Review of manuscript submitted to Geoscientific Model Development: "A technique for generating consistent ice sheet initial conditions for coupled ice-sheet/climate models" by J. G. Fyke, W. J. Sacks and W. H. Lipscomb

1 General comments

This paper reports on a technique for generating initial conditions for ice sheet model that would be directly applicable in coupled ice sheet/climate model simulations. Creating an initial state for the ice sheet model, that is in equilibrium or consistent with the applied climate model, is a challenge for everyone who is running coupled models and it is a similar task as when ocean models were coupled with climate models and flux corrections had to be applied because the climate models were not providing boundary conditions to create a applicable initial state. Authors want to avoid using flux corrections or other models (PDD or cheaper climate parameterizations) to force the ice sheet model into an initial state and therefore use the climate model to create three climate states, from which 30 years mean SMB matrices for Greenland are extracted. Then a continuous forcing time series is calculated from these three matrices using weighting based on the NGRIP ice core δO^{18} record.
This method of creating a composite forcing record is deemed necessary for the type of coupled simulations they want to do, which aims to have an initialised ice sheet model that is in thermodynamic consistency with both the simulated preindustrial 1850 climate, and pre-1850 climate evolution. Because of high computational cost the climate model cannot be run for the 120 ka, a full glacier cycle that the initialisation of the ice sheet model takes, forcing is therefore created from three climate states. The authors claim that the long (120 ka) climate history will effect the ice sheet conditions and response, but do not convincingly demonstrate that the two model states they create (transient and equilibrium spin up) actually do respond differently. A comparison of the responses of the two model states would be valuable for this paper to show how important this work is.
This study is interesting and as long as the climate models are not providing the necessary forcing to spin up ice sheet models some kind of solution, like the one suggested, needs to be used. However, this study does not take good care in describing the models used in the study, validate the resulting initial state or show in any way that the suggested method is providing applicable solution. Much data available for validation are ignored and it is not convincingly demonstrated that the achieved state of modelled ice sheet can be used as initial condition for realistic coupled simulation starting in 1850 (why 1850?). This study would benefit greatly from a thorough validation of the created initial model state, with comparison to observations, not only the comparison with the simulation that is forced with constant climate. Only one borehole temperature record is used in this study, but there are at least 4-5 additional

boreholes in Greenland that have temperature records (Camp Century, Dye-3, GRIP, GISP, Neem) that could be used to validate the results. Also, a study about elevation changes during Holocene *Vinther et al.* (2009) provides excellent validation data for the type of simulations done in this study. The paper as it is does not convincingly demonstrate the value of the suggested method. The paper text is not clear and can be improved in many places (see a few suggestions below) and care should be taken to be consistent in description of concepts and ideas. In many places it is confusing to read as new words are used for same things, a few places are pointed out below. There is serious lack of model description, which makes the discussion part difficult to read and also it is not clear from the text in section 2 that both SMB and Temperature fields are created as time series, but these are discussed in later sections. Figures can be improved, the color scale used in Figs. 5 and 6 could be improved to clearly show the zero values and there is a missing (or not clearly visible) minus in most of the axis.

2 Specific comments

page 2493 lines 23-25 Is there a reference for this statement, can they show how large impact the thermal memory of the ice sheet has on the present day and future dynamics? this is the reason for undertaking this study and more solid argumentation would be useful here.

Page 2494 In a number of places "reasonable" is used to describe quality of initial state (line 3, reasonable temperature field, line 9 reasonable approximation), but what do you mean by reasonable? well validated?

Page 2494 line 14-15, what do you mean by "instill ice sheet components"

line 18 - why would you want to apply simple calibrations to any in line SMB model? explain better.

Pages 2494-2495 The list is introduced as various shortcomings of existing attempts to circumvent basic problems, but the wording of the listed items does not indicate shortcomings, but possible ways of making initial states. My suggestion would be to rewrite this list to clearly describe the shortcomings, or introduce the list differently. Some of the items in the list are already discussed above.

Page 2495 line 21 what do you mean by consistent here?

page 2495 lines 22-26 suggest to rewrite these lines to reflect the content of section 4, no ice sheet model of climate model developments are described but a number of different attempts of creating inital states are discussed.

Section 2 is not clear and would benefit from thorough editing. It is not clear what is "climatology" and what fields the ice sheet model is forced with, is it

only SMB, or also Temperature fields? The temperature is shown in Figure 1, but no discussion of Temperature field in this section.

