbouring ECBilt cell. Whenever this is done in our scheme, we use a bilinear interpolation considering a~GRISLI grid point and the 4 surrounding corresponding ECBilt center grid points. Applying this simple interpolation to both temperatures and precipitation yield already reasonnable results as shown hereafter. To further improve the representation of the local surface temperature for the purpose of the SMB, we use a vertical downscaling approach that takes explicitly into account the differences in altitude between the ECBilt and the GRISLI grid. »
We did not design a flow chart only for the downscaling, but the generic process of exchange of fields between the model sub-components are now shown in the new figure 4 and the actual vertical downscaling process is shown in the new figure 5 that was requested in the following comment.

P5223,L23: Could the authors provide a schematic figure, that shows an example of the vertical downscaling of temperature?
We have created a new figure 5 that provides an example for the vertical downscaling of temperatures between the two models, as requested, see hereafter. The process of downscaling is described as follow on the figure : (1) the temperature at the maximum and minimum GRISLI altitude in one ECBilt cell is calculated (2) these two temperatures are used to compute a linear relationship between altitude and surface temperature and derive the vertical temperature gradient, gamma (3) using that computed gradient, the temperature at all altitudes on the GRISLI cells pertaining to that ECBilt cell.
The figure and a short description is now included in the revised version of the manuscript.

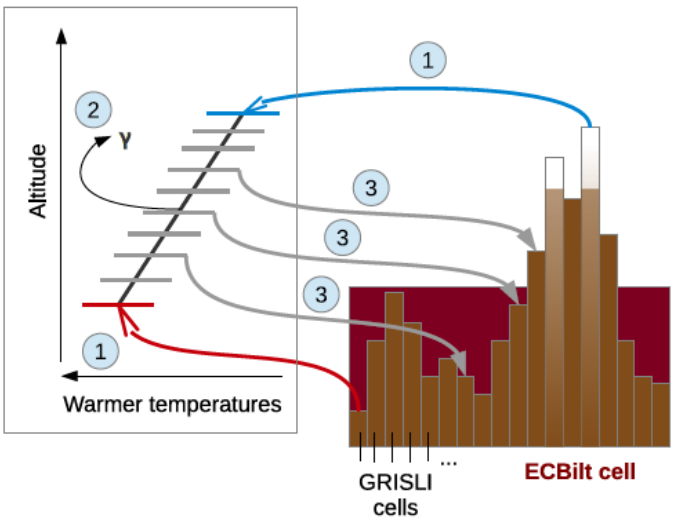


Fig. 5. ~~Transient equilibration~~ Scheme presenting the method used for the vertical downscaling. Numbering indicates the order of processing. 1) the ~~equilibrium runs SNOW~~ temperature at the highest and ~~PRECIP:~~ lowest Grisli point (tails of the blue and red arrows respectively) are retrieved for the given ECBilt grid cell (boundary in violet). 2) A vertical ~~axis~~ lapse rate is ~~ice volume, horizontal axis~~ γ is ~~simulation years~~computed from the these two extrem temperatures, using the line defined by the two temperatures and elevation extrema in addition to the ECBilt cell temperature. 3) Using γ, temperatures are derived for all altitudes in GRISLI, knowing their altitude.

P5223,L4: what is 'along-slope'? Vertical gradient?
Since we compute a vertical temperature gradient that is always based on the surface temperatures, the gradient computed is different from the vertical temperature gradient in the free atmosphere. The specific wording « along-slope » is there to introduce that difference. We have clarified the text to express that notion as follow :
« It shall be noted that the vertical surface temperature gradient computed with this method is different from the the free atmosphere vertical temperature gradient, since it is computed only using surface temperatures. Our method thus provide a different surface temperature gradient than would be computed using the free atmosphere one. We refer to it as the "along-slope surface temperature gradient" »

Figure 4: Could a clearer figure be a simple regression of non-downscaled vs. downscaled, temperatures? Perhaps this could be a second panel, of this figure.
We have created the figure as requested by the reviewer (see below). In our opinion, it does not provide a much clearer picture of what is shown. Since we agree that the figure 4 was not very clear, we have now removed it from the manuscript. The information on the downscaled versus non-downscaled is now presented in a new figure with a spatial distribution of the temperatures (see next comment).

