

Interactive
Comment

Interactive comment on “Large sensitivity of a Greenland ice sheet model to atmospheric forcing fields” by A. Quiquet et al.

A. Quiquet et al.

aurelien.quiquet@lgge.obs.ujf-grenoble.fr

Received and published: 3 July 2012

We are sincerely grateful for the numerous comments. The manuscript has been largely improved since then. In the following we will answer to each individual of the referee’s comment. Referee’s comments are identified by "RC" and authors’ by "AC".

General comments

RC: This paper describes experiments with the 3D thermo-mechanically coupled, hybrid (solving both SIA and SSA) ice sheet model GRISLI where a number of (8) forcing fields (temperature and precipitation) from temperature parameterization, reanalysis dataset (ERA-40), regional climate models and general circulation models (both coupled atmosphere-ocean and atmosphere only GCMs) are used to force the ice sheet

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

model, by applying a positive-degree day method to compute the surface mass balance. The focus is on how well the resulting ice sheet compares with observed present day ice sheet and conclusions about the forcing fields drawn from the differences. A simple downscaling of the temperature field, with a single topographic lapse rate for the whole ice sheet that varies seasonally with sinusoidal cycle with a minimum in July, is done from the low resolution of the climate models to the resolution of the ice sheet model (15 km) and the precipitation is downscaled with empirical equation linking temperature difference with accumulation change.

RC: The sensitivity of the model results to these parameters, topographic lapse rate and precipitation ratio parameter, is only tested by not applying these and comparing the results, but no tests of how sensitive the result is to the selected value is done. It would be interesting to perform a sensitivity study on the value of these parameters and present the results in the paper.

AC: The aim of this paper was mainly to study the sensitivity of the ISM to atmospheric forcing fields. We fully agree that topographic lapse rate and precipitation ratio are highly relevant in this context but many papers have already dealt with this issue (e.g. Stone et al., 2010, Graverson et al., 2010). The experiments with zero lapse rate have been performed in order to underline the importance of taking surface elevation changes into account and how fast constant surface climate and elevation feedback simulations diverge. To make this idea clearer we modified the text and chose the term “surface elevation changes feedback” rather than “no lapse rate experiment”. The idea behind this approach is to give relevant information on the question: Do we need to couple ISMs with atmospheric models to make future projections? Timing and amplitude of the separation are therefore shown.

RC: The experiments described in the manuscript are very important first step towards coupling the ice sheet model to climate model and the results demonstrate that not all climate models are providing realistic enough climate fields to maintain the current Greenland ice sheet and therefore not possible to couple these directly for projection

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

runs. It is concluded that appropriate downscaling methods are necessary and in some cases the climatic fields should be used as anomalies on top of reference climate fields rather than direct forcing fields.

RC: Before the forcing fields from climate models can reproduce present ice cap it may not be sensible to adjust the ice sheet model parameters, but the author do not explore the possibility that boundary conditions such as geothermal heat flux or the ice sheet's model parameters may be tuned for each forcing field to produce the present ice sheet, do authors think this possible?

AC: The calibration/initialization of an ISM is a difficult problem that would require assimilation methods to be rigorously solved (Arthern and Gudmundsson, *Journal of Glaciology*, 2010). However, this problem can be split into two parts depending on the considered variables. Ice velocity is a diagnostic variable which depends on surface and bedrock topography, 3D temperature field, basal drag and ice deformation properties but does not depend directly on surface mass balance. It is thus possible to calibrate ice flow parameters using only observed ice velocities. It is the approach we chose in this article and indeed it made our calibration almost independent on the atmospheric forcing fields. “Almost” stands for the impact of the temperature field that is a prognostic variable and thus depends on the past ice sheet evolution, past surface temperature and geothermal heat flux. However, we estimate that this effect is of second order compared to the atmospheric field impact (although it may change the value of the calibrating parameters to fit observed velocities). To answer more directly the question, we believe it would indeed be possible to tune the parameters (e.g. basal drag) for each forcing field to try to produce the present ice sheet geometry and observed trend in surface elevation but i) there are many atmospheric models for which it will not work because they have too strong bias ii) there is the risk to produce a velocity field very different from the observed one iii) it will be very difficult to compare atmospheric models iv) it is far beyond the aim of this paper.