What is "equilibrium 30-yr SMB climatology matrices" ? explain. Why 30 years?

it is not clear why the term "end members" are used, end of what?

page 2496 line 9, what does "fully coupled equilibrium climate states" mean in this context? does it include fully coupled ice sheet model?

line 14, what does "IG" mean?

line 14, suggest to move the reference (Oleson et al., 2010) to line 10

what does "multiple elevation classes" mean? (line 15-16) in line 22 "sub-grid "virtual" elevation classes" are introduced and on page 2499 line 2 it is stated that "the number of elevation columns was increased to 36" - do all these refer to the same? also, in line 17 it is stated that SMB are "then downscaled", what does that mean?

lines 19-21 this sentence is not clear. What is the difference between "30-yr SMB climatologies" and "Simple mean SMB Climatology"?

What is "non-zero effects on SMB"? explain better.

line 25-26 what does "physically realistic SMB" mean here, are they well validated agains observations? or does it only mean they are in a reasonable time series for forcing the ice sheet?

line 27 and onward - this sentence is awkward, what is in contact with SMB values? and what does that mean? that there is an SMB value for each elevation, whatever the evolution of the ice sheet is?

Page 2497 line 7 What does "proper imprint" mean here?

lines 8-10 what is "basic lack"? is it not available, or you have not computed it?

line 13 What do you mean by "largely swept from the system"?

lines 20 and on, this description is not clear and needs editing. "avoid aliasing of end member NGRIP values" as far as it is possible to understand from the text there are only three states that are used and the scaling is performed by using weighted values from each of these three SMB (and T?) states, based on what the time is and the NGRIP δO^{18} value, is this correct?

What does "thresholded slightly" mean?

why is 600 years average selected? does this period have importance or selected randomly? why? what impact has the lenght of the constant climate period have on the results?

Page 2498 line 7 "A looped, 30 yr climatology" what does this mean, are 30 yr mean SMB and T fields created for each 600 years interval?

is $\delta O^{18}_{EM_{+1}}$ same as $\delta O^{18}_{\overline{EM}}$? - the +1 index on EM is not explained.

is CC increasing from -122 ka to 1850?

The description of the method of creating the time series is not clear, it needs some editing.

lines 18-21 Can you show that the consistent ice sheet model state is realistic as well?

the models used in this study are not explained well, neither the ice sheet model physics or the surface mass balance, it is therefore hard to follow the text, suggest to explain better the used models and thereby make the lines 22- 5 on page 2499 much clearer.

Page 2499 line 14 "physically reasonable" what does this mean?

line 17 it would be useful to mark the locations on either figure 5 or 6

line 24 The thresholding appears to affect the results, but it is not discussed how the results can be validated, here some more discussion of validation would improve the paper.

Page 2500 line 4 LWE is not explained, assume it means liquid water equivalent, it is common to write that: m w.eq. yr-1

Only two points for the whole ice sheet are shown and discussed, but then a general statement of reduction in ablation during glacial period overwhelms any relatively small decrease in accumulation. How is this validated? can you show a time series of the total SMB for the whole ice sheet to support this statement?

line 20 How is the geometry modified?

line 23 here, for the first time, it is stated that Temperature field is used to force the model, it would be useful to have this information earlier in the paper.

line 27 "Climate begins to drop into glacial" suggest to edit this to make it clearer, what do you mean here?

line 28 "periodic pulses" are not clearly visible, what are you referring to?

Page 2501 line 5 "significantly warmer deglacial and Holocene ice" do you mean interglacial? how does that make sense?

line 10 here you could refer to figure 3 b

line 23 "the increase in marginal thickening" suggest to change to "increase in marginal thickness"

line 24 you have just discussed decrease in glacial period, so the start of this sentence is strange.

line 26 Do you mean at the end of the simulation, at year 1850. it would be useful to show a comparison of the measured and modelled ice sheet, for assessment of the success of this method.

how much larger is the modelled ice sheet?

Page 2502 line 7 something is missing, do you mean "spin up ice sheet model state"? the sentence is not clear as it is. same in line 14

line 16, this sentence is not clear, do you mean "transient spin-up procedure"? What is meant by "despite being driven solely by climate model output" - do you have other options?

line 20 "too high prescribed geothermal heat flux" - what is the geothermal heat flux used? this is not at all discussed in the model description (which is poor as discussed above) "too high" compared to what? are there observations for validation here?

line 21 "slight spatial biases within the ice sheet model" what is meant here? not at all clear from the context what is discussed here

I cannot find in the paper any statement of what the horizontal resolution of the ice sheet simulations are, so it is hard to assess the results, again, a better model description would be valuable for this paper.

