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Interactive comment on "High-resolution simulations of the surface mass balance of Greenland at the end of this century" *by* G. Krinner and N. Julien

Anonymous Referee #1

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This paper presents interesting results focusing on the current and future Surface Mass Balance (SMB) of the Greenland ice sheet (GrIS) using a GCM. The authors investigate in particular the changes in the precipitation for the end of this century (section 4.2) which is the originality of this paper. I suggest to the authors to make some corrections and improvements (listed hereafter) before publication.

Remark general: The use of a simplified snow model and a resolution of 60km make original these results against the outputs from the models used by the IPCC AR4 report. However, large uncertainties remain in their estimations due to errors in model or due to the simplified physic used in the snow model. To better estimate the errors

in model, I recommend to the authors to compare more in details theirs results with these one published for the current climate using RCM's (Box et al., 2006; Hanna et al., 2007; Fettweis, 2007).

Because the resolution of 60km is not enough to resolve reliably the ablation zone (which is not wider than 100 km in Greenland), the authors use an empirical relationship between interpolated summer temperature and run-off at a resolution of 10km. They forgot to mention that this relation, established with current climate conditions, is perhaps not verified for future conditions.

- Pg 356, line 24: ... biases do occur → biases occur
- Pg 357, line 10: The precipitation rate should be given in km3/yr to be comparable with results from Box et al., 2006; Hanna et al., 2007 and Fettweis, 2007. Table 1 in Fettweis (2007) summaries these estimations. Fig. 7 should also given the current precipitation (or. accumulation) pattern and should be compared with previous studies results (Bromwich et al. (2001), Fettweis et al. (2005), ...).
- Pg 357, lines 13→22: The authors compare successfully the simulated melt area with this one estimated by Fettweis et al. (2007) from passive microwave data. How do the authors distinguish melt from non-melt point in the LMDZ4 simulation ? Do they use the 60km model outputs or the 10km-estimations in Fig. 4? According to Abdalati and Steffen (1997), Fettweis et al. (2007) use a mean Liquid Water Content (LWC) of 1% by volume in the top metre of snow as a threshold value to distinguish melt from non-melt points in the MAR simulation. The mean number of melt days (shown in Fig. 4) have to be compared with Figure 6 from Fettweis et al. (2006) instead of Figure 3 from Fettweis et al. (2007) which shows only the trend of the melt area. In addition, a figure similar to Figure 3 from Fettweis et al. (2007) could be interesting to show if the model can simulate the current trend of the melt area. Finally, I think it would be better if Fig. 4b shows

TCD
1, S176–S180, 2007

only changes from now and not the mean number of melt days at the end of this century.

- Pg 357, line 23→pg 358, line 8: The mass balance rate should be given in km3/yr instead of kg/m2/yr to be comparable with data summarised in Table 1 from Fettweis (2007).
- Pg 358, lines 8→13: The references of Box et al. (2006); Hanna et al. (2007) and Fettweis (2007) need to be added. In Fig. 5, the scale is not easily legible (less levels could be better). Again, Fig. 5b should show the changes. What are the results (10km or 60km) used in Fig. 5 ? Fig. 5 suggests that it is the 60km model outputs? A figure showing results obtained at a resolution of 10km and 60 km could be useful to justify the use of the 10km empirical estimation.
- Pg 358, section 3.2: The mass balance rates should be given in km3/yr.
- Pg 359, line 12: The references of Box et al. (2006); Hanna et al. (2007) and Fettweis (2007) are more appropriate.
- Pg 359, lines 15→23: The authors compare their results only with the Mote (2003) estimations. A comparison with more recent results (Box et al., 2006; Hanna et al., 2007; ...) as it was done by Fettweis (2007) is needed.
- Pg 359, line 25: Be careful: a good simulated melt extent does no induce necessarily a good simulation of the meltwater production! The authors seems to agree with this according to pg 364, line 15? Once the snow pack is beginning to melt, the meltwater production could be overestimated by the model (as it is the case here) while the melt extent is in agreement with observations.
- Pg 360, lines 1→5: A comparison with the last results from Box et al. (2006) is more appropriate.

- Pg 360, lines 14→16: This comparison is surprising. LMDZ4, using both runoff formulations, overestimates the mean surface melt (SMB Precip-runoff) over the ice sheet (pg 357) but here underestimates it by comparison with the observations. The authors explain this underestimation with the fixed albedo used in the model. I agree with them but i think that the resolution could also explain this disagreement (60km is not adequate to resolve accurately the ablation zone). It should be interesting to show also in Fig. 6 the 10km-results.

- Pg 361, lines 19→20: This result is very interesting and is not in agreement with the last 30 years modelled trends in precipitation (Box et al., 2006; Fettweis, 2007) which shows an increase occurring along the south-western margin of the ice sheet while the current temperature is increasing. A the end of this century, this trend seems to be reverted.

- Pg 362, line 26: The results from Browich et al. (2001) are corroborated by Fettweis (2007).

- Pg 364, lines 7→9: I suggest to the authors to show the run-off sectors on a map (e.g.: Fig. 1)

- Pg 364, lines 11→14: It should be interesting to compare the 1981-2005 LMDZ4 freshwater flux to that one simulated by the MAR model (Fettweis, 2007) and by Hanna et al. (2007).

- Pg 365, line 1: It is Fig. 12 rather than Fig. 11. The trends value should be added in Fig. 12.

- Pg 365, line 14: The references of Box et al. (2006); Hanna et al. (2007) and Fettweis (2007) are more appropriate. The trend value should be added in Fig. 11.

TCD
1, S176–S180, 2007

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