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The Cryosphere
Discussions

Interactive comment on "The effect of more realistic forcings and boundary conditions on the modelled geometry and sensitivity of the Greenland ice-sheet" *by* E. J. Stone et al.

E. J. Stone et al.

emma.j.stone@bristol.ac.uk

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Firstly, many thanks to the editor for the handling of this manuscript, the two reviewers and Felix Hebeler for their constructive comments and suggestions on our paper "The effect of more realistic forcings and boundary conditions on the modelled geometry and sensitivity of the Greenland ice-sheet" by Stone et al. We have addressed all their comments, and the following pages detail our responses. Where significant modifications/insertions are made to the text, line numbers refer to the tracked changes version of the original manuscript which is attached as a pdf supplement to this author response comment.

The main changes made to the paper in response to the specific comments are as follows:

(1) More discussion of the implications of not including a full representation of the fast flow features such as basal sliding
(2) Restructuring and expansion of the discussion and conclusions section to include the results in the context of the sources of uncertainty and simplifications/abstractions made.

Due to a small numerical error in the temperature forcing we re-ran the simulations, but the new figures are very similar to the original figures, and the main conclusions and outcomes of the paper are unaltered.

Response to comments by Anonymous Referee 1:

Specific Comments:

I have a couple of problems with the approach of the paper, which you do cover in the discussion, however I think these are very important and the discussion of the future results should be more thoroughly grounded within the simplifications. The simplifications of the model which are important to consider are (a) the impact of running the model offline & to equilibrium (present day runs), and (b) not including a full representation of the fast flow (ice stream) features. The first you justify satisfactorily, however the second simplification here is critical and needs to be emphasised more carefully.

Not having any basal sliding in the model is fairly fundamental for the future predictions, and I don't think you make enough of it. The fast flow features (& marine interaction & subglacial hydrology..?) control the evolution of the ice sheet and if the role of these features is not represented, then any conclusions about the future of the ice sheet need to be made with caution. Having basal sliding in the model will mean that you will get more mass loss dynamically so you won't need to have such a high PDD factor (for example), this will affect the evolution of the interior massively in the future runs as you *show with plot 12. Also p262, lines 7-9: you suggest that including fast flow features could lead to larger dynamical changes. Yes, but if then you can have a smaller PDD factor, then this is a counter argument. Also if the dynamical changes are marine driven, then they will not last very long once the fast flow features are no longer marine terminating.*

Response: We agree that is important to discuss the results in the absence of any fast flow features and how this might affect the results obtained from the Latin Hypercube Sampling tuning exercise. As a result an additional paragraph has been inserted into the discussion and conclusions section on page 30, lines 13-30 of the pdf supplement. This outlines the impact of not only basal sliding but also sediment deformation (not presently included in the version of Glimmer used here) on ice-sheet evolution.

I think there is still value in the findings as they are, but they must be thoroughly grounded in the simplifications. I think the best way to address this is to restructure the discussion and conclusions. It felt like it ended a bit sharply, the last thing should be what you found, so someone can look to the last paragraph to get the main findings (i.e. the whole first part of the discussion up to p261, line 10, maybe reworded more briefly as a summary). But, these need to be seen in light of the discussion of the sources of uncertainty, which you present well, but you need to more strongly emphasise the problems of basal sliding and resolving the fast flow features. This part is the discussion and needs to come before the conclusion. The final conclusion paragraph should be a strongly worded message that future predictions for Greenland are highly sensitive to a number of factors, but that this includes the physical basis of the model, and that we really don't have this tied down very well at yet. You don't draw any direct conclusions about the future of the Greenland Ice Sheet from your modelling, and I think this is important not to do that. The conclusion of your work should relate to the model and not to the Greenland Ice Sheet. You have shown that the model is very sensitive to parameters, and hence any conclusions drawn from it about the future of the ice sheet itself should be seen in the light of this sensitivity and the model deficiencies.

Otherwise it would be easy for someone to take Fig. 12f, in particular, out of context, to show that Greenland may not exist in a 400ppmv world.

Response: We have restructured the discussions and conclusions with the discussion coming first followed by a summary of the main findings of the paper. The final paragraph has been expanded along the lines suggested (page 34, lines 13-24 of the pdf supplement) and we feel this now adequately addresses the model deficiencies and sensitivities to different combinations of parameters.

Another structural issue - in sections 3.2 & 4, talk about the datasets in the same order in both sections, it just helps to follow it.

Response: This has been restructured.

