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Interactive comment on "Exploring the impact of atmospheric forcing and basal boundary conditions on the simulation of the Antarctic ice sheet at the Last Glacial Maximum" *by* Javier Blasco et al.

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Blasco and colleagues examine the interesting and as of yet unresolved question of the maximum extent of the Antarctic Ice Sheet (AIS) during the Last Glacial Maximum (LGM). The existing literature on the matter exhibits a very large spread in potential LGM ice sheet configurations and volumes. Blasco et al. set out to systematically illuminate the sources of uncertainty regarding AIS LGM ice-volume and -extent reconstructions by means of paleo ice sheet modelling. This is a very valuable effort and well suited for the scope of The Cryosphere. Blasco et al. identify, and focus on, two

major sources of uncertainty when it comes to simulating the LGM state of the AIS.

1. Uncertainties in the climate forcing used to drive ice-sheet changes.

2. Uncertainties regarding the parameterisation of basal drag, which controls the efficacy of the ice drainage via outlet glaciers.

The manuscript is generally clearly structured and easy to follow. However, there are some issues which should be addressed to make this a valuable contribution to The Cryosphere.

Below I list my major concerns followed by some minor stilistic/editorial aspects.

1. The authors omit a discussion as to how their initial ice sheet configuration affects their results and conclusions:

1.1 How does the model spinup affect the final LGM extent of the AIS. Is there a thermal spinup, paleo-spinup or a "cold start". A more detailed discussion of the initial state of the ice sheet would be useful. I suggest one additional figure (this could be figure 1 or 2) which gives an overview over the initial state of the ice sheet and the present day (PD) tuning simulations (best fit, ice thickness change vs observations, ice volume and sea level equivalent, grounding line configuration, surface velocity). There are some figures in the supplement but I think an overview figure in the main manuscript is needed.

1.2 Arguably, the authors chose a relatively loose definition of a "good" present day fit with respect to sea level equivalent ice volume change (-3m to +3m). The ice volume spread at PD due to the parameterisation regime is about the same as the total LGM volume spread in their ensemble. How would the LGM spread change if more rigid conditions are applied for PD (e.g. pm 1m?). Also it seems that ice shelves are extensive in the PD tuning runs (supplementary figure 2). How does this affect grounding line advance (buttressing) as well as SMB (a very large area is gaining mass right away, whereas in reality there might have been no ice shelves).

2. The experimental setup assumes a steady state LGM-forcing for 80 ka. I understand that Blasco et al. chose an idealised setup in order to fully focus on the effects of different climatological boundary conditions and ice flow parameterisation. This is fine, however the fact should be discussed, so the reader can appreciate the potential impact on the results. In reality, full LGM-forcing was only sustained for maybe a couple of millennia, preceded by a long cooling period starting at the end of the last interglacial. The authors should include a discussion of the transient evolution of the AIS from the initial present day (PD) state to the final LGM state. How fast is equilibrium reached? Does it take several tens of thousands of years or only a couple of millennia? Is the relative homogeneity of the grounding line extent due to the long integration time under LGM conditions or the forcing? What role does the sea level boundary condition play? Actually, reading the text I was missing information whether sea level was set to LGM conditions (ca. -120 m) or PD or something in between? This is important information, as sea level alone exerts a big influence on the grounding line position via the flotation criterion. Here an additional figure would be nice which shows the transient growth of the AIS under constant LGM forcing for each ensemble member. This would elucidate the inter-ensemble differences in the pace of AIS grounding line advance and ice volume change.

3. Model resolution. This is a somewhat nasty argument as in theory very high spatial resolutions are required to adequately resolve grounding line migration. However coarse resolutions are a tried and tested instrument to allow for larger paleo ice sheet ensembles and the authors do use a sub-grid grounding line procedure to accommodate for the coarse resolution. Still, 32 km are on the rough end of currently used grid-spacing and it would be interesting the see the effect of say doubling resolution (16-km) on final LGM ice volume and extent. This does not have to be done for each and every run, but picking one single member and maybe the GCM-mean forcing would show the impact of resolution on LGM ice sheet configuration. This would mean only two additional simulations and should not take too much time.

General comments text:

The manuscript is generally well written but contains a couple of stilistic issues, redundancies, unclear sentences etc. of which I try to note a couple in the following:

Title :

I think the title is a little misleading, as you do not explicitly simulate the Last Glacial Maximum Antarctic Ice Sheet configuration per-se but rather potential equilibrium states of the AIS under LGM conditions. Below is an attempt at a slightly modified title.

Exploring the impact of atmospheric forcing and basal drag on Antarctic Ice Sheet equilibrium extent and volume under Last Glacial Maximum conditions.

Abstract: I think one interesting outcome of this study is that the ensemble spread with regard to sea level equivalent ice volume change is about the same for the tested parameterisations of basal drag as for the different GCM forcings used (both ca. 6 m). This should be mentioned in the abstract and discussion.

p3 l35: you assume a priori zero basal melt underneath ice shelves. For me this is fine, but how do you legitimize this choice? Relatively little is known about the state of CDW during the LGM, but I guess it is not to be excluded that regionally if the grounding line is located at sufficient depth, some basal melt is possible even during the LGM. Maybe a reference would be helpful here.

p4 l 31: The SMB is obtained from the difference between ice accumulation ...

p5 l10: how does this relaxation time relate to other figures used in the field? How does it affect the results?

p5 l 11 ... so an "enhancement factor" is used ...

p5 l27: This sentence is a little confusing, maybe change to : "For lower values of z_0, c_b falls more rapidly ..."

p6 l27: how realistic is this assumption? I guess in some regions basal freeze on could be extensive and for other regions basal melt is theoretically possible. A short discussion would be helpful.