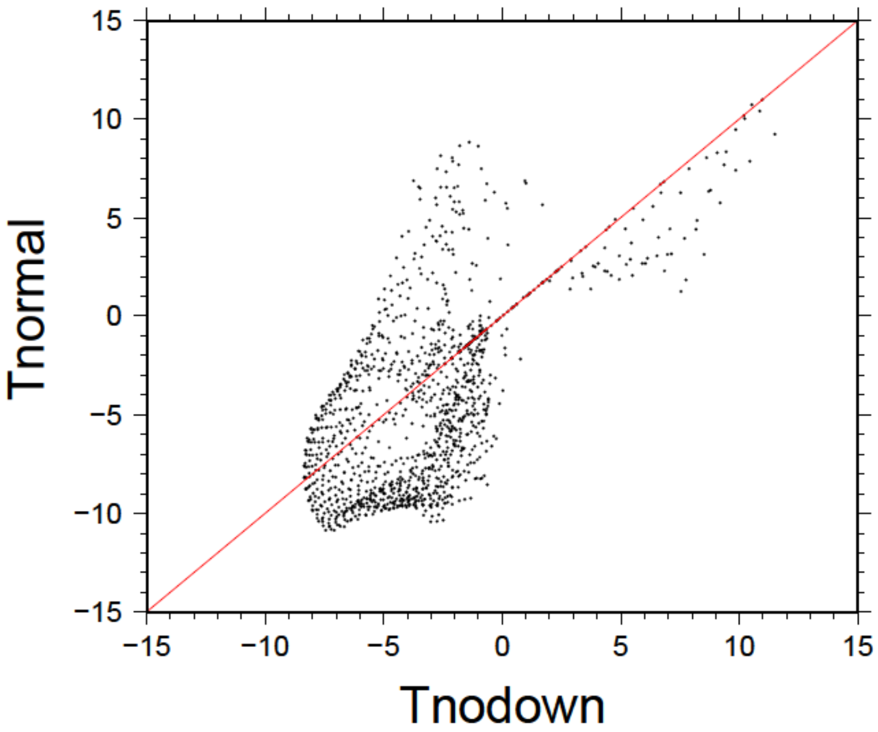


****Figure 4: Annual mean temperature is much less important than summertime temperature, for GrIS SMB. What does this plot look like for JJA temperature, for example (sort of like Figure 11, where the authors show July temperatures)? And/or, a spatial map of the difference between the non-lapse-rated vs. lapse-rated temperatures, would help show readers how this scheme affects downscaling.*

We have designed and included a new figure on the spatial effect of the vertical downscaling

(see below). It obviously tend to cool down the higher altitude region and warm up the lower altitude ones (for example in the Alps). In the case of Greenland it shows that the regions that are higher than the given ECBilt cell are cooled down and the ones that are below that mean area are warmed up. The region of warming between the southern tip of Greenland and the northern part is at the boundary between the ECBilt cells and are thus at lower altitude than the mean. The northern most cells in ECBilt are overlapping with the ocean and are thus cooled by the downscaling process.

A text describing the figure accordingly has been added to the new version of the manuscript, that reads : « ***The spatial effect of the vertical temperature downscaling is shown in Fig. \ref{downsc_effect} for the annual mean. Moutain ranges are depicted as colder than the rest of the region (e.g. the Alps), as expected for higher altitudes. Since we show the difference between the downscaled field and the non-downscaled scheme, there are regions in Greenland where the ice-sheet surface is lower than the mean of the ECBilt cell, others where the ice-sheet is higher than the corresponding ECBilt cell. Hence, the alternation of warmer and colder areas as a consequence of the downscaling over Greenland*****. ».**

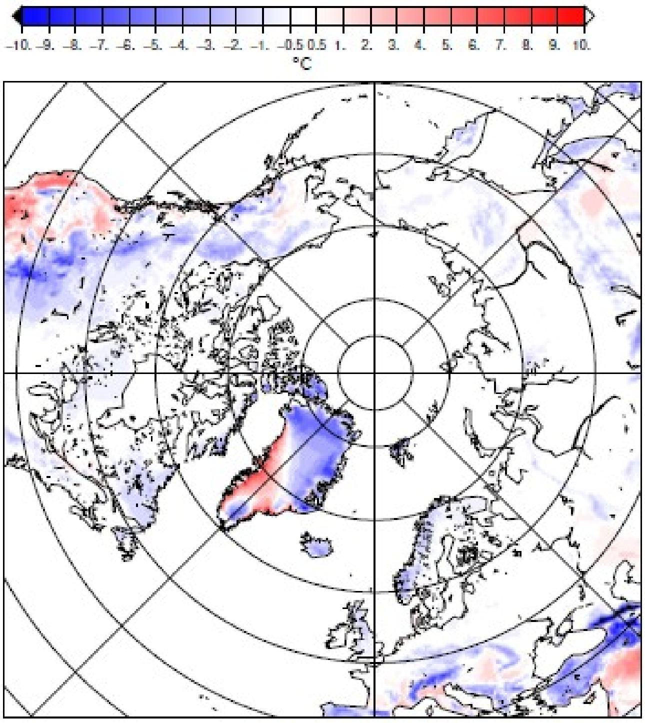


Fig. 6. ~~Ice sheet thickness (~~ Annual mean temperature anomaly due to the downscaling procedure, in ~~m) of observed ice sheet thickness (a)~~degrees, ~~of simulated thickness of SNOW experiment (b)~~ computed as the difference between the downscaled temperature and ~~of simulated thickness of PRECIP experiment (c). The red contour line corresponds to~~ the ~~observed present-day grounding line~~non-downscaled temperature fields.

