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The Cryosphere
Discussions

Interactive comment on "Estimation of the Greenland ice sheet surface mass balance during 20th and 21st centuries" *by* X. Fettweis et al.

X. Fettweis et al.

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We want first to thank Jonathan Bamber (Hereafter J.B.) and the reviewer #1 (Hereafter R.#1) for their constructive remarks and their propositions to extend the discussion about the near future GrIS SMB estimations.

The main corrections in the revised paper are in *italic*. Most of the Figures were improved as asked by the reviewers. The main changes in the text occur in section 4, which was rewritten in part.

Hereafter the response to the reviewers questions:

- **Q1:**

  - *J.B.: Given that the authors estimate the coefficients a and b why do they not*

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present them but only the ratio? I don8217;t understand this. Is not the coefficient a, effectively the sensitivity of the SMB to DT? If so, then how does the sensitivity of the MAR SMB scheme compare with other SMB models for Greenland and the value obtained by Oerlemans et al?

- R.#1: Why do you normalize to have the time series vary between +1 and 8722;1. Wouldn8217;t it be better to normalize such that the standard deviation is 1? What would be the impact on the results?

Indeed, Table 2 and 3 could also list the coefficients a and b used in Eq.1 and the coefficient a in Eq.1 drives well the sensitivity of the SMB to ΔT.

Oerlemans et al.(Ann. of Gla., 2005) use a similar estimation of Eq.1 which is in total agreement with that we have found in Eq.1 by using the MAR simulated time series. Using the same unit as Oerlemans et al. (2005), we obtain a coefficient a of -43.3 mm/K compared with -49 in Oerlemans et al.(2005) and a coefficent b of 3 mm/% compared with 3.8 in Oerlemans et al.(2005). If we use the SMB time series from Hanna et al. (2008) and the temperature and precipitation anomalies time series in Region 1 and 2 from the ERA-40 reanalysis in Eq.1, a coefficient a (resp. b) of -49.4 mm/K (resp. 3.4 mm/%) is found which fully agrees with that was found by Oerlemans et al.(2005). These value are also in agreement with Box et al. (2006).

However, as rightly suggested by the reviewer #1, we decided to normalize the times series by the standard deviation rather than by the min/max (to be included in $[-1, 1]$). The normalization by the min/max allows us to directly estimate the influence of DT compared to DP on the SMB variability but it can not be interpreted in a statistical way since it gives too much importance to exceptional years. This change in the normalization of the time series (introduced to estimate the future SMB changes) impacts a few of the future SMB projections.

The text, both Tables 2 and 3 as well as future projection were updated in the revised paper according to the change in the normalization of the time series.

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Below is Table 5 showing: (top) the future SMB projections for the 21st century using the old normalization; and (bottom) the new normalization of the same time series.

Decade	ΔSMB (in km^3) for a/b=-1	ΔSMB (in km^3) for a/b=-2
2010–2019	-55 ± 61	-83 ± 65
2020–2029	-65 ± 36	-102 ± 44
2030–2039	-85 ± 59	-140 ± 64
2040–2049	-93 ± 51	-165 ± 57
2050–2059	-139 ± 76	-214 ± 75
2060–2069	-153 ± 76	-234 ± 80
2070–2079	-167 ± 74	-268 ± 87
2080–2089	-177 ± 81	-284 ± 90
2090–2099	-185 ± 82	-298 ± 96
2080–2089	-209 ± 134	-328 ± 157
2090–2099	-220 ± 120	-355 ± 148

Decade	ΔSMB (in km^3) for a/b=-1	ΔSMB (in km^3) for a/b=-2
2010–2019	-68 ± 52	-91 ± 62
2020–2029	-83 ± 38	-113 ± 54
2030–2039	-114 ± 50	-159 ± 67
2040–2049	-135 ± 46	-192 ± 66
2050–2059	-172 ± 59	-236 ± 75
2060–2069	-188 ± 63	-257 ± 84
2070–2079	-216 ± 72	-300 ± 97
2080–2089	-229 ± 76	-318 ± 101
2090–2099	-242 ± 78	-335 ± 113
2080–2089	-267 ± 125	-366 ± 162
2090–2099	-290 ± 121	-401 ± 160

- **Q2:**

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- J.B.: How did you select region 1 for calculating a? From 64257;g 1 it looks like most of the ice sheet shows the same correlation and the choice of the region appears arbitrary. Does it make any difference to the results, with a different choice?

