Answers to reviewers'comments on manuscript entitled «Modelling Northern Hemisphere ice-sheets distribution during MIS5 and MIS7 glacial inceptions»

We thank both reviewers for their very useful comments. We now think that the manuscript has been largely improved. We addressed all the reviewers' requirements by adding a substantial number of ice-sheets simulations to the ones previously included in the first version of this manuscript. All the figures and text have been updated appropriately. We have also made several changes to the manuscript to improve readability and added new figures. In particular:

- a new figure about pre-industrial ice cover has been inserted, Figure 7
- a new figure about lapse rate and precipitation sensitivity tests has been added, Figure 12
- Figure 11 has been modified to account for lapse rate and precipitation sensitivity tests as well
- the Ice sheet model description and methods section was rewritten and corrected
- CESM results section was further detailed
- Steady-state ice sheet results section was partly reformulated
- Transient ice sheet experiments results were structured in different subsections and reordered. We also added the analysis about lapse rate and precipitation formulation
- The Conclusion section has been amplified

This response consists of two parts:
- a point-by-point response to the reviewers' comments
- a copy of the revised submission with all the changes highlighted relative to the previous submission. Additional text or reformulated text is shown in **bold font** while the part that have been removed are indicated by **[...]**.

On behalf of co-authors,

Florence Colleoni

Reviewer 1:

1) Would it be possible to see the sensitivity of the ice-sheet results to the choice of PDD parameters in the paper? If redoing a run or two is too onerous in terms of resources, could a quantitative idea be imported from the sources cited for the values? The exact numbers used, for the temperature lapse-rate particularly, can have a significant effect on the ice volume you can grow, and it would be good to know what sort of error bar just those parameters could put on the results. On this note, the authors might want to look at Gregory et al, Clim Past 8 1565-1580 (2012), who also looked at feedbacks during inception in a transient AOGCM–ice-sheet simulation - they found a very large sensitivity to the values of the parameters in the PDD scheme.

To explore the impact of PDD parameters, we performed two series of experiments to test both lapse rate and precipitation formulation impact on ice growth:

[1] Lapse rate impact: we discovered that our steady-state experiments did not account for any lapse rate due to a bug in the steady-state mass balance routines. We then re-run all the ice sheet experiments, steady-states and transients, and we tested the lapse rate values of 8°C/km, 5°C/km and 0°C (no elevation correction) respectively.

[2] We also tested the impact of precipitation formulation on surface mass balance. The standard experiments described in Sections 3.2.1 to 3.2.3 use Charbit et al. (2002) precipitation scheme, i.e., they account for elevation changes through the saturation of vapor pressure. We ran all the experiments with also a precipitation formulation not accounting for those elevation changes, i.e. full precipitation. Charbit et al. (2002) precipitation scheme decreases precipitation along with temperature and thus in our case strengthened the lack of precipitation caused by CESM cold bias.

With the lapse rate experiments, we obtained an upper bound (8°C/km) and lower bound (no elevation) ice thicknesses and we therefore follow the suggestion of one of your later comment. In Sections 3.2.1 to 3.2.3, we choose to show the final ice extent of the experiments which use a lapse rate of 5°C/km since they are more realistic in the context of glacial inception. The lapse rate over an ice sheet evolves with time and if we consider that over Antarctic and Greenland, present-day lapse rate ranges from 7° to 9°C/km (Krinner and Genthon 1999), starting with a low lapse rate value over ice free area is more logical.

Results of those sensitivity experiments are displayed in Figure 11 and in Figure 12 and described in Section 3.3 «Lapse rate and precipitation impact on ice sheet distribution». As you can see, the spread between the various experiments is not large and furthermore this analysis was particularly interesting since it strengthened our conclusions.

2) The climate state from CESM is applied to GRISLI without any attempt at removing the known biases of the present day climatology of the model. This choice of forcing is not unreasonable, but I think it is important to know what the effects of those modern biases really is. The relevant temperature and precipitation biases are shown (fig 6), but I would like to see what sort of ice-sheet grows when forced with the raw CTR1850 climate. Although it's not going to be a simple linear term that could be subtracted from the ice-sheets grown for the other states, it would be good to see how the model biases translate into ice-sheet anomalies for one, known state at least.