****P5223,L12: I would expect the difference in downscaled temperatures to be also a function of the difference between ECBilt and GRISLI elevations?*

This is exactly the case as is already shown in equation (4) where the ΔH term is the difference in elevations between the two grids.

Also, what are the range of lapse rates, and the spatial pattern of lapse rates that the scheme generates?

We have included a lapse rate figures for July and February in the revised version of the manuscript (see below). The obtained surface vertical temperature gradient for July is of 5-6°C/km over Greenland, in good accordance with observations that show a vertical surface temperature gradient of ca. 4-5.5°C/km in July between Summit and Swiss camp (Steffen and Box, 2001). For February, we obtain a lapse rate between 6.5 and 7.5°C for the steep areas of the ice-sheet, also in broad accordance (though on the low end) with (Steffen and Box, 2001) that report between 7 and 10°C/km. A paragraph has been added to the manuscript to describe the figure that reads : « *The computed lapse rates are in good accordance with present-day observations for Greenland \citep{steffen01}, indicating a higher gradient between coastal Greenland and central Greenland for winter (6 to 9{\degree}C per \unit{km}) and a reduced gradient for summer (4 to 5{\degree}C per \unit{km}). Our values of 5 to 8{\degree}C per \unit{km} in February and 4 to 5.5{\degree}C per \unit{km} in July are falling into the range of observations.* »

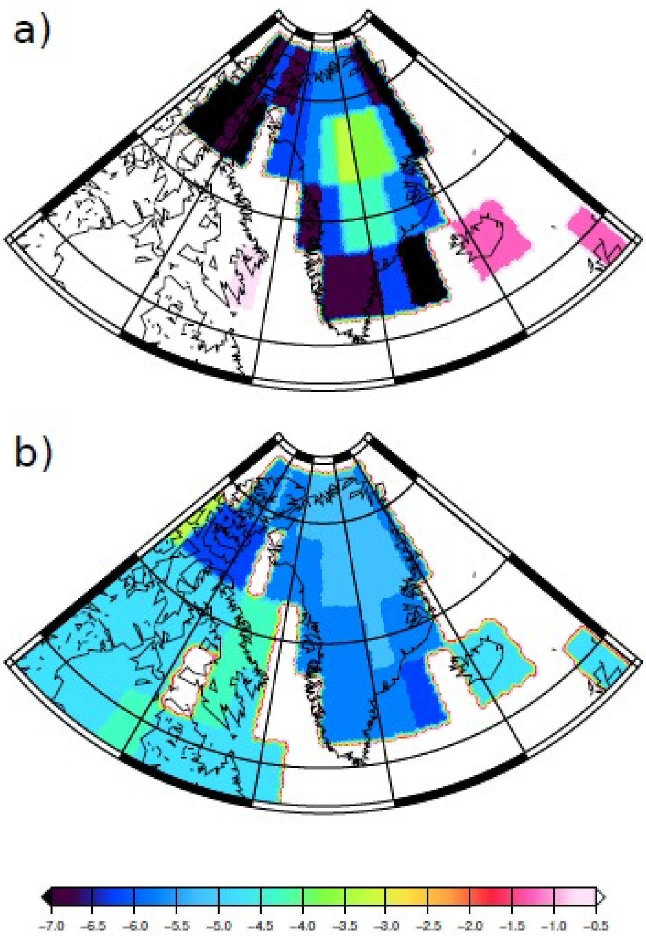


Fig. 7. Calculated lapse rates within the downscaling procedure, in degrees per km. a) is for February, b) for July. The computation of the lapse rate involving a linear regression over three temperature points, there is no lapse rate calculated when the correlation coefficient of the regression line is too low: hence the large white areas over north America in panel a).

Does it produce negative lapse rates (inversions)?

No the scheme do not produce inversions, since the ECBilt model lack entirely the planetary boundary layer dynamic needed to generate the inversions.

****P5224,L5: Is it either entirely snowfall, or entirely rainfall, for an entire month, if the monthly temperature threshold is below 2C (so that even if it is warmer than 2C, it is still snowing, for example)? Or, more preferably, can the model switch between snow and rain, on a sub-monthly basis (e.g. on the atmospheric model timestep)?*