- R.#1: The choice of the precipitation region is somewhat strange because it includes, in its northern part, a region where the signal is anticorrelated the the GrIS mean precipitation. Could the region be limited to its south-eastern part? How sensitive are the results you present to the choice of the region?

The west coast of the GrIS was chosen to take the temperature anomalies (Region 1) because the correlation with the SMB time series is the highest (see Fig. 1 and 2) in this region. Furthermore, there are a lot of DMI weather stations along the west coast on which the data set (CRU, GHCN,...) are based. The Region 2 for precipitation anomaly is centred on the Summit (see Fig.1) where the autocorrelation is the highest.

The boundaries of these regions are chosen by tests/errors to have the higher correlations between the GrIS SMB modelled by MAR and the SMB estimated with Eq.(1) by the temperature/precipitation anomaly simulated by MAR as well as from the different data set (CRu, GHCN, ECMWF, ...). The choice of the boundaries does not significantly impact the results as shown below.

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Sensitivity of the choice of regions for Eq.1 using the MAR
time series.  The legend of this table is the same as Table
2.  The first line is the boundaries used in this paper.
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Region 1	Region 2	ΔSMB	$RMSE$	a/b
55-45^{o}W,63-73^{o}N	55-30^{o}W,65-75^{o}N	0.97	28.7	-0.98
54-46^{o}W,64-72^{o}N	55-30^{o}W,65-75^{o}N	0.97	28.8	-1.00
56-44^{o}W,62-74^{o}N	55-30^{o}W,65-75^{o}N	0.96	29.1	-0.95
55-30^{o}W,65-75^{o}N	55-30^{o}W,65-75^{o}N	0.95	33.6	-0.80
55-45^{o}W,63-73^{o}N	54-31^{o}W,66-74^{o}N	0.97	29.0	-0.99
55-45^{o}W,63-73^{o}N	56-29^{o}W,64-76^{o}N	0.97	27.8	-0.96
55-45^{o}W,63-73^{o}N	50-30^{o}W,65-72^{o}N	0.94	36.8	-1.01
65-25^{o}W,60-80^{o}N	65-25^{o}W,60-80^{o}N	0.93	39.5	-0.90

- **Q3:** *- R.#1: Section 3, page 229, line 17: "These parameters are computed over 1970-1999 by using de-trended time series to minimise the dependence on the reference period. It would be interesting to see what impact the use of de-trended series has on the results. Are the parameters a and b very different from those obtained when using the raw time series in calculating the regression?*

  Here is Table 2 using (top) de-trended and (bottom) raw time series.

Name	ΔSMB corr.	RMSE	a	b	a/b
CRU	0.81	64.6	-64.7	48.1	-1.34
ECMWF	0.89	49.3	-63.3	60.9	-1.04
GHCN	0.83	61.4	-60.2	51.2	-1.16
NCEP	0.89	49.6	-72.7	59.7	-1.22
UDEL	0.83	61.0	-71.5	43.7	-1.64
MAR	0.97	28.5	-61.9	63.3	-0.98

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Name	ΔSMB corr.	RMSE	a	b	a/b
CRU	0.80	66.9	-64.4	45.4	-1.42
ECMWF	0.89	49.6	-63.5	61.0	-1.04
GHCN	0.80	65.9	-59.1	47.2	-1.24
NCEP	0.89	49.6	-72.7	60.2	-1.21
UDEL	0.80	63.4	-70.1	36.2	-1.94
MAR	0.97	28.5	-63.5	63.7	-1.00

The use of the raw time series in calculating the regression does not impact signficantly the results.