Technical Comments:

Title:
I don't think the title is worded very well, I particularly don't like the phrase 'more realistic forcings' very much, you don't know conclusively that they are more realistic over the entire ice sheet. You also don't mention parameters when this is a large part of what you investigate. You should probably mention that you run future scenarios as well as investigate the present day. This is hard to summarise briefly, but I think it could be done better.

Response: We agree the title is not worded particularly well and does not mention the numerical model simulations or the future scenarios, which forms a large part of the paper. We have, therefore, changed the title to 'Investigating the sensitivity of numerical model simulations of the modern and future response of the Greenland ice-sheet to climate change'.

Abstract:
p234, line 8: remove 'in terms'.

Done

Introduction:
p236, line 3: Split these references to put them after the dataset they refer to, e.g. ...topography and ice thickness (Bamber et al., 2001) as well as...

Done

Model description:
p237, line 12: change 'ice-sheet density' to 'ice density'.

Done

P237, line 21: change 'used to change the speed of ice flow' to 'used to change the flow law parameter, and, hence, change the ice flow velocity.'. Then start a new sentence: 'The flow enhancement factor accounts for...'

Done

p238, eq.4: use a letter for melt, rather than spelling it out.

Done

p238, line 11: singularise albedos & densities: i.e. albedo & density.

Done

p239, line 19: add the words 'surface' before topography to clarify you are talking about the ice sheet surface topography rather than bed topography.

Done

p239, line 23: remove definition of sG, you have defined this already, and you have also given it the wrong definition.

Done

Datasets:
p240, line 5: change 'section' to 'part'.

Done

p240, line 5: remove the word 'realistically'.

Done

240, lines 8-10: change this sentence to a list to make it easier to understand, i.e. '...changes under (a) steady state present climate conditions, (b) ...'

Done

p240, line 15 & lots of other occasions: remove hyphen between 20 and km.

Done and corrected on all occasions.

p240, line 21: should be 'Hsurf'.

Done

p240, line 22: Ta is already defined.

Done

p241, line 7: do you mean surface topography here? Or do you mean bed topography? Check this sentence.

Response: We mean bed topography.

p242, line 11: You state 'it has been shown to be reasonable for Greenland' is this based on one of the references? This sentence is a bit unqualified on its own.

Response: This is based on the Serreze et al. (2005) reference.

Sensitivity:
p244, line 23: Add something like 'this can be explained by' before the explanation of why the extent changes but not the volume.

Done

p246, lines 8-10: This is a bit awkward, clarify what you mean by the bedrock topography determining the ice thickness.

Response: We have expanded this to:
'The quality of the bedrock topography is important in ice-sheet models since it largely determines the ice thickness at regional scales. This is because topography influences where the build up of snow and ice can occur and therefore is a major control on the threshold of ice-sheet initiation. Furthermore, topography influences the convergence and divergence of ice flow such that flow into lowland basins and valleys from surrounding higher relief regions will result in faster build up of ice compared with flow from an isolated upland region into a lower basin (Payne and Sugden, 1990).' See page 11, lines 14-22 of the pdf supplement.

p246, line 13: change to 'compared to the Letreguilly datasets, with the ice thickness...' & other occasions, refer to 'the Letreguilly dataset' rather the just Letreguilly.

Done and changed for all occasions.

p246, line 26: again be careful how you refer to the datasets, change 'Bamber grid' to the 'Bamber datasets'.

Done

p246, line 27: Is Table 2 needed? It just repeats what can easily be seen from Figure 1? You can talk about the % change figures in the text without needing the table.

Response: We think that the summary table is useful at this stage to make it easier to follow the text that follows, and so have kept this table.

p247, line 16: This sentence sounds a little harsh on Glimmer!

Response: We have changed 'poor' to 'poorer'.

p249, line 15: See the note above, I don't think you can dismiss basal sliding quite so easily as you do.

Response: This has been addressed in the specific comments above. Also see page 16, lines 22-23 of the pdf supplement.

p249, line 23: commas around 'therefore' & a few other occasions.

Done

p250, line 12 & numerous other occasions: add a space between W and m-2. It may be neater to change the units to mW m-2?

: The units have been changed to mW m-2 with a space inserted for all occasions.

p251, you mix using 'PDD factor for ice' and alphaice in this paragraph, use one or the other to make it easier to follow.

Response: We have changed all to PDD factors for ice.

p251, line 13: add a space between 3 and mm.