RC: No ice sheet or climate model developments are presented, or innovative down-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

scaling or coupling methods developed and no suggestions for a way forward to accomplish a coupled ice sheet-climate model for projections into the future are given in this manuscript. The described experiments are important and necessary first step for the coupling of climate and ice sheet models, but the critical question: would the perfect climate model actually force this ice sheet model into a steady state similar to the present ice sheet? Is not addressed. Can we expect that once the climate models are bias free the coupling to this ice sheet model will be smooth? What criteria should be met? How can the results be assessed?

AC: It is true that we do not present any model development or methodology, and actually, we wanted to use a rather “classical” approach to emphasize the sensitivity to the choice of atmospheric forcing fields. We also stress the importance of not using some of these fields as an absolute forcing. As you also mentioned, we think that this study is a very important first step towards coupling of an ISM with an atmospheric model.

AC: If the ISM was forced by a “perfect climate”, we expect that the response will simulate reasonably well the observed present ice sheet (surface topography, surface elevation change, surface velocity). However we acknowledge that the fit cannot be perfect because of the grid resolution, the deficiency of the model (hybrid type not “full stokes”), the uncertainty on geothermal heat flux, bedrock topography and ice rheology (impact of ice fabric for instance). Note that this difficulty affects all the ISMs. Up to now, the initialization of an ice sheet model for near future experiments is a compromise between the following criteria:

- Simulated topography compared to the observed topography.
- Simulated rate of surface elevation change (dS/dt) close to the observed surface elevation change (satellite gravimetry measurements and altimetry).
- Simulated surface velocity field similar to the observed surface velocity.
- Temperature field in agreement with the vertical profiles measured in the few ice boreholes.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Most authors find necessary to run the model for a few decades (relaxation) to meet these criteria (see Gillet-Chaullet et al., The Cryosphere, submitted, Seddik et al., Journal of Glaciology 2012).

RC: My recommendation is to publish the paper, this study is important, but request that the authors critically assess their results with the aim of giving a clear indication of what has been learned by doing these experiments and what the steps forwards should be to accomplish a coupled model system. The language in the paper is in many places poor and needs good editing, I have pointed out a few places, but good editing and polishing of the text is necessary before the paper can be published.

AC: Thanks to your comments. We have refined our conclusions. We have the text corrected by a native english speaker (from the United Kingdom) and we hope that it will be now well written and more understandable.

Specific comments

RC: Be consistent when discussing the atmospheric model forcing fields or climate model output, I would not call the climate model output datasets as they do not have observational nature, throughout the paper it is called dataset (e.g. page 1045 line 6, line 15, Table 1 caption) or experiment (e.g. page 1059 line 10) - please go through paper and be consistent.

AC: Done, we chose the denomination “atmospheric forcing fields”.

RC: It is interesting the the ERA-40 reanalysis is too warm over the ice sheet, but both RACMO and MAR that use ERA-40 as lateral boundaries are not - does this indicate that the dynamical downscaling done by the regional models add something to the forcing fields that the lower resolution models cannot include?

AC: Yes, the surface conditions from RCM could be very different than the ones from the forcing fields. A lot of processes resolved by the RCMs at the surface as well in for the boundary layer are not taken into account by the ERA-40: e.g. the katabatic

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

wind are not well represented in the ERA-40. A part of these differences is due to the coarse resolution in the forcing fields which does not allow to resolve these (near-)surface processes.

RC: Why not compare the steady state modeled ice sheets to the observed ice sheet? It is confusing to compare volume difference to another volume difference. The text can then be simplified if the modeled volume is simply compared to the observed volume.

AC: Our presentation allows to better show the differences among the model. As the different North and South volumes are not the same, we kept ourselves focus of the deviation from the initial state (observed topography). To account for this remark, we systematically give in all the captions the value of initial observed volume for the considered region.