- **Q4:** *- J.B.: Line 25, p 228. 8217;The excellent agreement8217;. There is good agreement in terms of correlation between the different estimates of DSMB but there also some signi64257;cant differences. For example, in the early 808217;s MAR deviates by 732;50 km3 from Hanna and Box but more serious is the discrepancy that exists from about 2000 onward. This is explained by the use of operational analysis (OA) rather than reanalysis data but I don8217;t understand this explanation because if all models are using the same input data it should make no difference. If the authors are saying that post 2002 they are using different releases or versions of the OA then the comparison post 2002 is not really very meaningful and hard to interpret. The authors need to clarify this point and if they use the same OA data explain why it should worsen the agreement. But, to be clear, differences post 2000 are as much as 200 km-3 which is almost as large as the SMB in the near future.*

  The interannual variability compares well between the models before 2000 while this last one is higher in the MAR model than in the Hanna08 time series. The Hanna08 runoff model is forced by monthly mean atmospheric fields from the ECMWF (re)analysis which could underestimate the impact of extreme warm events during the summer and then reduce the interannual variability.

  After 2001, large discrepancies indeed occur between the models. First, after

2001, there is a succession of negative SMB anomalies inducing an acceleration of the melt the following year due to the albedo feedback and the humidification of the snowpack. These feedbacks could be overestimated in the MAR model compared with other models. In addition, these feedbacks are not taken into account in the (Polar) MM5 model because the surface model is reinitialized every day in MM5. Secondly, different sensitivities of the various SMB models to several profound changes in the ECMWF model configuration/resolution used to produce ECMWF operational analyses (as opposed to the fixed model scheme of ERA-40) could explain the differences in the models since 2002.

- **Q5:** *- J.B.: Fig 6 and 7 are not clear. It is hard to make out the mean and other coloured lines. They need to be redrafted to make them clearer. - R.#1: Figures 5 and 6 are not easy to read. You could use different types of trait, in particular bold, in addition to the colours.*

  We fully agree with the reviewer. We improved "the reading" of most of the figures.

- **Q6:** *- J.B.: Fig 7 is similar to Fig 4 of (Gregory and Huybrechts 2006) but only to 2100 and only for A1B. As far as I can tell the SMB trend, however, is signi64257;cantly smaller in this study even for the case of a/b=-2. 1 mm SLR= 360 Gt/yr or 392 km3/yr. The largest value in (Gregory and Huybrechts 2006) at 2100 for A1B is around 3.5 mm/yr or -1372 km3/yr compared with the estimate of about -150 km3/yr in this study. This is a huge difference. Why?*

  In Fig4. of Gregory and Huybrechts (2006), it is the contribution to sea level (mm yrK1) due to surface mass balance changes. They estimate the current net surface mass-balance to 225 km^3/yr (equivalent to 0.62 mm /yr of SLR) shown in green in their Fig.4. Their largest values of SMB anomalies at 2100 for A2 (red line in their Fig.4) is around 2.7 mm/yr. Our highest SMB changes for 2100 is -668 km^3/yr $\simeq$ 1.8 SLR mm/yr (resp. -624 km^3/yr $\simeq$ 1.7 SLR mm/yr) for CSIRO-Mk3.0. (resp. MIROC3.2 (medres)) with $a/b = -2$. Therefore, the difference is not so large.

The projected sea-level equivalent rise (4-5cm) from SMB changes is in full agreement with Huybrechts et al. (2004), Oerlemans et al. (2005) and IPCC AR4 estimations. This paper confirms then the previous studies. Several comparisons with previous works were added to Section 4.

- **Q7:** *- J.B.: I don8217;t really understand Table 5. There appears to be no correlation between DT and DP, which seems counter-intuitive and non-physical. In fact, in (Gregory and Huybrechts 2006) they quote a value for the effect on DP of DT, and it is around 5% K-1 for both Antarctica and Greenland. If this is really the case in the simulations that there is no relationship then the authors need to explain why, whether this is physical (I don8217;t think it is) and what impact it has on their results.*

  I think that the correlation between DT and DP is not so obvious. For the last 30 years (see Fettweis, TC, 2007), the MAR model simulates an warming of 2.4K since 1979 while no significant changes of precipitation are simulated.