Done

p252, line 20: is the optimum parameter really the same for the ice volume as or the max ice thickness? Unless I am mis-interpreting the graph, it seems to be that you haven't reached the optimum flow parameter for ice volume (at 0% change)?

Response: This is a mistake and has been corrected to:
'In contrast, the optimum enhancement factor is not reached for ice volume within the limits of the range (1 to 5) investigated.'

p252, line 28: comma after 'In contrast'.

Done

P253, second paragraph: This paragraph needs tidying a bit - you talk about how the geothermal heat flux could affect the ice sheet through basal sliding, but then say well, actually it doesn't matter anyway because you don't have any sliding anyway.

Response: We have restructured this paragraph beginning with a description of how geothermal heat flux could affect basal sliding if this was switched on. We then discuss how it might affect the ice-sheet from basal melt alone. See page 20, lines 5-28 of the pdf supplement.

p253, line 18: 'forcing was so cold resulting in low ice temperatures...' is awkwardly worded.

Response: This has been corrected to:
'A similar result was found by Hebeler et al. (2008) for the Fennoscandian ice-sheet where very cold mean annual atmospheric temperatures resulted in very low ice temperatures. As a consequence, the influence of geothermal heat flux on the thermal regime of the ice-sheet was minimal.'

p254, first paragraph up to spatially varied: is this paragraph necessary? It seems like a bit of repetition.

Response: We agree that this is repetition from the model description section so has been largely removed.

p254, line 10 & around, also figure 7: Be careful with talking about lapse rates here. You say a 'ice surface extent increases with an increase in lapse rate'. You then go onto quote negative lapse rates when you have positive lapse rates on the figure. Technically your lapse rates should be negative, and then is it not a decrease in lapse rates?! Maybe you should talk about more or less negative values?

Response: We agree with this. All values are negative so we have switched the following sentences:

'Relationships derived from ERA-40 reanalysis data also yield summer lapse rates as **low** as -4.3°C km-1 at the margins and an annual lapse rate of -8.2°C km-1 for the bulk of the GrIS.'

is changed to:

'Relationships derived from ERA-40 reanalysis data also yield **less negative** summer lapse rates of -4.3°C km-1 at the margins and a **more negative** annual lapse rate of -8.2°C km-1 for the bulk of the GrIS.' See page 18, lines 24-27 of the pdf supplement.

'Equilibrium ice surface extent increases with an **increase** in lapse rate (Fig. 7). A similar relationship holds for ice volume but is less pronounced. This is because a **smaller** lapse rate results in relatively warmer near-surface air temperatures at high altitude, thereby expanding the area available for ablation. The **lowest** lapse rates results in the least error but are not typical of the annual lapse rate of -6.5 to -8°C km-1 used in several studies'

is changed to:

'Equilibrium ice surface extent increases with an increase in **negative** lapse rate (Fig. 7). A similar relationship holds for ice volume but is less pronounced. This is because a **less negative** lapse rate results in relatively warmer near-surface air temperatures at high altitude, thereby expanding the area available for ablation. The **least negative** lapse rates result in the least error but are not typical of the annual lapse rate of -6.5 to -8°C km-1 used in several studies.' See page 21, lines 20-24 of the pdf supplement.

p254, line 13: remove 's' from 'results'.

Done

p255, line 18 & other occasions: I'm not sure I like the word 'setups', perhaps instead 'parameter sets'?

Response: Setups has been replaced with parameter sets throughout the paper.

p255, line 18: comma after 'ice sheet'.

Done

p256, line 1: remove capitalisation of Error.

Response: This has now been abbreviated from normalised root mean square error to NRMSE.

p256, line 5: Add a linking sentence to explain that you will remove any that are similar, otherwise it is hard to follow where you are going with this.

Response: This section has now been removed and all six parameter sets selected from the ranking are used to examine the future melting experiments. This method has the advantage of being more robust and less subjective. See page 23 of the pdf supplement.

p257, line 1: Remove 'obviously'. If it's obvious you shouldn't have to write it!!

Done

p257, line 21: change 'ESIMINT' to EISMINT.

Done

p258, line 13: add km-1 to units for lapse rate, and again talk about in terms of more or less negative.