As you suggested, now Figure 7 includes two pre-industrial variables coming from our climate experiment CTR1850 and its associated ice-sheet simulations. The figure is described in a paragraph of Section 3.2.1. We show the perennial snow cover from the CESM simulation and the ice thickness from a 100,000 model year steady-state experiment. As you will see, the cold bias of CESM does not produce unreasonable ice thickness during pre-industrial time period and the ice is growing where CESM produces perennial snow.

3) I found the description of the construction of the transient temperature forcing Trec in section 2.2.1 rather unclear. For one thing, equations 1-3 cannot be the exact story, as the units do not add up, and as the variables are described the only variation in time comes from Tindex. I found the role of the Tindex also unclear - I would expect it to modulate the interpolation between Ts1 and Ts2, so that the climate forcing at the beginning of the transient experiment was the same as in steady-state experiment 1, and the forcing at the end of the transient was the same as in steady-state experiment 2. As written, it appears that the Tindex term is simply added in, which means that the forcing at the end of the transient is instead rather different from that in the second steady state experiment. If this were

the case, it would be no surprise that the transient runs ended up somewhere rather different to the steady state simulations of 115 and 229kyr - especially for the 115kyr case, where Tlic is large at 115k (fig 3). Whatever the case, could this be explained more clearly in a revision, please? As it is, I'm unsure how to view the results of the transient experiments.

We reformulated and improved the ice sheet methods description in Section 2.2.1. We corrected the equations and we chose to follow the recent formulation of Quiquet et al. (2013) since they used the «Index forcing» method with the same ice-sheet model. As you said, for the beginning and the end of each transient experiments, climate forcing correspond to that of the steady-state experiments, without any additional temperature perturbation.

4) For the transient run, two temperature indices are generated, one including only the feedbacks actually present in the CESM steady state runs (Tgis) and the other attempting to include cryosphere impacts as well (Tlic). The differences between the two sets of runs are then used to denote how elevation and albedo feedbacks help the ice-sheets grow. The situation seems a little more complex than this to me. After all, the PDD scheme used to translate the climate into the SMB forcing already includes an elevation feedback term through the lapse rate adjustment. All of the runs thus include some cryosphere feedback to the climate forcing. Does this mean that part of the Tlic correction is double counting? Should it really be interpreted as showing the effect of large-scale cryosphere impact, rather than everything including local feedbacks? I guess I'd expect the albedo part of the cryosphere adjustment term to be more important anyway to be honest, but I think this could usefully be discussed somewhere in the text.

I think that you are right. However, after thinking carefully to this problem, it appears to be slightly more complex than what you suggested. The original index from Quiquet et al. (2013) used to force our transient experiments comes from central Greenland and is a representation of temperature evolving through the last two glacial cycles. This temperature index results from the interaction of all the components of the Earth climate system, i.e. regional feedbacks all together. As a consequence it accounts for a lapse rate corresponding to a particular ice topography which evolved with time. We have no idea of the relationship between the lapse rate and Greenland ice topography through time. If we extract part of this index, as we did in our work and we associate its magnitude to elevation/albedo feedbacks, we do not have any guarantee that this partial index will represent a realistic lapse rate /elevation feedback. This is why, in the revised version of our manuscript, we performed the transient ice experiments using LIC index with different values of lapse rate as described above, as well as no elevation feedback.

5) pg6239, line 25 states that the transient runs should, "theoretically", end up in the same place as the steady state runs at 115k and 229k. I don't think I agree with this. If the forcings in the transient run were changing slowly enough compared to the adjustment timescale of the ice-sheet that the transient run represented a series of quasi-equilibrium states then this might be true, assuming there were no hysteresis- like effects in ice-sheet growth (which we know there are). However the whole of the transient run is only 10kyr, with a climate forcing that changes significantly (the 125- 115kyr Tlic index alone is 12K in amplitude), and I'd say the icesheet is unlikely to be in quasi-equilibrium under such a forcing. The simulation results bear this out - even the case of T115-GIS, which has a similar ice volume to SS115, has significant differences in ice-sheet topography. On a related note, the first paragraph on pg 6240 isn't very clear - could this be rephrased?