The question should be related to P5225, around L5 (reference to P5224, L5 point to a text on the ΔH term discussed in the previous question). We have two schemes in the model. The PRECIP scheme where the total precipitation (liquid+snow) is given to the PDD scheme every month and there is decided which fraction of it is snow based on the downscaled temperature on the GRISLI grid ; the SNOW scheme where the amount of snow is computed in ECBilt, based on temperature of the ECBilt grid every 4 hours. The PRECIP scheme, as pointed out by the reviewer, decides of the amount of snow based on monthly high resolution temperatures on the GRISLI grid. It thus uses the high resolution temperature pattern but a

low time resolution for the fraction of snow / liquid. The SNOW scheme is the opposite : it has a high time resolution with a computed transition between the snow and precip every four hours (cf. new figure 4) but using the low spatial resolution temperature. A perfect scheme would have the high resolution for both time and space, but then would require to apply the downscaling of temperatures every four hours, which would be very time consuming. We agree with the reviewer that the SNOW scheme might be more physical in terms of succession of snow / liquid precipitation phases ; however it neglects the important elevation differences between the two model grids and its relationship to the transition from precipitation to snow (the fact that it « snows in the mountain » and « rain in the valleys »).
What is the best scheme between the two is debatable and this is precisely the reason why we conduct a sensitivity study on that aspect in our manuscript.
To clarify that aspect we have modified the text as follows :
« *For the computation of the accumulation on the GRISLI grid, two methods are possible. The first (called SNOW hereafter) consists in using the snow amount calculated in ECBilt every four hours, depending on the temperature calculated in the ECBilt grid. It provides a method that has a high temporal resolution with transitions in the amount of liquid precipitation and snow every four hours, but neglects the fact that more snow should occur at higher altitude on the GRISLI grid since it is presumably colder. To transfer that accumulation from the ECBilt to the GRISLI grid, a simple bi-linear interpolation is performed. The second (called PRECIP hereafter) is the opposite: we take all the precipitation (liquid plus snow) on the ECBilt grid, accumulate it over time and transfer it to the GRISLI once a month (cf. Fig. 4). Since we also have the surface temperature downscaled on the GRISLI grid, it is possible to use it to compute the fraction of the total accumulation that is delivered as snow. Deriving a snow [...]* »

P5225,L12: Does ECBilt atmospheric circulation actually respond dynamically to changes in GIS topography, during the course of a simulation?
Yes, the orography is provided back from GRISLI to ECBilt every year or coupling period as described on the new figure 4. This is exactly what is said at the cited paragraph. We have modified it in the revised version to include the citation of the new figure.

****P5225,L16: How is albedo, and albedo changes over the ice sheet, represented, given the PDD nature of the SMB scheme, the coarse nature of the model. This is important to describe, since the authors explicitly claim 'albedo is exchanged' in the Conclusions.*
The text was not clearly formulated indeed. What is exchanged is an icemask, not albedo directly. Whenever the icemask is set, the surface type in ECBilt is set to be ice and consequently the albedo calculated as a snow albedo. The actual value of the albedo is set through the normal scheme of ECBilt which depends on season, location etc.
The text has been modified and now reads :
« *[…] as ECBilt distinguishes between the different surface types. The surface albedo in ECBilt is then computed from the icemask provided, as in the standard. The orography on the GRISLI grid [...]* ». We have modified also the text in the conclusion to reflect that not the albedo but an icemask is exchanged.

****General: Are ablation zones simulated in the model?*
What is the spatial pattern of SMB on the GRISLI grid?
The Surface Mass Balance is now added to the manuscript as mentioned before.

How is the snowpack simulated? Again, this is very important information, which is not presented at all in the current manuscript.
It is not clear to us what the reviewer means by « simulating snowpack ». If it refers to the simulation of snow compaction, water infiltration in the snowpack, radiation balance and snow density, this is certainly not simulated at all. The only point where a snow layer is taken

into account in the model is for the simple energy balance computations on the ECBilt grid to determine sublimation/melting of the one snow layer of LBM. There is no compaction, no ageing of the snow etc. On the GRISLI side, nothing is done either since the only process is the PDD scheme that only use different coefficients for the melting of snow and ice, as described.

P5225,L20: Are isolated 'glaciers' actually simulated by GRISLI during the coupled simulation? This contradicts the statement: 'the ice reaches the sea all around Greenland'.
There is indeed no glaciers simulated. The SIA/SSA approximation used in GRISLI is not meant for the simulation of glaciers anyhow. The text has been modified to account for this.

****General: I think the manuscript need much more justification and motivation for focussing on the sensitivity of an equilibrated ice sheet to the form of precipitation downscaling. Do the authors consider this the most sensitive aspect of the model design, or the aspect of the model that is least-constrained? As it stands, it seems like this sensitivity test is very arbitrary and not well motivated, given the wide range of other aspects of the model that could use sensitivity testing. I'm not sure the simulations presented here really explores model performance, especially since precipitation downscaling has very little effect on model results.*

When a model development is performed, many sensitiviy studies can always be performed. One has to make a choice given 1) the limited possibility of performing the actual sensitivity tests (each experiment performed costs about a month of computation) 2) the relevance of the tests when the model tuning is performed. Clearly, we have performed much more sensitivity studies than the ones presented here in the course of the model coupling. However, the sensitivity studies once performed are not with the given components of the model as they are described in the manuscript. Thus, we cannot present them simply. As noted by the reviewer, the downscaling method and its link to the conversion of precipitation to snow is important, even if it does not impact the results greatly. If that is not satisfactory justification, then the only option we see is to remove entirely any sensitivity study and only keep one simulation as a pure description manuscript. It is clearly beyond the scope of the present description manuscript to perform a full suite of sensitivity studies to each parameter introduced in the model coupling. We leave the decision on that aspect to the editor.