RC: Suggest to move Figure 4 to Figure 1 as it is an overview figure and it is mentioned first in the text.

AC: We agree and have modified the ordering of the figures.

RC: The Enhancement factors are discussed in section 2.1 and their values are given in Table 2, before the discussion of the atmospheric model output, suggest to move table 2 to table 1 and refer to this table in the text in Section 2.1

AC: Done.

Technical corrections

RC: page 1038 line 11 suggest to rewrite, replace “difficulties” with “biases in temperature and precipitation near the coast”. Or discuss why biases arise, model resolution perhaps?

AC: Done. Yes, very fine model resolution is needed to resolve correctly orographic induced precipitation and temperature pattern (van de Broeke et al. 2008).

RC: Line 3, change “insights on” to insights into line 25, inconsistency in reference, in

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

reference list the year is 2011, but in text it is Robinson, 2010 check which is correct

AC: Done.

RC: page 1039 line 8, suggest to replace “accessible” with “feasible” line 15, add e.g. to reference, or add more, as there are many more publications discussing observations of fast processes in Greenland line 18, suggest to replace “Relevant” with “Applied” line 20 suggest “coupling to a GCM” or “coupling to GCMs” line 21 when forced the GCM output line 29 “projections of the future ice sheet state”

AC: Thank you for your corrections.

RC: page 1040 line 2, suggest to replace “reacts” with “responds” lines 2-4 rewrite, this sentence is unclear, suggest something like “extent of ablation zone is often less than 100 km” GCMs have lower resolution than typical ISMs regardless of the extent of the ablation zone, this sentence does not really make sense. Lines 7-10 unclear sentence, rewrite to make the point clearer line 8 ! explicitly Lines 13-16, also unclear sentence, please try make clearer what is meant with this sentence line 23 “specificities” ! specifications line 26 “ths” ! the

AC: Thanks for your corrections. We have tried to express ourselves more clearly now.

RC: page 1041 line 1, suggest to add “and suggestions” for future attempts : : : line 6 “uses” for what? rewrite sentence, suggest: applies SIA and SSA to solve the Navier-Stokes equations line 11, suggest to use different word, “association” ! combination, or relationship between line 17 are the velocities observed or calculated balance velocities? If balance velocities, what is the SMB used to compute the balance velocity? Line 22 rewrite, either a linear viscous sediment type, or linear viscous sediments

AC: Done. Satellite estimated velocities have been used (Joughin et al. 2001).

RC: page 1042 line 3 specificity ! specialty? Lines 3-4 rewrite, sentence is not clear, suggest: that combines the strain rate components from the Glen’s and Newtonian flow laws. Line 7 multiplication coefficient line 9-10 impact of the fabric – on what? Not a

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

clear sentence, rewrite line 22, suggest to move Figure 4 to Figure 1 page 1043 line 1 would not call the model output datasets as they are not of observational nature line 2-3 not a clear sentence, rewrite, suggest: The ISM requires the climatological monthly mean near surface air temperature and precipitation as well as the climate model's topography. Line 4, suggest: "from a common 20 years reference period, 1980-1999" line 11, rewrite, as discussed in - or explained by line 18 how is the IPSL surface scheme updated? What has been updated, is there a reference? This sentence needs more explanation line 20-26 not well written sentences, please rewrite and be concise and clear line 24 "this case" ! model? Line 25 "enhanced" ! improved?

AC: Thanks, we took your remarks into account and made some clarifications of the text.

RC: Page 1044 line 5-6 scaling effects on what? Explain better, how much different surface climate? In what sense? Line 8 "data" ! model output line 9 either simulation from Fettweis et al. (2011) or "the model output stems from 1958-2009 simulation (Fettweis et al., 2011) line 20 data ! model output line 24 data set ! model output or climate fields line 27 it consists of:

AC: Page 1044 line 5-6: The much higher resolution over Greenland compared to IPSL-CM4 induces scaling effects of the parametrizations that lead to much different surface climate. In particular the impact of orography near the coast is much better represented in the zoomed model, and it can influence moisture transport and temperature on the entire ice sheet.