You are right. We removed this statement and we reformulated this paragraph. We also introduced some statements in the transients experiment Section 3.2.2 to highlight the differences in accumulation time between steady-states and transients. Furthermore, there is also a different accumulation time between the two time periods considered here. MIS5 ice only start accumulating toward 121 kyrs BP in our transient experiments since air temperature is too warm and ablation is consequently large. On the contrary MIS 7 ice accumulates from the very beginning because temperature is always negative and ablation is lower than during MIS 5. We described all those differences along with the results of the sensitivity experiments in Section 3.3.

Reviewer 2:

The main measure for the distinction between success and failure of the MIS5 and MIS7 inceptions is the ice volume. In the *successfull* MIS5 inception, a substantial fraction of the ice grows in the Bering Strait area, where no ice sheet should grow. It is therefore not obvious why this ice volume should be considered for a comparison with the sea level reconstructions. For MIS7, the sea level reconstructions differ greatly (at some times by 60 m). Therefore comparing to these reconstructions is of limited use.

We removed the ice from the Bering Strait of our sea-level equivalent ice volume calculations and we corrected Figure 9. About MIS 7 calculations, as you say, reconstructions greatly differ and this is why we are interested into MIS 7 and we think that the comparison between these reconstructions and our modelled ice volumes is valuable. In fact, our simulated ice volumes are reasonable and fall in the range of those reconstructions. Furthermore, the aim of our study is not to reproduce perfectly this stage since, as you pointed out, there are missing feedbacks in our CESM climate forcing. I think that what is important here is not the exact magnitude of sea-level drop, but the fact that from the beginning of this stage sea-level is lower than present-day, contrary to most of the last four interglacials and contrary to MIS 5.

Adding the temperature index and the linear interpolation between both climate states results in a double correction of the temperatures, especially close to the S2 time slice. Could you please comment on this?

We reformulated and improved the ice sheet methods description in Section 2.2.1. As you said, for the beginning and the end of each transient experiments, climate forcing correspond to that of the steady-state experiments, without any additional temperature perturbation.

Similarly, the precipitation seems to be double-corrected in equation (3). Staying with the precipitation, but jumping into the results chapter, on page 6240 the authors write "If the temperature field becomes more negative during the experiment, the precipitation is increased to compensate for the fact that the climate might become dryer during the transient experiment and to allow for ice accumulation in region initially very dry.". Please include a detailed description of this treatment in the section on the experiment setup. What are the consequences of this treatment for the model results? How does it affect the comparability of the steady-state and the transient simulations?

We removed this sentence that was completely wrong. Precipitation in GRISLI accounts for the water vapour saturation in atmosphere following Charbit et al. (2002). As a consequence, when temperature drops, precipitation drops as well and vice versa.

General Comments:

A verification run of the of the ice sheet model with the pre-industrial forcing would be crucial for model validation. In the case that you cannot perform a full steady-state run, could you please comment on the response of the ISM to pre-industrial forcing over something like 10 kyrs and compare it to the first 10 kyrs under the MIS5 and MIS7 conditions?

We now include a new figure about pre-industrial control run. Figure 7 includes two pre-industrial variables coming from our climate experiment CTR1850 and its associated ice-sheet simulations. The figure is described in a paragraph of Section 3.2.1. We show the perennial snow cover from the CESM simulation and the ice thickness from a 100,000 model year steady-state experiment. As you will see, the cold bias of CESM does not produce unreasonable ice thickness during pre-industrial time period and the ice is growing where CESM produces perennial snow.

Considering that the authors aim at analysing the role of feedbacks for inception, it is surprising that in the introduction (page 6226, lines 21–25) they state that "CESM 1.0 provides the possibility of computing dynamical vegetation and dynamical aeolian dust distribution, but we turned off those options and prescribed the pre-industrial vegetation cover and dust distribution over continents in all our experiments since in this work we focus only on the impact of external forcing on the Earth's climate.". Why are feedbacks that control the response of the climate system to changes in orbital forcing deactivated?