Response: We have changed the units and modified the sentence from:

'The main difference in parameter values between Fig. 12f and the other four experiments is the atmospheric lapse rate which is at least 2°C **larger** than any of the other lapse rates chosen. During ice-sheet retreat a **higher** lapse rate will act to warm the region further and cause more surface melt than a **lower** lapse rate via the ice-elevation feedback mechanism'

to:

'The main difference in parameter values between Fig. 12b and the other four experiments is the atmospheric lapse rate which is at least 2°C km-1 **more negative** than any of the other lapse rates chosen. During ice-sheet retreat a **more negative** lapse

rate will act to warm the region further and cause more surface melt than a **less negative** lapse rate via the ice-elevation feedback mechanism.' See page 26, lines 1-5 of the pdf supplement.

p260, line 8: reword part starting 'where the faster...'

Response: This has been reworded to:

'The maximum ice thickness and ice volume were shown to depend on the factors affecting ice flow. In this case increasing the flow enhancement factor makes the ice flow faster which lowers the height of the ice dome.'

p260, line 17: See general comments above - the high PDD factor is probably compensating for the lack of basal sliding. This needs to be emphasised more strongly.

Response: This has been addressed briefly in the response to specific comments above. We have stated in the discussion section the implications of this missing process. This is reiterated in the context of the statement above in the conclusions section (page 33, line 17-18 of the pdf supplement).

p261, line 23: change to 'we only initiate the ice sheet model from the present day...'

Done

p261, line 27: change to 'current net mass loss' - the ice sheet is losing mass all the time!

Done

p262, line 4: change 'on' to 'in'.

Done

Tables:
Table 1: Density should be kg m-3.

Done

:

Figure 1: Increase the font size, it is a little hard to read.

Font size increased, see Figure 1 at the end of this comment.

Figure 1: Caption, last sentence is not worded very well.

Response: This has been changed from:

Figure 1. Evolution of modelled ice-sheet (a) volume and (b) ice surface extent for different boundary conditions and forcings. The EISMINT-3 experiment is also shown for comparison, and observations derived from Bamber et al. (2001) and Letreguilly et al. (1991). Each boundary condition/forcing is changed one at a time.

to:

Figure 1. Evolution of the modelled ice-sheet a) volume and b) ice surface extent for each of the different boundary conditions and forcings changed one at a time relative to EISMINT-3, when they are all varied together and when they are linearly combined. The EISMINT-3 experiment and observations derived from Bamber et al. (2001) and Letreguilly et al. (1991) are also shown for comparison.

Figure 7: Lapse rate values should be negative.

Done

Figure 9: change to 'The small black dots represent...'

Done

Figure 12: where did the '2' come from in future warming scenarios?

This was an error and has been removed.

Response to comments by Anonymous Referee 2:

Are substantial conclusions reached?

Consistent and helpful conclusions are reached. However the fact that using "improved" datasets result in considerably "worse" simulations of today's GrIS strongly implies that for these datasets, some of the assumptions and abstractions applied in ISM (Glimmer and others) might not hold, for example the lack of higher order physics to represent fast flowing ice, the lack of basal melting or sliding, the limited resolution modelling the ablation process, etc. While the authors mention these points, I feel that the consequences might be more important and far-reaching, and should be explored more elaborately.

Response: This has partially been addressed in response to referee 1 above in terms of the missing fast flowing processes. We have commented on the abstractions applied to the ISM might not hold in the conclusions section, page 32, lines 24-29 of the pdf supplement.

Are the scientific methods and assumptions valid and clearly outlined?

All methods are clearly defined and well documented. Considering the length of the paper, I would be tempted to reduce the description of the Glimmer ISM as well as the EISMINT experiment setups even more, as they have been subject of a number of papers which are readily available to the reader.

Response: We feel that it is important to keep the model description in the paper since all aspects mentioned relate directly to the parameters changed in later sections. Furthermore, the paper (Rutt et al., 2009) outlining the Glimmer model description is not open-access and therefore not readily available to all readers. This is also the case for the papers describing the datasets used in EISMINT-3.

Are the results sufficient to support the interpretations and conclusions?

Results are in general sufficient to support the stated interpretations and conclusions, in fact I find the conclusions to be a bit too brief and superficial. While the approach using LHS to determine the best-fit parameter set is valid and innovative, the results *suggest that ISM such as Glimmer, that use a number of abstractions and simplifications and run on relatively large scales, might not be a valid choice for the modelling ice sheets at the given spatial and temporal resolutions. While the shortcomings of these models are acknowledged, alternatives are not being discussed, and it is only suggested that with increasing computational capacities models using higher order physics, which can be run at higher resolutions and capture fast flowing ice, will solve the problem. A number of alternatives, such as finite element modelling, subgrid modelling for calculation of ablation, methods of calculating mass balance other than using PDD etc are not being discussed. The fact that assumptions used in current models result in pronounced deviations with observation in the field might also hold consequences for scenarios and forecasts made in the past using these models. Additionally, it could be concluded that the results presented in this paper show that topography and it's inherit uncertainty appear to have a significant effect even on large scale models of considerable abstraction. It should be discussed whether the use of more "realistic" topography and climate data is sensible, where the degree of abstraction (in terms of ice dynamics and topographic resolution) of a model is rather high.*