p7 l 10-12: as mentioned in my comment 1.2, how does the large SL spread in the PD simulations affect the spread at LGM. What happens if you only account for those simulations with a spread of e.g. pm 1 m.

p7 l22: suggest to change to: Here we present the simulated AIS equilibrium configuration under LGM conditions for different basal friction parameters.

p7 l22: change to: Ice volume change is converted into ...

p7 l28 : change to : ...basal friction reduces basal sliding...

p7 l29 : change to : ...also reduces ice volume...

p7 l30 : suggest to change to: We do not identify a strong impact of marine basal friction on equilibrium grounded ice area, as the final grounding line configuration is similar in all ensemble members (Fig. 2b). Comment: is this mainly due to the long integration time? How quickly is the final ice extent reached?

p8 l11 ...a slowly decreasing basal friction

p8 l26: ...a spread of 6.2 msle.

p8 l32: use other word than "appreciable", maybe "strong" ?

p9 l6 ...identify the surface temperature ...

p9 l7 maybe change to: Whereas low surface temperatures lead to similar ice extend, relatively warm surface temperature forcing results in smaller equilibrium grounding line advance.

p9 l8 change to: given the overall low surface temperature at LGM, ablation can generally be discarded as the ...

p9 l16 it is unclear here what these hypotheses are. I assume you mean:

1. The more slippery the bed the farther the ice extend and the lower the volume. 2. the colder the surface temperature the larger the ice volume 3. the higher the precip the larger the ice volume

For the reader it would be nice if the authors main hypotheses are spelled out in the beginning.

p9 l20 what is abnormal? change phrasing.

p9 l20 suggest to omit "unexpected"

p9 l22 ... the regions with grounding line advance to the continental shelf break (e.g. the Ross Basin)... Comment: how do they contribute to low ice temperature? Due to lapse rate effects? Clarify.

p9 l27 too low for what? Suggest to change to: If the viscosity is low ...

p9 l28 necessary for what?? grounding line advance?

I suggest to rephrase the whole last paragraph, beginning at "In summary". You mix climate effects on the ice sheets rheology with topographic effects due to bedrock configurations and the location of the continental shelf break under the header of "Impact of climate forcing".

The last sentence provides an important finding as it shows the impact of different SMB regimes under similar ice sheet configurations.

p9 l33. I think at the current state of art in the field it is unclear what approach is "valid" given the large persisting uncertainties in paleo ice sheet modelling (as well as ice sheet projections). I therefore suggest to rephrase to: ... is a common approach...

p10 l3. As of yet it is unclear what a "realistic" SLE is, this is something you rightly state at the beginning of the manuscript. Therefore I suggest to rephrase to : All simulations produce SLE ice volume in the range of previously suggested figures and ice extend similar to reconstructions (e.g. Bentley et al. 2014) if using the same coefficients for basal friction and different climate forcings. Overall, consistently ...

p10 l4 change to: This is solely due to the difference in forcing, as the parameterisation of ice flow is identical.

p10 l5 change to: Since surface temperatures are not sufficient to cause surface melt, differences in ice volume and extent are exclusively due to differneces in accumulation anomalies.

p10 l7. It is evident that the main source of ice volume differences is due to changes in the WAIS configuration.

p10 suggest to change header of 4.1 to: "Role of basal friction" or similar

p10 l21 : between the end members

p10 l25 I know what you mean with "still agree with PD observations" but I suggest to to rephrase the sentence or split it in two.

p10 l26 change to: The choice of the friction law ...

p10 l30 suggest to change wording to: ...is especially relevant...

p11 l7 The simulated grounding line advance is strongly influenced by air temperature.

p11 l12 if temperatures are sufficiently cold (< 20 °C) ice full advances ...

p11 l13 The RAISED consortium shows a similar grounding line extend, albeit with two large ice shelves ... Comment: to my knowledge Bentley et al. show grounding line extend but not the presence of ice shelves but I might be mistaken? Please clarify.

p11 l29 Overall, homogenous climate anomaly-forcing relative to present day leads to a ...

p11 l32 Thus, recent paleo ice sheet model exercises utilise climate forcing derived

from GCMs

p11 l1 Nevertheless, ... resulting from different assumptions of basal drag.

p12 l4 change to: By design the modelled ice sheet could be expected to be driven towards the configuration used as a boundary condition in PMIP3. However, ...

p12 l6 ...the comparison with proxy-observations.

p12 l8 ... more accurate paleo-climate forcing will hopefully be available.

p12 l21 Imposing the PMIP3 fields, which explicitly assume an LGM ice sheet configuration, leads to higher preci...

p12 l24 I guess you mean WAIS not AIS here?

You show in your results that the uncertainties regarding basal conditions are as high as the uncertainties regarding climate forcing, this should be restated in the conclusions as I think this is an important finding of this work.

General comments figures:

Figure 3. Cmap different to read. Suggest to use simpler colormap (e.g. Red-Blue) and plot ice thickness changes relative to PD. For the surface contours I suggest using one color (e.g. gray).

Figure 4. The figure size seems overly large given that it shows less information than the following figures.

Figure 5. Tough too discern features with this color scale, I suggest something simpler (e.g. Red-Blue or similar) and plotting delta thickness with respect to present day observations instead of LGM surface elevation. This way it is easier to identify regional changes caused by the different GCM-forcings.

Figure 7. With the "jet/rainbow" color scale it is tough to discern between different ensemble members, I suggest different marker styles ("x o , ." etc) for each GCM in

addition to the colors.

Figure 8. same as Figure 5. Suggest different color scale and delta thickness instead of surface elevation. You can keep the surface contours for reference. Why is ice thickness lower in the coastal regions of the Bellinghausen Seas for the PMIP3av even though accumulation is higher? It can't be basal shelf melt as this is set to zero?

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