As underlined in our manuscript, the main reason is that we wanted to explore the role of external forcing only and compare their impact on two different glacial inceptions. We were not interested into vegetation and dust impact although we are aware of their importance for polar regions climates (Colleoni et al. 2009a and b.). Moreover, since MIS7 is colder than MIS5, we wanted to limit the causes of discrepancies between those two stages to properly interpret the ice-sheet simulations. Since we are only looking at the very beginning of inception, we think that neglecting vegetation and dust would not alter significantly our results as shown by Kohler et al. (2010). In fact, dust records only peak when climate starts to be very cold and atmospheric circulation is greatly intensified. This is not our case here. For vegetation, since most of the Eurasian ice sheet lies on the shallow Arctic continental shelf, vegetation feedback is not of great importance during inception. It becomes important when expanding southward at later stages. Vegetation is more important for North America, as demonstrated by Vettoretti and Peltier (2003) for e.g. because North American ice sheet is mostly continental. However, in our ice sheets simulations, the growth of North American ice sheet seems not inhibited by the lack of vegetation feedback.

For each of the previous two glacial inceptions (MIS5 and MIS7), the authors analyze two time slices. For MIS7, they choose two time slices in the cooling phase with falling sea levels, while for MIS5 they choose one time slice rather early in the interglacial (judging by the development of the sea level and by the Greenland temperature index used later in the work, that shows a strong warming between 125 kyrs BP and 120 kyrs BP, figures 1 and 3), and one later during the inception. The difference between the choices of the time slices obstructs a comparison of the model results.

As mentioned in the text, we chose the pre-inception runs based on maximum sea-level and we now precise that is was based on Waelbroeck et al. (2002) and we choose our inception runs based on minimum of insolation. The fact that the index peaks towards 121 kyrs BP is due to the observations used to compile this index (see Quiquet et al., 2013 for further details). All sea-level reconstructions do not agree on the exact timing of maximum sea-level during MIS 5 (see Caputo, 2007 for example). In the reconstructions, it ranges from 125 kyrs BP to 120 kyrs BP. As a consequence, we do not see any particular problem in comparing the results between those two periods since: (1) we do not aim at reproducing the exact ice distribution of those two stages but rather catch the main discrepancies due to external forcing and (2) as we answered in a previous comment, the most important fact is that MIS 7 sea level stays below present-day value and MIS 5 goes beyond present-day level. However, to better understand the impact of this index in the case of different PDD parameters, we performed additional experiments accounting for different lapse rate values or no elevation feedback. Results are displayed in Figures 11 and 12 and described in Section 3.3. Depending on the parameters, Greenland ice sheet does not disappear completely during the transient experiments. This is the case for the simulation accounting for a lapse rate value of 5°C/km, which is the solution on which we base our main analysis. In the previous version, the results presented some biases linked to the absence of elevation feedback that we have now fully corrected in the present version.

The global application of a Greenland temperature index seems somewhat trouble- some. Climate models are used exactly because the climate does not change uniformly. During the LGM, Greenland shows extremely strong changes in temperature (e. g. Kim et al., 2008) when compared with other regions, especially considering that the surface elevation in Greenland remains largely unchanged. The concern that this temperature index might lead to too extreme results is mentioned by the authors in the results chapter (page 6240 lines 12–13) and is supported by the complete destruction of the Greenland Ice Sheet during the transient simulations of MIS5. In reality, parts of the ice sheet did survive this time period.

About the index, the paper that you mention refers to LGM, which is not the case of our paper focusing on inceptions which are by far less extreme than glacial maxima. Furthermore, we only use the very beginning of both cycles in the temperature index for our experiments. When we wrote for example that the index might be too extreme, it was referred to the fact that this temperature index comes from central Greenland and is applied to ice free regions of Eurasian and North America. However, we think that it is preferable to force our experiments with this reconstructions rather than with Antarctic temperature index from Jouzel et al. (2007), since the variability of the index would differ significantly from that of Greenland due to the seesaw effect and other regional climate differences.

Specific comments:

Introduction

Please to include the inception studies by Herrington and Poulsen (2012) and Gregory et al. (2012) here and detail on the additional value of your work. You might also want to mention Ganopolski et al. (2010); Ganopolski and Calov (2011) as examples for 3D ISM-EMIC simulations. Please clearly state that Born et al. (2010) also force an ISM with GCM output for 115 kyrs BP and study the glacial inception.

The introduction has been corrected by properly citing the references that you suggested.

