

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.

Anonymous Referee #2

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Authors applied the BISICLES ice sheet model to the Amery ice shelf. Using outputs of atmosphere and ocean models, together with sensitivity experiments they show that this region of Antarctica will probably not become a very significant contributor to sea-level rise (SLR) in the foreseeable future.

Results are interesting, and may sounds well-founded. I however have three main criticisms avoiding publication in the present state of the manuscript. The list of changes I suggest below are numerous so that a second review will probably be of interest.

(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

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should be shown that such uncertainty does not impact significantly the results.

(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.

(iii) adding to the conclusion on the low contribution to SLR, there are two other important results that I believe could be better discussed and highlighted in the manuscript.

- 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! 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.

- 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 has 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.

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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.

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

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

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

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.

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.

-Tamean? I do not know what it means.

-LA, GI, FI, not defined in the caption.

-Trend should have a unit.

Table 2.

-caption: removes -> removed

-caption: refer to figure 2 to explain the difference between S1, S2, . . . S6

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

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

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?

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

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a figure similar to Fig 3 (same position of the panel for a given simulation) presenting map of mean acceleration during the simulation.

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 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 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.

[Interactive comment on The Cryosphere Discuss., 7, 5683, 2013.](#)

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