  However, for the future, as explained now in Section 4 and listed in Table 5, the changes of DP reasonably follow the ”5% K-1” from Gregory and Huybrechts (2006) while it is not perfectly linear. The changes of DP in connection with the temperature increase ”5% K-1” is shown in Fig. 7 where the Eq. 5 using this relation fits very well with the ensemble mean in Fig. 7. In addition, the standard deviation of the ensemble AOGCMs mean precipitation for the current climate (in the 20C3M experiment) is 30% suggesting that these changes are mainly affected by the interannual variability which explains that DP is not 100% linear to DT.

  `Updated Table 5`

Decade	ΔT (in K^o)	ΔP (in mm)	ΔP (in %)	5% K^{-1}
2010-2019	0.62	9.32	3.2	3.1
2020-2029	0.8	16.18	4.2	4.0
2030-2039	1.13	41.99	6.9	5.7
2040-2049	1.37	48.34	9.9	6.8
2050-2059	1.63	37.14	8.2	8.2
2060-2069	1.79	26.70	8.6	8.9
2070-2079	2.09	50.28	12	10.4
2080-2089	2.22	76.04	13.4	11.1
2090-2099	2.33	40.95	12.9	11.6

- **Q8:** *- J.B.: Why do the authors chose A1B to do the future predictions with? Is this the most likely SRES scenario or?*

  The A1B is a mid-range scenario and all the IPCC AR4 AOGCM's have outputs for this scenario. It is not the case for A2 (resp. B1) scenario.

- **Q9:** *- J.B.: Line 23, p 231. Authors claim SMB 8217;approximately balances8217; glacier discharge. This entirely depends on what is meant by approximately and the time period considered. It is, for the present day, incorrect, else the ice sheet would 8217;approximately8217; be in balance and it is not.*

  We fully agree with the reviewer. This approximation is due to the lack of reliable estimation of the glacier discharge and basal melting. The last known estimation of these last one comes from Reeh et al. (1999) which assume that the GrIS is in balance and is estimated to be $\sim$ 300 km^3/yr. The GRACE data suggest that the whole ice sheet is losing mass these 5 last years but large uncertainties remain in these observation-based studies because they are not representative for long-term variations.

  We reformulated this litigious sentence in the revised version.

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- **Q10:**

  - J.B.: Line 17, p 232. The authors misunderstand the processes driving glacier acceleration in Greenland. They should cut this section altogether as it is not necessary to the paper, highly speculative and based on erroneous assumptions about ice dynamics.

  - R.#1: Page 232, line 19 (Melt-induced outlet glacier acceleration): Probably not the only process yielding accelerated glacier 64258;ow in a warmer climate (e.g. buttress effect).

  Thanks for these suggestions. We removed this litigious section.

- **Q11:** *- J.B.: L 13-14, p 235. The authors claim that because the multi-model average results agree with observations for 1970-1999 over a small area in Greenland the average 8217;can be used as a reliable estimate of future changes8217;. This is a dubious and unproven statement and, based on the analysis of the precip differences between the different AR4 models, quite possible wrong.*

  Indeed, the multi-model average could miss the evaluation of the future changes as discussed in the conclusion. For a more reliable estimation, a detailed validation of the IPCC AR4 AOGCM's for the current climate should be needed to select only the models able to simulate reliably the current GrIS climate. XF is performing this validation with one of his PhD students (Bruno Franco). However, to a first approximation, the multi-model average could be used, as in Lefebvre and Goosse (2008) and Gregory and Huybrechts (2006).

  Lefebvre W. and H. Goosse, 2008: Analysis of the projected regional sea ice changes in the Southern Ocean during the 21st century, Climate Dynamics, 30, 59-76, DOI 10.1007/s00382-007-0273-6.

- **Q12:** *-R.#1: In a warmer climate, the albedo of the Greenland ice sheet will be different. The same is true if there is more or less precipitation. Therefore,* *the sensitivity to temperature variations will also change. Can you estimate how important this effect would be?*

We can not estimate the albedo feedback without explicitly simulating the future GrIS SMB with a energy balance model (ECM) such as the MAR model. We plan to make such of simulations in the framework of the ICE2SEA project.

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