Methods

On page 6226 lines 6–8 the authors correctly refer to Calov et al. (2009) for a study with a comparison of asynchronous and synchronous coupling. However, the context with this study that uses neither of the two methods remains unclear.

You are right, and we remove the reference.

Page 6226, lines 10–14: The use of Greenland ice cores for modulating temperatures goes back beyond Charbit et al. (2002). See Abe-Ouchi et al. (2007) for some references.

You are right, we substituted with Charbit et al. (2007) who use North GRIP d^{18}O to force precipitation over the last glacial cycle.

CESM

On Page 6227, lines 21–24 the authors state that the climate snapshots for the early MIS setups were chosen at the sea level highstands. For MIS5, this clearly conflicts with figure 1 and the model results.

We corrected Figure 1 to display only Waelbroeck et al. (2002) sea-level reconstructions on which we based our choice. As we stated before, sea-level observations and other climate proxies do not agree between each other on the exact time of MIS 5 maximum. Since the most important fact for our experiments is that MIS 5 sea level goes above present-day sea level and MIS 7 stays below, we consider that those discrepancies do not alter the comparison and the conclusions of this work.

GRISLI

Could you please describe how the ice sheets interact with the ocean? A few words on calving and shelf basal melt would be nice, since the parametrizations and thresholds used here can vary strongly between different setups and have a large influence on the results.

We now updated the methods in Section 2.2 with a more detailed description of the parametrisations.

On page 6229 line 23 the authors state "The climate memory of ice sheet is about 5 kyr to 10 kyr". This is a very low estimate considering the 150 kyrs the authors invested into the steady-state simulations and the results of Rogozhina et al. (2011).

Since this sentence was not particularly relevant and since most of the ice sheets results in Section 3.2 were re-written in this new version, we deleted it. Nevertheless, we added some sentences about the accumulation time and the differences that it generates between steady-state and transient experiments.

Results - CESM

On page 6233 lines 23–24 the authors state that "In all the experiments, the Northern Hemisphere mean annual precipitation is reduced by more than 20% with respect to CTR1850" Please distinguish more clearly between local changes and a large-scale mean. Similarly, the "thickening of the sea-ice cover (of about 2 m)" needs a specification of the region affected, especially since here no plots are provided. Please provide more detailed information on the sea ice extent and concentration. Sea ice is

crucial for the climate around the Arctic Ocean. The way it is written in the paper, it appears as if the higher albedo of the thicker sea-ice would directly decrease the precipitation. Please rephrase.

We followed your suggestion and we included a more detailed description of precipitation and sea-ice spatial distribution variations between all the experiments in Section 3.1.

page 6235 lines 4–9: The authors conclude from a comparison of the central Greenland temperature change with the Greenland temperature reconstruction by Quiquet (2012) that their model captures the desired effects on NH scale reasonably well and that the temperature and precipitation anomalies do not affect this. This is a far-reaching conclusion from comparing one temperature point and a comparison with other climate simulations for 115 ka (e.g. the already cited simulations by Jochum et al. (2012) with a high-resolution version of the same climate model) would help adding credibility to this statement.

You are right and in order to strengthen the comparison, we included Jochum et al. value in the Figure 3 as a light blue square. As you will see, their value is similar to our one and therefore, we left unchanged.

Steady-state ice-sheets experiments

As stated above, the steady-state results would greatly benefit from a comparison with a steady-state experiment with pre-industrial forcing. This would also help sorting out errors from the climate model bias.

This has been done in Figure 7. Since almost no ice develops in our pre-industrial simulation, we assume that the results of the paleo simulations are not too unreasonable as well.

page 6236–6237: In the comparison of the steady-state sea level equivalents with reconstructions for the transient evolution during the interglacial and especially during the inception, it should be clearly stated that one should not necessarily expect these to match, since the ice sheets have much more time to grow/melt during a steady-state simulation, than in reality.

We specified according to your suggestion, but we added a sentence in the transient experiments Section 3.2.2 instead.

While the cold bias in CESM results in less precipitation, the low temperatures should foster ice growth, especially in steady-state simulations, where the ice sheets have time to grow. Please comment on (and plot) the time evolution of the modeled ice sheet volumes in the steady-state experiments.