****P5226,L9: this section describes 1 control simulation, but Table 1 lists 2 control simulations. It is very unclear how to reconcile this, and also, which control simulation is described in the text.*
There is only one CTRL simulation, from which we extract the accumulation calculated from the PRECIP or SNOW setup. CTRL1/2 pertained to a previous version of the manuscript. It is now suppressed in the current version and the legend of the figures (previous #8, 9, 10, 11) where modified to account for this. We apologize for the confusion generated.

****P5226,L15: For 'SNOW' simulation, the authors say that ECBILT provides the snow accumulation. But earlier, the authors state "To this end, ECBilt does not provide snow only but the total precipitation (liquid plus snow) to the coupler." What does ECBilt provide in terms of snow/rain, and what are the authors considering snowfall, in the SNOW simulation? Do the authors assume snowfall=total precipitation? This is very confusing, and hinders any further analysis by the reader.*
There is clearly some confusion here since the quoted statement (coming from P5224, L18) refers to the PRECIP experiment where indeed the total precipitation is transferred, not the snow accumulation. In the revised version of the manuscript, the paragraph that was previously at P5224, L18 has been rewritten to clarify the difference between the SNOW and PRECIP setup (as mentioned earlier in the response to comments).

P5226,L18: the authors should comment on the potential impact of a transient spin-up (i.e. through the last glaciation), on results. At the very least, this is because in the very first sentence of the manuscript is: "The most prominent feature of the Quaternary era is the alternation of glaciated and less glaciated periods" yet after this, little mention is made of the transient nature of glacial evolution, or how the model would perform in a transient, or at least a paleoclimate snapshot context.

We have added the following sentence to the manuscript :

« It should be noted that integrating the coupled climate system for 14\,000\,\unit{yr} under constant climate forcing is a theoritical state study; indeed the ice-sheet evolution over the last 14\,000\,\unit{yr} has seen a part of the last deglaciation and was therefore further from equilibrium than simulated in our setup. »

Figure 6: 'Grounding line': do the authors mean, 'ice margin'?

The legend of figure 6 as been corrected as per suggestion.

Figure 6/7: Combine these figures to show both absolute thicknesses and differences in an easily viewed manner. Currently, Figure 6 is not very useful, given the small ratio of the difference to the absolute thickness.

To answer other aspects from the reveiwer#2, the figure showing the thicknesses have been expanded to 5 sub-panels. It is therefore not practical to add the differences as well on the same figure.

Figure 7: Difference both SNOW and PRECIP against 'observed', to make it easier to interpret the performance of the PRECIP experiment.

Given the very little difference between the SNOW and PRECIP experiments, the difference of each with respect to the observed will not show any visible difference. It is the same issue as the one outline by the reviewer for the previous version of figure 6. We chose thus keep the difference of SNOW to observed and between the SNOW and PRECIP.

****P5227,L5: I don't believe a 33% overestimate of volume, and ice reaching the coast everywhere around Greenland, is actually consistent with the final statement of the manuscript: "Results of a 14 000 yr integration under pre-industrial yields a reasonnable ice-sheet distribution on the overall which brings us confidence for the use of this coupled model for long term climate change applications." It rather seems to me, like the model is simply growing the maximum-sized ice sheet possible, and is only being limited from growing further by the presence of a coastline. Is there actually an ablation zone in the model? What is the fraction of ice lost to calving, versus surface melt? Generally, and importantly, it is not clear the current result adequately validate the model for future science, in any substantial way.*

As shown in the revised version of the manuscript in figure 11, Greenland is not fully glaciated in our simulations. As mentioned before, the fact that the ice-sheet is expanding on the continent but not bridging the straits to Baffin island (an easy thing to do with the ice-shelves) shows as well that the statement of the reviewer « only being limited from growing further by the presence of a coastline » is incorrect. With respect to the SMB, please refer to the previous answer given on the same comment above. We have removed the cited sentence from the manuscript.

****General: A much stronger test of the model would be the difference in ice sheet geometries that results from snapshot simulations of multiple climate states (for example, LGM, Eemian, and present-day). Also, a very good indicator of model performance would be at what CO2 level the GriS deglaciates completely (for example, is the GrIS*

still stable at unrealistically high Cretaceous CO2 concentrations?).
All these simulations will certainly be performed in the future, but are beyond the scope of the present manuscript which deals with the description of the coupling method and of the initial results. Computing a transient LGM simulation (as the ice-sheet at the LGM is very much dependent on its glacial cycle history) require about ~6 months of computation time. We have performed a short experiment with a 4xCO2 concentration which shows a decrease of the ice-sheet volume as expected (see figure below).

P5227,L16: “where observations are giving ice-free conditions” -> “where no ice currently exists.”
The paragraph was corrected as suggested.