RC: page 1045 line 6 data set ! forcing field line 15 different datasets ! different models line 21 – is there a reference for this statement?

AC: Done. We added a reference to the precipitation spread figure among CMIP.

RC: Page 1046 lines 1-8 rewrite, this sentence is very confusing

AC: Done.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

RC: line 8 suggest affect/impact the ice sheet model line 10 – here is the term atmospheric fields used, suggest to use this rather than datasets for the model output line 21, even though the scheme is commonly used, that is not a good reason to use it, suggest to add something like: and can be tuned to simulate observed SMB and its variability line 26 The melt capacity computed with the PDD method is first used : : line 28 please explain this better and give reference, why 60%?

AC: We agree that is not because the method is widely used that we should necessarily use it. We complemented in the text that we chose this method because it was easy to implement for a range of atmospheric forcings, because it needs a limited number of fields and these fields are easily accessible from the different modelling groups because they are widely analysed. On the other hand, in a paleo-reconstruction context, this approach also provide an easy estimate of past SMBs where this variable is not constrained. More sophisticated methods, such Energy Balance Modelling (EBM) require a lot more variables, like short wave radiation, humidity, etc. The PDD method does not require this kind of input, which is difficult to obtain for long past time periods, and additionally it allows to take into account altitude-temperature feedbacks. Another alternative would be the SMB gradient method, as used in Helsen et al. (2011). But if this method is appropriate for RCM forcing fields, it is not suited for coarse resolution GCMs. Line 28: We made the text clearer about the refreezing and added a reference to Reeh 1991.

RC: page 1047 line 1 Remaining melt capacity is used .. What is the time step size in these PDD computations? Line 11, again, can you give a better justification for using a simplified scheme? Does it give realistic results? Line 12-14 how big is slight difference? can you be more specific, does it mean that refreezing does not matter? Line 17 can you validate and thereby justify the use of this simple partitioning between liquid and solid precipitation? Does it give realistic results?

AC: The PDD are computed every month but only the annual SMB is used in the mass conservation equation. Refreezing and partitioning of total precipitation into rain and

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

snow are of course an important issue in SMB calculation, but we can slightly moderate the importance of those. On the one hand, these two processes play a significant role only at the margin and the order of magnitude of these terms is substantially smaller than the one of ablation rate except in a very narrow range close to the equilibrium line. We acknowledge that refreezing and partitioning may modulate the surface elevation-surface mass balance feedback but this will mainly affect the experiments in which this feedback is important (those that are already diverging from the observed state). On the other hand, sophisticated refreezing and partitioning models require numerous variables that are generally not available for an ISM: firn layer for the refreezing, vertical atmosphere profile for partitioning, etc. The parameterization of these processes is a big issues but is not the aim of this paper. We can mention that we tested the model with different parameterizations (both refreezing and partitioning) but the differences in the simulated topographies were small enough to consider the forcing field itself as the dominant uncertainty.

RC: Page 1048 lines 5-7 rewrite, this sentence is not clear, suggest: The hypothesis that the sensitivity of the results to topographic lapse rate correction is of secondary order compared to the different forcing fields is tested in Section 3.5 line 15, add extra digit to the value, or remove the digit in line 16 line 16 remove “approximately” page 1049 line 6 suggest: still affected by the temperature increase during the last deglaciation

AC: Thanks for the rewriting suggestions. As for the digit, 7.3% is not here the conversion of the 0.07 value. It is the result of Eq. 2. So we are not sure that your suggestion is relevant here.

RC: page 1050 line 9 here should be ESSA line 11 surface velocities? Lines 14-15 suggest to take “us” out and rewrite the sentence, it is not clear, what is a distribution of velocity amplitudes?

AC: Thanks for noticing this error. Sentences have been rewritten.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

RC: Page 1051 lines 2-5 here sensitivity study is briefly mentioned, more detailed explanation and information about the results would be helpful here lines 12-18 this paragraph is very unclear, please rewrite and explain better, what do you mean by this type of experiment is closer to a future projection experiment, in what sense? - because the initialized ice sheet model does not resemble the present day ice sheet?

