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## ***Interactive comment on “Modeling the response of Lambert Glacier–Amery Ice Shelf system, East Antarctic, to uncertain climate forcing over the 21st and 22nd centuries” by Y. Gong et al.***

**Y. Gong et al.**

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We thank Anonymous Referee 2 for their thorough review of our original manuscript. The referee suggested a number of modifications to the manuscript, and we have followed the advice given in the main.

"(i) the grounding line is generally slightly retreating, however it is not stated which amount of melt is prescribed for new ungrounded regions. This must be stated and it should be shown that such uncertainty does not impact significantly the results"

We have discussed this issue in som emore detail and added an experiment where higher melt-rates are prescribed at the grounding line, but which does not result in

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additional retreat.

"(ii) clarity in the discussion could be improved. Particularly, the figures are in general of poor quality, and do not help the reader to follow easily the discussion. Some suggestions to improve them are proposed below."

We have revised the entire text and all of the figures

"Surface mass balance (SMB) scenario does not have a significant impact on change in ice stored on the continent for the next 200 years. It is a striking result! And according to table 1, the various SMB scenarios are very different, by a factor of almost 3. This deserves to be fairly discussed as a common assertion is that coming change in SMB will balance coastal outlet glacier changes. Why is it not the case here? This should be stated. I guess the main explanation is that the study is focussing on a small and coastal region. But could this be generalized to other outlet glaciers? Does this indicate that knowing accurately the evolution of SMB changes in coastal regions is not important in the future? . . . In opposition to inland region where the issue is the evolution of SMB? I really think that the authors have material to discuss these points."

The SMB scenarios are very different, and they lead to very different results - or at least variation in sea level change that can be understood almost entirely in terms of the size of SMB anomaly. The region is rather large (1.3 million square kilometers), but receives little extra snowfall (no more than a few cm extra per year on average), which is enough to balance or even outweigh the fairly subtle dynamical thinning. So we think that future SMB projections are important for this region, although SMB might well be less important if one just considers the small fast flowing parts.

The original manuscript gave SMB anomaly figures over the whole of the model domain, but reported VAF changes just for the Amery Ice Shelf drainage basin (the region outside is just to provide a no-flow boundary condition at the ice divide, in effect, and is quiescent). We have revised the tables to give figures for the SMB anomaly over the drainage basin.

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"Change in the ice shelf melt rate only significantly impacts the main grounding line position and related dynamic thinning if it increases north to Clemence massif. Authors confess that more study should be done on the subject, which I agree. However, it already has some major implications that are not discussed. This confirms previous work by Gagliardini et al. (2010) made with flow line model. But here it is shown that grounded region within the ice shelf have tremendous impact. To my knowledge this is not widely and clearly stated in the literature. It also has an impact on the future modeling and survey of ocean / ice sheet interactions in general (and in the region in particular). They are regions more important to investigate than others in order to establish reliable projections of ice sheet behavior, and in the present case it is relatively well defined : North to Clemence massif."

We have expanded the discussion of the sensitivity experiments and added relevant figures. We tend to see Clemence Massif as one end of a narrow channel that starts at the Lambert Glacier grounding line - presumably there would be even more buttressing if it were continuous with the grounded ice to the south. We do agree that our experiments suggest that channel should be the focus of further investigation in the region especially with regard to ocean modelling.

Minor comments.

"p. 5685, lines 17 and following. A description of the geography is given. A map may help. It exists as figure 2 (may become fig. 1?) but it should be referred to. And all the features described in the text should be pointed on the map."

We added a map with the features labelled (now figure 1)

p 5686, line 27. no space before the comma.

Fixed

"p 5687, line 11. is 1.25 km resolution enough for modeling the grounding line change in the region. This is stated later in the manuscript that a thinner resolution does not

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impact significantly the results. I think this should be mentioned here, not later."

We have expanded the discussion of resolution to include extra text and figures (4 and 5), and we also re-ran the experiments at 625 m resolution.

"p 5688, line 16. "decomposed into ambient and near grounding line components". I am not sure to understand exactly what it means."

We expanded the description of this process, and added figure 3 showing the melt rates.

p 5690, line 20. "all the simulation show . . . except for WC-FES. . .". 4 exceptions over 14 simulations is not exactly all. Should be rephrased.

p 5691, line 12. A reference to Gagliardini et al 2010, Geophys. Res. Lett sounds appropriate. + main comment (iii) above.