P5228,L13: Here is the first suggestion that there is actually more than one CTRL simulation.
The text has been clarified and now reads : « it is useful to compare the two accumulation fields (SNOW and PRECIP-type) from the CTRL experiment where the ice sheet ».

P5228,L13: “Analysing the CTRL runs reveals that the results over Greenland are very similar “ : : : Similar to what?
The sentence has been modified and now reads : «   are very similar to the SNOW and PRECIP experiments ».

****Figure 8: It is not clearly explained: - what information the CTRL simulation(s?) give, in the context of the PRECIP/SNOW simulations -why the off-GrIS differences in accumulation are so large (according to the color bar changing the downscaling scheme over the GrIS results in >150% changes in accumulation over Canada, Baffin Bay, for example) -why Figure 8b has a large greyed-out region in the Southeast of the plot.*

The text referred to has been clarified an now reads : « Comparing the accumulations for the CTRL(-SNOW) and CTRL(-PRECIP) reveals that the results over Greenland are very similar with less (overall ' 10 %, locally 30 %) accumulation for the PRECIP setup. Conversely, the same computation of snow from the total precipitation (PRECIP) tends to increase accumulation on the southern border of Greenland, where the topography is steep and the mean temperature close to the freezing point (see hereafter). In this region, the higher resolution of GRISLI allows for snow fall whereas ECBilt mainly computes rain. The fact that the total precipitation is taken into account by GRISLI as either snowfall or rain is seen below 70° latitude as the CTRL(-PRECIP) run displays up to 150% more accumulation falling as rain, which is not included in the CTRL(-SNOW) experiment. »

For the grey area, the caption of the figure has been modified and now reads: « Grey areas correspond to areas without accumulation in PRECIP (as can be seen in Fig. 9e); division by zero causes an error which is expressed in grey color ».

P5229,L3: Please clarify the description of the observational dataset used to compare the model against. For example, does Arctic precipitation over land come from Serreze and Hurst, or New et al?
The climatology used is a mixture of different source. On the continent, it is CRU10 (New et al., 1999). Where latitudes are above 60°N, data comes from Serreze and Hurst (2001). Over the oceans where latitude is less than 60°N, data from gpcp-pentad79-98 is used. (Adler et al., 2003)

Figure 9: what is 'TS1'?
There is no TS1, it is a text artifact added by the GMDD editorial office after submission (!). It was corrected in the revised version.

Figure 9: It seems like a large signal of temperature difference between observations (TS1??) and the model over the ocean comes from differences in sea ice extent?
As stated before, there is no TS1. The sea ice extent simulated by the model is very close to the observations (cf. Goosse et al., 2010).

****General: Have the authors simulated the recent historical period with the model, and if so, how does accumulation pattern change? For example, one would expect recent historical accumulation to be higher than preindustrial accumulation due to a warmer climate state.*
We have not performed an historical run.

P5229,L24: "somehow in between these two extreme cases" is not a clear statement.
The sentence has been rewritten and now reads : « At ECBilt resolution, Greenland is never dry enough in the high altitude regions and the moist advection is too widespread in the interior of Greenland. »

****P5229,L28: As the authors note, mean annual temperatures are not as important as summertime temperatures, in determining SMB and eventual ice sheet geometry. Yet, the discussion of temperatures is dominated by discussion of the annual temperature bias (3 paragraphs, versus 1 paragraph for summer temperatures).*
The discussion on SMB has now been added.

P5229,L28: Now the authors mention only one CTRL simulation, not two.
There is indeed only one CTRL simulation.

****General: Can the authors carry out a recent historical simulation, to actually generate directly comparable temperature fields instead of indirectly comparing preindustrial simulated temperatures to modern reanalysis?*
Given the differences between the model and the observations for an equilibrated run, the very small differences of 0.1 – 0.2°C will not change the overall pattern of the results. Besides, as mentioned by the reviewer previously, what need to be done is a run from the LGM to the present + historical given the long equilibration timescale of the ice-sheet.