We wrote on the time evolution of ice volume in the steady state experiments with a sentence at the beginning of Section 3.2.1. The spin-up of all the experiments are very similar. The trend is exponential with a quasi equilibrium reached after 50,000 and total equilibrium after 100,000 model years. However, for SS125, the trend is inverted compared to the other time periods since ice volume is decreasing. The total equilibrium is reached after 115,000 model years, which is slightly longer than for the other experiments in which ice is growing. But the difference is not large and it did not change the conclusions of the steady-state experiments section.

page 6237 lines 20–21: why do the simulations only account "to some extent, to the effect of the sea-ice albedo"?

We removed «to some extent».

On page 6238, lines 9–11 the authors write "However, the already cold climate context minimizes the impact of the land-albedo feedbacks that would more quickly cool the climate over the inception areas." Please explain.

We removed that statement since it was wrong. Actually, what happens is the contrary. Charbit et al. (2002) formulation reduces precipitation as temperature decreases. We inserted that statement where appropriate in the text.

Transient ice-sheets experiments

As stated above, I have serious concerns regarding the method used to construct these simulations. I will nonetheless provide comments on the presented results.

Please provide more information on the fate of the Greenland Ice Sheet. It should definitely be included in the ice volume plots in figure 10. The final shape of the Greenland Ice Sheet in the MIS5 experiments is very peculiar. Please provide more information on the evolution of the northern part. Why are the rims of the ice sheet so much thicker than the interior? Could you please plot the ice sheets at their minimum in the MIS5 experiments?

In this new version, results are slightly different because of the error that we detected in the mass balance routines of the steady-state experiments, as stated at the very beginning of this document. Therefore, the «hole» observed in the Greenland ice sheet in the old version of the manuscript disappeared. However, when using a larger lapse rate value, this hole could be observed again in the Greenland ice sheet.

This effect is perfectly understood and results from isostasy. When Greenland ice sheet melts, it leaves a hole in central Greenland which is about 1km deep an below sea level. The relaxation time of this hole is larger than 20 000 years. In some of our old transient experiments, Greenland ice sheet melted completely. In our case, the duration of the transient experiments is 10 000 years, which seems to be not enough to completely reconstruct the Greenland ice sheet and fill the depression caused by isostasy. Furthermore, in that case, the depression caused by isostasy counteracts the lapse rate effect while Greenland ice sheet starts to develop after the peak interglacial and ice mainly accumulates along the margins whose elevation is above sea-level. In addition, in our case, the lack of precipitation in our climate forcing probably slowed down the accumulation there.

It would be a critical problem if: (1) we attempted to reconstruct the real Eemian Greenland, but this not the case since we do not account for all the needed feedbacks as vegetation for example. To see a detailed reconstruction of Greenland ice sheet at MIS 5, I suggest Quiquet et al. (2013), (2) Greenland ice sheet would be accumulating during the peak interglacial instead of melting. However we would like to strengthen the fact that our work does not focus on Greenland, but on inception over the Northern Hemisphere in general. The fate of Greenland implies different kind of assumptions. All the hypothesis mentioned in our work are devoted to the study of North American and Eurasian ice sheets. However we agree that the state of Greenland at MIS 5 is a good indicator of the validity of the experiments. Since no major problems arise, we consider that our results can be physically interpreted.

On page 6239 lines 25–29, the authors write that they would assume the ice sheets at the end of the transient run to match those of the steady-state run. This could be expected if, and only if, the ice sheets were in constant equilibrium with the climate and had no hysteresis behavior. I do not see why either of the two conditions would be fulfilled. Furthermore, as stated above, it seems as if the forcing were different because of the addition of the time series.

You are right and we removed that statement.

What do the error bars in the modeled ice sheet volume time series in figure 8 indicate? Why is this time series not continuous?

We corrected this figure, which becomes Figure 9 in this new version. We changed the graphical representation of the time series that are obviously continuous as you highlighted.

The authors force the ice sheet models with different temperature anomaly time series. A comparison of the modeled evolution of ice area with the forcing might help explaining the differences in the evolution of the ice covered area between the different experiments.

We inserted the forcing indices in Figure 11 as suggested.

Discussion and Conclusion

As stated above, please separate this more clearly into a discussion and a conclusion.

We added a more detailed conclusion part after the discussion.