This particluar sentence has been removed, though we do now cite Gagliardini et al 2010 in the discussion of the extreme sensitivity experiments and in the discussion of the melt rates applied.

"Table 1. Some lines appear in bold other not. Why? My understanding is that authors have access to simulations of the ocean and atmosphere models that are not used (normal character). Why showing them? To my opinion it adds noise to the message."

We changes the experimental design in resposne to referee 1 and we use all the data that we mention

"-Tamean? I do not know what it means."

There is now no mention of Tamean

"-LA, GI, FI, not defined in the caption. -Trend should have a unit."

These are gone, the trend is indicated by giving averages for 2000-2100 and 2100-2200

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"Table 2. -caption: removes -> removed -caption: refer to figure 2 to explain the difference between S1, S2,. . . S6 C3131"

The experiments are described in the text, they are a bit more obvious now as they are simply named after the forcing data.

"Table 3. I think it is not explained exactly how (and where) GL advances have been measured."

This table is gone - we think it is not needed now that the figures are larger

"Fig. 1. in caption, 625 km -> 625 m, btw the resolution is not consistent with the text (p. 5687, l. 10). Personally I do not find this figure useful, I think it could be removed"

We have replaced this figure with one that shows end-of-run grounding line position as a function of resolution and a coarser mesh that can be seen more clearly (now fig 4)

"Figure 3. I suggest to replot the figure, a bit of ordering may help the reader, some suggestions -sort panels from Worst Case to Best Case passing through Neutrals, it sounds more logical than the current order. -mention the last year of the simulation in each panel." -WC-BRI-SMB is not described in table 2. I guess it is WC-SMB -I suggest to plot WC-SMB for lines presenting WC-FES, and WC-BRI. Presumably it is the same data (should then be mentioned in the caption) but with different color scale. This would help the reader who is not carefully reading all lines of the tables and all the text as he will naturally search for the WC-FES-SMB panel." -Add one specific line for the control run, keep it vertically aligned with SMB panels -because duration of simulations is different it is not easy to make comparison from one panel to another. Why not expressing the result as mean annual change?"

This figure was replaced. The thinning pattern is much the same now the BRIOS runs are removed (as suggested by referee 1), so we show some flowline plots (now fig 6), including the control run.

"Figure 4. As far as I understand, melt rates are not attached to a given simulation but to

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an ocean model and an emission scenario. Then this is the way it should be described in caption, probably with a reference to the simulations that use one map rather than another. There are four different melt rate scenarios, why plotting only three? Last comment, to me a negative melt rate is an accretion of ice, this is in conflict with the discussion, sign should be changed in the figure."

Figure 4 is replaced with the new figure 3. We changed the sign as requested. There is just one scenario plotted (HadCM3/A1B/FESOM) in the new figure, but we have shown something of its time- as well as space- dependence. The other scenarios are similar in distribution (if lower in amplitude).

"Figure 5. I have done my best, but I did not succeed to clearly follow the discussion related to that figure. I would suggest to rethink completely the figure. Suggestion: make a figure similar to Fig 3 (same position of the panel for a given simulation) presenting map of mean acceleration during the simulation."

We have done as suggested, see the new figure 7. Since the patterns of thinning and acceleration are determined by the ocean model, we plot only the acceleration for the one set of ocean data that reaches 2200 - HadCM3/A1B/FESOM.

"Figure 6. There are apparently two grounding lines in the figures. This is not described in the caption. Furthermore, the aim of the figure is to show the change in grounding line position. Then why showing all the domain rather than focussing only on the ice shelf? This would magnify the picture by a factor of 2 and it would then be much easier to read."

Figure 6 is now split into figure 7 and figure 9, which both show just a region around the ice shelf as suggested, and are larger too.

"Figure 7. Colors seem to have been chosen randomly. I think this would help to make some choice, maybe a given color for BC simulations, another for WC, and symbols depending on the model used. . . . In the present state information are basically lost

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because of the large number of line. I would also suggest to have a second vertical axis showing the SLR contribution in mm. Variation of VAF may be also more suitable than VAF itself."

We have split figure 7 into figures 8 and 10. In figure 8 we use different line styles to pick out the most notable results, some of the others may be harder to pick out because they are so similar, and if we use symbols they overlie one another. In figure 8 the progression of the results makes it easy to pick them out, and we use a progressive color scale too. We have added SLR axes on the right, and switched to variation in VAF.

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