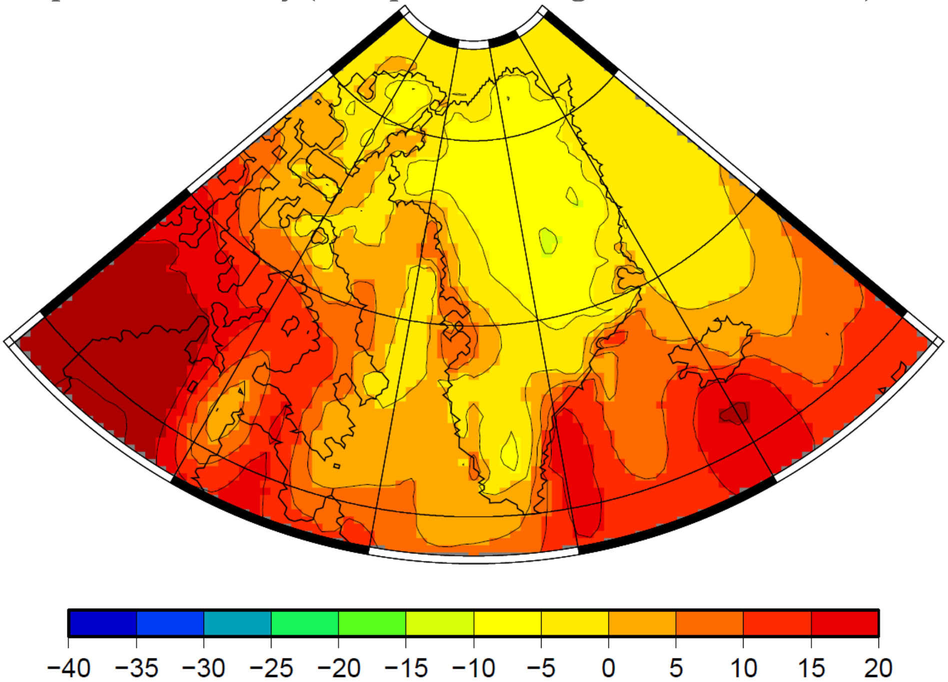
P5230,L20: As the authors note, it is not clear that the improved annual temperature in the SNOW simulation is a valid result, given that they indicate it results from cancelling errors.
We do not claim it is a valid result, we just mentioned the fact that the annual temperature is improved and give the reason for it.

Figure 10: Does a warm (red) color mean it is warmer or colder in the model, relative to reanalysis? I am having trouble reconciling the text description of these figures, with these figures themselves. For example, there is a warm (red) bias in southern Greenland in figures 10c/d, yet in the text the authors say: "there is a common pattern of cooler conditions : : :. in central and southern Greenland".
There was indeed a small mistake in the text. It has been corrected in the new version that now reads : « there is a~common pattern of cooler conditions than climatology of about 2 to 4\,{\degree}C in central Greenland. In northern Greenland, the cooler bias is even more pronounced in the PRECIP experiment (Fig.~\ref{fig5_marianne}d). These differences can

readily be explained by the large overestimation of the ice-sheet thickness in the SNOW and PRECIP simulations, causing a~higher elevation. » And at the end of the paragraph : « In South Greenland, the temperatures are overestimated in both the PRECIP and SNOW by up to 5{\degree}C causing more ablation and a lower ice sheet thickness than observed (Fig.~7a). ».

****P5230,L27: The large overestimate of summertime temperature is very confusing, since this should drive much excessive melting, which should in turn lower the ice sheet. It is not clear at all to me why the ice sheet is able to grow under the presence of such a high positive summer temperature bias, even given the positive accumulation bias.*

I think there is a confusion there that we can explain. There is an overestimation of the temperature of less than 10°C in most of the ice-sheet, except in the southestern part where this is above 10°C (cf. figure 11b of the previous manuscript version, with a observed ice-sheet). We agree this is a lot, but since the temperature in the climatology is -10 to -15 °C in the same area, this implies that the temperature in our model is still negative. Hence, using the PDD method, there is not much melting in summer anyhow. This can be seen very clearly in the absolute temperatures for July (the equivalent of figure 11d but absolute) :



In addition, it is not clear that the author's explanation that ice height growth alone is remotely capable of countering this large bias (by up to 13C), as the authors suggest. Using a conservative July lapse rate of 5C/km, this implies a vertical change of over 2 km, which is not the amount the modelled ice sheet grows.

There is a misunderstanding here on what we are discussing in the text : we discuss the fact that the differences between figure 11b and 11c,d are due to a change in height. The difference between 11b and 11c,d is only a few degrees (maximum 5°C). Since we overestimate the altitude of the ice-sheet by an amount of 500 to 800 meters, even with the « conservative » estimate of the lapse rate given by the reviewer, we are able to explain the difference between 11b on one hand and 11c,d on the other hand. We did not understand where the 13C mentionned in the comment of the reviewer comes from.

Furthermore, it is unclear how modelled temperatures of (apparently) 5-10C above zero can occur *in July over the ice sheet (e.g., Figure 11a plus Figure 11b), given that excess energy should go to melting ice, and not raising surface and air temperatures. Is the bias in Figure 11b is due to a lack of back-coupling between the SMB model and the climate model, in terms of energy?*

As discussed in the text before, the simulated temperature in July are not 5-10°C above zero in some places, but -5°C in most of the ice-sheet. As mentioned in the new coupling method paragraph, since this is the temperature without taking into account the ice-sheet melting, it does make sense. In a way it does show a lack of coupling between atmospheric model and the ice-sheet model as proposed by the reviewer. This is now stated explicitly in the text in the which reads : « *Part of the latter mismatch is due to lack of energy coupling between ECBilt and GRISLI in the present version of the coupling approach. Temperatures in the atmospheric model should be buffered by the presence of the ice-sheet (through the take up of latent heat needed to melt the ice), a process absent in the current version of our coupling procedure.*».