AC: As we explained at the beginning of this response, this study does not aim at testing the sensitivity of the ISM to a suite of parameters. Of course it is important, and we probably could obtain similar simulated ice sheets with two different atmospheric forcing fields but with a different set of parameters for each forcing. In a sense, one conclusion of our study could be “An ISM should absolutely not be used directly with a given atmospheric forcing fields without an important calibration phase”. Concerning the lines 12-18, we hope that the new version of the manuscript is clearer at this point.

RC: Page 1052 line 1, what do you mean, that the climate is better presented in the southern part in all the climate models? Line 7 is that because of the low resolution of the ice sheet model? - or the physics of the model? Line 10 have you tried running the ice sheet with a higher resolution? Would it give better results? Line 16 – rewrite the English here is not good, delete “in general” and “here” line 22, can you give reference to the “nudging procedure” - can you confirm that it is due to this procedure?

AC: Line 1: “stable” was not the good term. We replaced it with “Similar topographies”. A piece of explanation is given later in the text: the range of temperatures simulated by the climate models for the North is greater than the range for the South. That could be an explanation for the larger range in ISM responses in the North. Line 7: Narrow fjords (a few kilometres large or lower) are observed on the margins of the GIS, in particular in the South and East coasts. They cannot be represented in a 15 kilometre grid. Furthermore, the “shallow approximations” are not suited for such fine scale features: shear stress due to lateral boundaries may become dominant. We recently run the ISM with a 5 kilometre grid and the East margin in particular is better reproduced (lower elevation at the East flank). Line 22: He added a reference for the

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

nudging to Storch et al., Monthly weather review, 2000. It is difficult to say with certainty that it is because of the nudging. What we can say is that advection of temperature and moisture will be similar, since the weather patterns on a weekly time scale will be more or less the same, but other similarities in model biases for REMO/ERA could also be important (causing for example relatively high summer temperatures). It will be difficult to be more specific without further testing.

RC: Page 1053 line 12-13 suggest to rewrite: regional mean precipitation is compared in Figs. 6 and 7b. Line 19 suggest: colder than observations line 20, rewrite, not clear sentence, explain better suggest: the assumption of sinusoidal seasonal variation does not give realistic results lines 23-25, can you show that it is the higher temperature, not the higher precipitation, that causes higher velocity?

AC: Your text propositions have been taken into account. Ice velocity is a direct consequence of dynamical parameters of course and of temperature and surface slopes. With the IPSL forcing, the simulated ice sheet is flatter than with other models (thin ice sheet), suggesting lower velocities. If velocities are higher it is essentially due to the temperature within the ice. We selected the exact same dynamical parameters for all forcings in order to do this type of comparison.

RC: Page 1054 line 5 do you mean thinner ice? Line 9 rewrite, what do you mean by “certainly partly” line 11 either a storm-track, or storm-tracks line 21 replace dataset with forcing field line 27 replace experiment with forcing field line 28 and line 1 on page 1055 – is this only one accumulation field? With two references? Or two accumulation fields?

AC: Text has been corrected. Burgess et al. (2010) provide accumulation over GIS only (ice mask). This field has been combined with the van de Veen et al. (2001) for ice free area. This forcing is available on the CISM website (http://websrv.cs.umt.edu/isis/index.php/Present_Day_Greenland).

RC: Page 1055 line 1 why is the accumulation field not suitable to force the ice sheet

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



model? Explain better line 12-13 rewrite, this sentence is not clear line 16-17 why is this true? Can you explain better line 18, suggest to replace “measuring” with “assessing” line 22-through to page 1057 – why do you compute anomalies with respect to one of the models, why not simply compare the modeled volume with the observed volume? It would make this whole section simpler and easier to read. Line 22, 24 and 25 suggest to replace “variation” with “difference” lines 24 and 28 what do you mean by “standard” line 28 volume difference smaller than?