Page 2503 line 8, this sentence is not clear, what is "equilibrium spin up case" do you mean the ice sheet model state forced with constant climate forcing?

line 13 and onward, this text does not make any sense. What do you mean by "recent mass gain"? observations show that the largest mass loss is at the margins see for example *Sørensen et al.* (2011).

What about comparing to observations?

line 17 what do you mean by "more recent Holocene climate trends"?

line 21 here would some validation of the method be appropriate, not only that it is consistent.

line 23 here is a statement of surface temperature matrix to force the ice sheet model, but it is not explained in Section 2, this should be clarified earlier in the manuscript.

Page 2504 line 1 "energy-balanced SMB" is not a clear term, do you mean SMB computed with surface energy balance model?

line 4 what is meant by "accurate spin-up of an ice sheet model"? well validated?

line 6 is should be "in"

line 7 do you mean (i.e. SMB)?

line 11-14 missing references here, what do you mean by this procedure?

line 16 missing references, for example to *Price et al.* (2011) and *Gillet-Chaulet et al.* (2012)

line 19, what do you mean by "coupled models are in no way constrained by observations"? because you chose not to validate the coupled model with available observations?

line 21 "tradeoff for full system consistency" - do you mean that it is not possible to run a fully coupled model system that is well validated with observations? I would disagree with that.

line 23 What do you mean by "very small biases"? compared to what? and "inconsistent"? what does it refer to?

Page 2505 line 2 what "surface conditions"? please specify, elevation, surface velocity or other?

line 23 first here is some description of the ice sheet model that is used, it would be appropriate to describe the models, ice sheet and climate model (including how the SMB is computed) earlier in the text.

Page 2506 lines 1-2 What does this sentence mean? response of the model to what? "qualitatively similar" to what? How can you validate this statement?

lines 11-15 This sentence does not make sense here. "accurate migration of the summit elevation though time" has not been shown in this study, what do you mean? what does "reasonable" mean in this context?

Page 2507 line 2 "energy-balance-derived SMB fields" this needs better explanation and as discussed above there is lack of model description earlier in the paper. Also, here is missing discussion of the temperature field that apparantly also is used to force the ice sheet model.

line 10 "summit migration" has not been shown in the paper

line 15 "improved in places by up to 500 m" what do you mean? there is no presentation of comparison with observed ice sheet elevations, how can this statement be validated?

lines 17-19 this sentence is unclear, what does "They" refer to? what is "realistic spun-up ice sheet" in this context?

Figure 1 Missing minus on vertical axes. The figure caption is not clear, the NGRIP core is not thresholded, suggest to rewrite to something like "flat sections show the period when the values are forced to the threshold values, as described in section 2." The location of the selected margin/summit points would be shown in figure 5 or 6.

Figure 2 minus sign in vertical axis is not visible. suggest to rewrite the figure caption (it is not clear as it is) to something like: Note the two vertical axes have different range in order to highlight the anti-correlation between the two time series. It is not clear why there is cutoff in the

margin SMB value at zero, it seems strange that the margin point cannot have positive value during glacial times.

Figure 3 Colour scale could be improved to clearer show that variability. minus sign is not visible in figure b)

Figure 4 the minus sign is not visible in figure b) Missing reference to the NGRIP temperature profile in figure caption.

Figure 5 : Label on the colour scale should be temperature difference. The colour scale could be changed to clearly identify zero, for example positive values could be red and negative blue and zero white between, the minus on negative values is not visible. The zero contour is not dashed as stated in the figure catpion.

Figure 6 : The colour scale could be changed to clearly identify zero, for example positive values could be red and negative blue and zero white between, the minus on the negative values is not visible. The zero contour is not dashed as stated in the figure catpion.

References

Gillet-Chaulet, F., O. Gagliardini, H. Seddik, M. Nodet, G. Durand, C. Ritz, T. Zwinger, R. Greve, and D. G. Vaughan (2012), Greenland ice sheet contribution to sea-level rise from a new-generation ice-sheet model, *The Cryosphere*, *6*(6), 1561–1576, doi:10.5194/tc-6-1561-2012.

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