****General: I think the consistency, or lack thereof, between summertime temperature biases, precipitation biases, and resulting ice sheet evolution, needs to be much more clearly explored, definitely as more than one short paragraph.*

A large part of the discussion has been rewritten with clarity and consistency in mind.

****P5231,L24: I think that the fact that the simulated ice sheet reaches the coast is not so much due to the lack of higher-order ice dynamics, but rather large climate-side biases. It is hard to judge this, however, without an actual map of SMB on the GRISLI grid, which clearly shows the integrated effect of precipitation and temperature biases. SMB is also the basic boundary condition that the climate model generates for the ice sheet model, but it is not shown at all in the manuscript. Lack of an SMB figure is a critical missing component of this study.*

SMB has been added, see the previous comments.

****P5232,L10: This point, that the heat fluxes to the ice sheet PDD model are not reflected in equal but opposite fluxes to the atmosphere model, during run-time, should be made clearly earlier in the manuscript, to clearly describe the level of 'coupling' that has been achieved with the model. This is important, so that subsequent science with the model can be interpreted in the context of level-of-coupling. As one specific example, described above, it is hard to interpret the near-surface temperature biases (Figure 11b) and their impact on SMB, since it is not clear whether surface temperatures are actually pinned to near the freezing point during the summer.*
This has now been included in the new coupling paragraph and the new coupling figure 4. Cf. response to comments above.

P5232,L14: : : :"Seem to be in better agreement with pre-industrial temperature reconstructions: : :" - this comparison was not actually shown at all in the paper; the authors only compared against reanalysis temperatures.
No figure is shown on this aspect, but it is discussed in the manuscript in the previous version already, on P5230, L5-8.

P5232,L9: "...relatively reasonnable (sp) distribution of ice in Greenland": what are the author's criteria for "relatively reasonable"? This is a very subjective statement, given the excessive area and volume the model produces.
The sentence has been modified and now reads : « Regarding the two accumulation

techniques, both versions yield a too thick ice sheet owing to overestimated precipitations in central Greenland.

P5232,L22: I cannot understand why "cumulated differences (???) in these regions" shows that "the model is able to reproduce regional effects."
The sentence has been modified and now reads : « Only the southern tip and the Arctic coast of Greenland behave differently with altitudinal differences of up to 500\,\unit{m} between both at the end of the simulation. »

P5233,L9: What is the weak dependence of ice sheet thickness to temperature fields in central Greenland? Where was this shown in the text? Do the authors mean the their argument that reduction of surface temperature biases comes from growth of the ice sheet?
What was meant here is that PRECIP and SNOW do not yield any difference in central Greenland since it is always snow in both schemes there. The sentence has been modified and now reads : « Testing different coupling methods, we find that the PRECIP and SNOW methods yield very similar ice-sheet thickness in central Greenland where the prevailing cold conditions at all spatial resolution (ECBilt and GRISLI) always produce snow accumulation. »

****P5233,L16: How will a scheme to redistribute moisture to avoid excessive accumulation in central Greenland work? The problem here is a regional climate bias, not a problem with the sub-gridscale distribution of precipitation, I believe.*
Two approaches could be taken there : first precipitation redistribution to allow the ECBilt cells with high slope in GRISLI cells to account correctly for more moisture down, less up. The second thing would be to make sure that the ECBilt model does 'sees' Greenland at the right height, by modifying the way the topography is taken into account dynamically in the model. But this is only to be done in the next version of the coupling.

P5233,L14: 'slight preference of direct accumulation of snow computed on the ECBilt grid.' Do the authors mean, omitting the dependence of precipitation type on temperature produces a better result? This does not seem like a useful result, because it just indicates that consciously neglecting one physical process within the model fortuitously cancels the errors of another poorly resolved process or regional bias.
As mentioned previously, the two schemes PRECIP and SNOW are both valid ; both are using a temperature dependence to the snow / precip limit and both have a form of downscaling. As mentioned previously in the response to question on P5224,L5. Each method « ***conciously neglect*** **» a physical process. PRECIP neglects the conversion between snow and liquid precipitation at higher temporal resolution than montly and SNOW negects the temperature dependence of that limit at higher spatial resolution than the ECBilt grid. There is no method that is more physical than the other. Both are equally good or bad methods in that respect. The only true advantage of SNOW is that it is consistent in snow amount between ECBilt and GRISLI. That is the reason why we prefer it and it is now stated as such in the manuscript.**

****P5233,L22: It is simply not clear to me how this one set of equilibrium preindustrial simulations that result in quite over-estimated ice sheet area and volumes 'brings us confidence for the use of this coupled model for long term climate change applications'.*
The sentence has been removed from the manuscript.

****General: The conclusion is somewhat confusing, and difficult to tie with the various results presented earlier.*
Conclusion has been partly re-written for clarity.

****General: Extensive work is required to improve the grammar and flow of this manuscript.*

The final revised version will be proof-checked by a native speaker.