Response: We agree with the referee that the shortcomings of current ISMs, discussed in Section 7 of the paper, does not elaborate fully in terms of alternative methods used in these models. As such a discussion has been included on the advantage of using the finite element numerical method compared with finite differences to solve the governing ice-sheet equations (see page 32, lines 1-8 of the pdf supplement). The application of subgrid parameterisation of ablation/accumulation has also been described (see page 32, lines 10-14 of the pdf supplement). We feel, however, that the energy-balance/snow pack model as an alternative to the PDD approach for calculation of surface mass balance has been adequately addressed previously (page 31, see lines 18-33, of the pdf supplement).

We also agree that these results could suggest that it is perhaps not suitable to use high resolution and sophisticated datasets in conjunction with large scale models of considerable abstraction. A discussion on this has been included (see response above).

Does the title clearly reflect the contents of the paper?

I would personally change the title to reflect the topic and results of the paper a bit more concisely.

Response: We agree and this has been addressed in response to Referee 1 who also highlighted this.

Is the overall presentation well structured and clear?

The paper is well written and structured, but the overall length is relatively large, which makes it difficult to follow in parts. I would prefer it to be more to the point. An overview of the conducted experiments, for example in the form of a table, would probably make it easier for the reader to anticipate the content. This might also help to cut down the overall size of the paper a little. I would suggest revising some of the more elaborately phrased sections to be more concise.

Response: A table has been inserted outlining the experiments performed in Section 4 for clarification (see page 47 of the pdf supplement).

The conclusion and discussion section is a relatively short and repetitive, because results from the different experiments are subsumed. Consequently, the actual yield of the discussion is below my expectations.

Response: The discussion and conclusions section has been restructured and in particular the discussion section has been expanded in accordance with specific comments made by both referees.

A rather long paragraph of section 3 (datasets) is devoted to explaining why the newer parameterisation of climate date by Fausto et al 2009 has not been used. I would move this explanation to the discussion.

Response: This section has been moved to the discussion.

Is the language fluent and precise?

In some places, I would prefer the wording to be more concise.

Response: We have tried to address this wherever possible without losing precision.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

As mentioned above, I think description of model physics and EISMINT setups could be shortened.

Response: This has been addressed in response to the referees' previous comment.

Consider revising the captions of figures 8-10 to make them even clearer to the reader.

Done

Please make sure that all maps in Fig 12 are using the same projection and are scaled alike. (Fig 12a appears "squeezed" compare to Fig 12b-f)

Response: All subfigures in Figure 12 are now on the same projection. See Figure 2 at the end of this comment

Additional questions:
Are you sure that the variables (e.g. parameters) used within LHS are independent of each other? Values are chosen independently, but in reality PDDs for ice and snow and lapse rate might not really be independent of each other...

Response: We agree that this is an important question to address and the following has been inserted into the discussion and conclusions section:

The parameters varied using LHS are strictly independent in a mathematical sense. However, it is possible that the values chosen could have similar and opposite effects

on accurately predicting the present day GrIS geometry. For example, high PDD factors in combination with low lapse rates could simulate a good representation of the GrIS. In our conclusions we do not attempt to make a probabilistic interpretation of the results such that certain combinations are more likely than others in producing an accurate ice-sheet(see page 33, lines 23-29 of the pdf supplement).

Response to Short Comment posted by Felix Hebeler:

In the insets of fig 8 you show the "optimal" parameter sets to represent modern day GrIS. Since model runs are ranked, it is a bit unclear to me how for example run no 250 with the optimal parameter set for ice surface can be compared to run no 250 of ice thickness, since these are not the same experiments, ergo parameter combinations. It would be interesting to see for example, if run no 87 was the best parameter combination to reproduce ice surface, where on the ranking this run was for ice thickness, volume, etc.

Response: This is in fact addressed in Figure 10a where each selected experiment is compared with all experiments for each diagnostic. To make this clearer we have also inserted a percentage of how well each experiment performs for each diagnostic compared with the others. For example, 100% means that this experiment performs the best out of all experiments while 0% indicates the worst performing experiment for a particular diagnostic. See Figure 3 at the end of this comment.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/4/C645/2010/tcd-4-C645-2010-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 4, 233, 2010.