AC: For the accumulation, we added the statement: “The reason is that, on the one hand, atmospheric models generally do not provide accumulation rate as output. On the other hand, although we have some confidence in temperature anomalies (e.g. via constraints from isotopic content), accumulation is less constrained, being a joint result of both near surface air temperature and precipitation.” Line 16-17: A warm bias at an ice stream terminus is likely to have a higher impact than the same bias in a slowly moving area, because of a possible larger ablation zone due to a spreading of the ice. We added this hypothesis in the text. Line 22-through to page 1057: In this part we replace the precipitation map of each forcing by the here-called reference precipitation map (FE09) in order to assess the relative importance of temperature and precipitation. Simulated volume have to been compared with respect to the simulated volume of the FE09 simulated volume. The difference with the observed value (V_0) is shown to stress on the relative deviation. As a matter of fact it is only a manner of present our results. To make ourselves clearer, we added in the legend of Fig. 9 the value of V_0 for each region. We also replaced the term “variation” by “difference”.

RC: Page 1056 line 12-13 much warmer than what? Rewrite this sentence to make it clearer line 15 simulated volume difference, or anomaly line 16, replace negative with positive line 22 larger than line 24 rewrite, take out “however”, what is reference level? Reference volume anomaly?

AC: Page 1056 line 12-13: Warmer than the reference (FE09), that why we have to statement in page 1055 line 12-13 (see comment above). Thank you for your other

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

comments, we tried to take them into account.

RC: Page 1057 line 5-9 this paragraph is very confusing and needs rewriting line 7, could it mean that the temperature is well simulated in the climate model then? Very realistic? And in second case (line 8) the temperature bias is large? Line 14 precipitation anomaly line 17 It appears – or even, can you say that you can confirm that : : : ? lines 20-23 – this paragraph is confusing, please rewrite to make it clearer

AC: Page 1057 line 5-9: We hope that we make ourselves clearer. Page 1057 line 7: We cannot judge that the temperature is realistic or even well represented but it seems that the atmospheric models agree better. Page 1057 line 17: We have used 8 forcing fields relatively different but it could be difficult to strongly confirm our conclusion regarding the limited number of forcing fields. Page 1057 line 20-23: We have rewritten the text.

RC: page 1058 line 1 each point lines 6-8 again this paragraph is confusing and should be written to become clearer. Volume gain indicates positive rate of change, if the volume gain is decreasing I would think it is still increasing, but at slower rate, but if I read the sentence right author means that the volume is decreasing, is this right? Please rewrite line 11, threshold for line 17 rewrite, suggest: The ice sheet model is forced with the 8 climate fields again, but without lapse rate correction. Why do you now make a sensitivity study for the value of the lapse rate? How sensitive are the results to the choice of lapse rate correction parameter? Line 24 replace “models” with “runs” or model experiments – result in : : : replace “lower value of the volume anomaly” with volume closer to observations line 28 “not adapted” - do you mean the resolution is not high enough?

AC: Page 1058 line 6-8: We added the following to make the point clearer: “This means that the South region is gaining mass with a temperature increase, but at larger rates than for the short term response.”. The section dealing with the lapse rate is now entitled “Importance of the feedback from surface elevation changes”. The aim of

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

this section is to assess the importance of taking into account the elevation changes feedback. Indeed, in a case where ISMs are not included in future projections of sea level rise for example, how large is the error? We realized these experiments to answer this issue. We added the following statement at the very beginning of this section: “Sea level rise projections generally use complex climate models with fine resolution and/or sophisticated physics. ISMs are not yet included in these models and in this section we want to assess the importance of including the elevation changes feedback on temperature and precipitation for the ISM response.” Once again, sensitivity to parameters is not the focus of this study even if it is an important issue of course. Line 28: Yes, as mentioned above in this response, the resolution should be far better to solve the complex flow in the southeastern mountainous region. Additionally SIA and SSA are not designed to solve problems where lateral shearing is dominant (shallow approximation).