Technical corrections

The Barents Sea has an s at the end of the first word. 3 out of 4 occurrences are misspelled.
Köhler is spelled with an ö instead of an o.

We corrected the typos.

Title

It should probably be "ice sheet distribution" instead of "ice sheets distribution". Please check with a native speaker, if you have not already done so.

Done.

CESM experiments setup

On page 6227 line 25 "five experiments" should be replaced by "six experiments".

We substituted.

Ice-sheet experiments results

The equation for sea level change (7) should be a sum over the whole model grid.

You are right. We modified the equation.

Figures and tables:

Please use a shared color scale for plots that have the same color scale (e.g. the top row in figure 4).
Also make sure, all levels are clearly labeled on non-linear color scales.
On the temperature scales in fig 4 and 6, the degree-sign is broken.

We corrected as suggested.

Figure 3: Please flip the time axis. Time running from left to right is more conventional in modeling.

We did not flip the axis because this figure represents data only and not model results.

Table 3: Please add units as in tables 1 and 2.

We added the units in Table 3.

Figure 5: Please use a wider color scale. One suggestion would be: < 1 day ice-free, < 1 week ice free, < 1 month ice free, < 1 season ice free, < 2 seasons ice-free, > 3 seasons ice-free, or so (please experiment

with different values) This would allow to distinguish between regions, where practical perennial ice cover is reached and those that are far away from this state. Presently, the plot is practically binary. Please also make sure, that you also plot snow cover on sea ice. Can you explain the snow cover zone reaching south from Greenland to Baffin Island at about 65 to 70°W? Please also add figures for your control runs.

We changed the display and we chose to show only the continental perennial snow cover corresponding to a residence time of 365 days per year. We did not plot the snow cover over the sea-ice because all the Arctic Ocean is perennial except in the Norwegian Sea and Labrador Sea. Plotting sea-ice snow cover would have also decreases the legibility of the figure. Nevertheless, we added a few words in the CESM results description (Section 3.1). Since our climate simulations have T31 horizontal resolution, grid pixels are larger than the actual high resolution coastline displayed by NCL software. The pixel that you see in the Baffin Bay is also the result of the coarse land-sea mask used in CESM.

Figure 6: The label should read "Bias in temperature and precipitation" (a "d" is miss- ing), substitute "as evidenced by the NCEP reanalysis" by "compared to the NCEP reanalysis" (or something similar). Please label the left end of your color scale. As it is exceeded, the value is of interest.

We corrected the caption.

Figure 7: "each of the steady-state experiment" needs an "s" at the end.

We corrected the caption.

Figure 8: please flip the time axis. Why do the ice sheet volumes have error bars? Could you please plot them as continuous time series?

We decided to not flip the axis because in our ice sheet transient experiments, time is really decaying according real time and not model time, from 236 kyrs BP to 229 kyrs BP and from 125 kyrs BP to 115 kyrs BP. Furthermore, transient simulations use the time series displayed in Figure 3. Consequently, to be consistent with the other figures and to facilitate comparison, we plotted our modelled time series on top of sea-level data with a time axis decaying from right to left. We plotted the ice volume time series with continuous lines and we removed our previous error bars.

Figure 10: Please flip the time axis. Please add lines for the Greenland ice sheet. Please add an extra line for the Bering Strait ice sheet.

See comment above. We completely redraw this figure, which becomes Figure 12 in this new version. We only displayed the sensitivity test of lapse rate and precipitation on ice-covered ares. Since, as we explained previously, our study does not aim at reproducing realistic ice sheet distribution due to the lack of feedbacks in our simulations, Greenland ice sheet is only indicative. We inserted a few words in the text on that issue. Finally, we did not plot Bering Strait ice sheets ice volume. The latter has been removed completely from the analysis in this new version.

Citations:

Charbit et al. (2002) is QSR vol. 21, not QSR vol. 23

We corrected the citation.

References used in our answers:

Caputo, R., Sea-level curves: perplexities of an end-user in morphotectonic applications
Global Planet. Change, **2007***, 57*, 417-423

Krinner, G. and C. Genthon. 1999. Altitude dependence of the ice sheet surface climate. *Geophysical Research Letters*, **26**(15), 2227–2230.