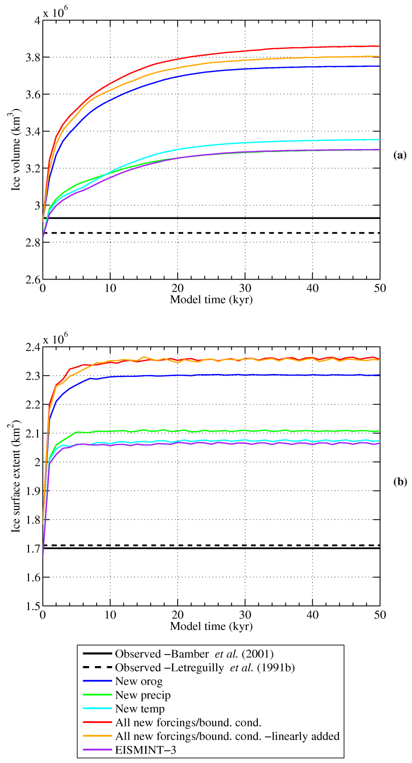

Fig. 1. Modified font size in response to technical comment made by Referee 1. This Figure corresponds to Fig. 1 in the original paper.

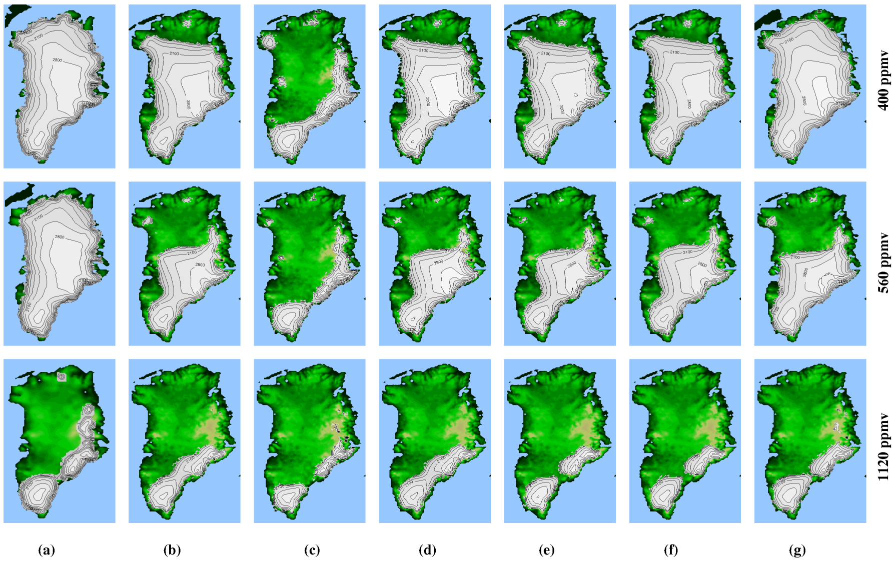

Fig. 2. Projection modified in response to comment made by Referee 2. This Figure corresponds to Fig. 12 in the original paper.

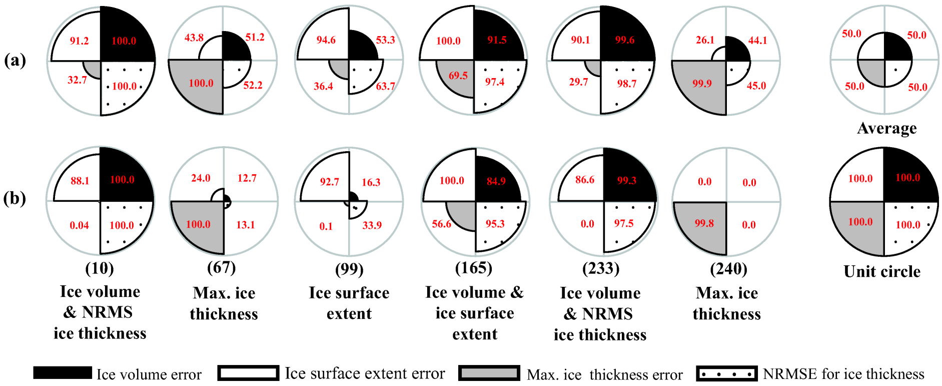

Fig. 3. Modified in response to the short comment posted by Felix Hebeler. This Figure corresponds to Fig. 10 in the original paper.