RC: Page 1059 line 5 this is a very important point and should be emphasized better, maybe even write into the conclusion? Line 8 replace “global” with “general” line 9 replace “feedback” with “lapse rate correction” line 10 Two model runs : : . How big is huge? Line 11 replace “two models” with “two simulations” or model runs lines 11-14, unclear, please rewrite. This area is very dry, does that affect this conclusion? Line 15 replace datasets with forcing fields line 17 – rewrite, suggest: advance of ice over area which normally is a tundra zone line 19 rewrite, suggest to replace “important driver” with “results are sensitive to the lapse rate correction” - how sensitive are they to the value of the correction parameter? Line 19 replace dataset with forcing field Line 22-23 – unclear to me what this sentence means

AC: Page 1059 line 5: We agree and placed this point in the conclusion. To make the text clearer in this section we replaced the term “lapse rate correction” with “surface elevation changes feedback”.

RC: page 1060 line 1 you have not only proposed this study, but also done it, rewrite line 1 add “forcing” in front of fields line 3-9 poorly written sentence, please rewrite to

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

make the points clearer line 13 replace “major driver” with “important for the” line 17 does it mean that there are small biases in July temperature in the climate models? Line 19-20 and 21-23 rewrite, poor English here, it is not clear to me what the main point of these sentences is line 27 role in long simulations for several thousand years line 27 of secondary order

AC: Page 1060 line 3-9: The whole paragraph has been rewritten. Line 17: We cannot conclude that the climate models have small biases in July temperature, but at least they have a comparable bias.

RC: page 1061 line 1, rewrite to make sentence clearer

AC: Done.

RC: Figure 1 caption: add information about the time period for the averaging as well as the area, is it average over the ice only or all land points?

AC: Done. We used a land mask to compute the average.

RC: Figure 2 remove the title (above the figures) as the information is in the caption (same for Fig. 3)

AC: Done.

RC: Figure 5 caption suggest: Simulated ice sheet topography at the end of the 20 ky constant climate forcing model run.

AC: Done.

RC: Figures 6 and 7, Rewrite the figure caption to make it clearer: suggest to put (a), (b) at the front of sentence for clarity. Suggest to change “for each individual atmospheric model” to “for the steady state model runs shown in Figure 5” (b) add information about how the values are computed, as average over ice mask, or land mask above 75degN? (c) and (d) are the model values the closest grid point to the station? Or average of nearest grid points? What are the vertical bars? Variation in the observation? For how

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

long time period are observations available? Is the initial regional volume the observed one or something else?

AC: Figures 6 and 7 captions have been modified. More information has been added. Initial volume is indeed observed volume.

RC: Figure 8 suggest to rewrite figure caption: Difference in annual accumulation between observation based map (Burgess et al 2010; van der Veen et al.,2001) and downscaled accumulation from each atmospheric model. (what is ISM evaluation here?).

AC: Done. The legend was not clear. It now reads: “Difference in annual accumulation between ISM evaluation (after downscaling, snow and rain partitioning and refreezing) and observations based map (Burgess et al. 2010, van der Veen et al. 2001) for each individual atmospheric forcing.”

RC: Figure 9 is initial volume observed volume? Make that clearer. “for each model” add “run” or “experiment”. Rewrite the caption suggest: change “volume variation” to volume difference, “hatched bars correspond to volume difference (dVi) computed with the precipitation in each model replaced by the Ettema et al. (2009) precipitation map. The first solid bar is the reference volume difference (see comment above, why not compare directly with observed ice sheet volume?)

AC: Initial volume is indeed the observed volume. We added the value.

RC: Figure 11 rewrite figure caption

AC: Done.

RC: Figure 1 in Supplementary material. Suggest to rewrite figure caption: “ Difference between observed and simulated topography at the end of the 20 ky constant climate forcing model run applying the 8 atmospheric forcing fields”

AC: Done.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

RC: Figure 2 and 3 in Supplementary material. Rewrite both figure captions, suggest: Difference in annual precipitation rate (mean July near surface temperature) (for wich period? - same for all models?) between the RACMO model (Ettema et al. 2009) and the other models used in the study (in meter of ice equivalent)

AC: Done, periods have been added.

Interactive comment on The Cryosphere Discuss., 6, 1037, 2012.

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

