

Interactive comment on “Reconstructions of the 1900–2015 Greenland ice sheet surface mass balance using the regional climate MAR model” by Xavier Fettweis et al.

Anonymous Referee #1

Received and published: 27 December 2016

Review of

Reconstructions of the 1900–2015 Greenland ice sheet surface mass balance using the regional climate MAR model

By Fettweis and others

General assessment

This paper describes output of the regional climate model MAR, at 20 km horizontal resolution, over the Greenland ice sheet, forced at the lateral boundaries and the model top by a multitude of global reanalysis products, some of which go as far back in time as the 19th century, but in this paper the period 1900 – present is addressed. The pa-

C1

per also evaluates model performance for the benchmark run forced with ERA-interim. These results are certainly interesting and potentially important, but I have two main concerns. First, I found the paper very hard to read, mainly because of the writing style and the fact that the authors decided to show nearly all results rather than making an informed decision on the most relevant parameters and results. Second, the results are described in a merely qualitative fashion, without a more quantitative analysis of the reasons of the substantial differences between the various model runs. As a result a clear conclusion is lacking as to the quality of the re-analysis products for reproducing Greenland surface mass balance. These concerns are described in more detail below, together with some textual comments where clarification is needed (listing not complete). Major revisions will be necessary to bring the paper to a publishable level.

Major comments

The paper is very difficult to read, owing to the multitude of model acronyms and the overwhelming number of figure frames and tables contents. Already early in the paper I got confused by all MAR model versions presented. On page 3 alone, there is mention of MAR, MAR3.5.2, MARv2, MARv3.x, MARv3.2. Some more sobering statistics: the manuscript contains six tables with about 500 numbers, sixty lines in line graphs, more than 40 maps, and the acronym MAR is used almost 300 times!

Readability can be further improved by not combining multiple results in a single sentence. Moreover, results should be easily traceable in figures. This now is not always the case. For example, p. 9, l. 17 reads: “MAR also underestimates accumulation in the south-east versus ice cores but overestimates versus BOX13 because this data set is based on RACMO2 outputs which are known to underestimate accumulation in this area (Noël et al., 2016).” This sentence starts by stating that MAR underestimates accumulation in the southeast compared to ice cores. But in Fig. 6c I see red colours, indicating an overestimation? The sentence continues that MAR overestimates (accumulation) vs. BOX 13, but Fig. 6c shows red and blue colours? Then it concludes that BOX13 is wrong in this area because RACMO2 outputs are underestimating accumu-

C2

lation in the southeast. So what is the conclusion?

p. 3, l. 13: how can SMB be robustly calculated outside the ice sheet mask? If ice is assumed to be present in a certain grid cell that is currently tundra, then the local climate would be altered (cooled) and hence SMB would be influenced?

p. 3: Selecting the 700 hPa temperature as predictor for melt appears unfortunate, because this pressure level intersects with the ice sheet surface. This implies that T at 700 hPa at the lower parts of the ice sheet represents a free atmosphere temperature, at intermediate parts of the ice sheet it represents a boundary layer value (temperature inversion) and at the highest levels a below-surface (extrapolated?) value. This is confirmed by the blue lines in Fig. 1a, which show a conspicuous local minimum over the higher parts of the ice sheet, clearly caused by the ice sheet surface, a feature that would not be expected if a higher level (500 hPa) had been selected. The authors partly recognize this problem by masking out the level below 2000 m in Fig. 1, but that is again unfortunate because melt also does take place above this altitude so part of the interesting information is lost.

p. 4: Why using the obsolete 20CRv2 at all when an improved/corrected product is available (20CRv2c)?

p. 5: A temperature correction of +/- 1 C is applied to 20CRv2 and ERA-20C, based on a 1980-1999 comparison with ERA-Interim. Can one be sure that these biases have been constant before and after this period? Can you show how stable this bias has been in time for the full period 1958-2015 for which ERA-Interim and ERA-40 are available, which appear to perform adequately? Why is this only done for NCEPv1 (line 26)? Why are these results, which are instrumental for the interpretation of the results in this paper, not shown? The same question applies to the height of the 500 hPa level displayed in Fig. 2. How constant are these biases in time over the period with reliable forcing (~1950-present)?

p. 7: To improve the logical sequence of the paper, the evaluation of the ERA-Interim

C3

forced run (section 4) should be presented before the other results in Figs. 1-3.

Figure 5: Knowing that absorbed shortwave radiation is the main energy source for melting, it is quite remarkable that ablation is so well reproduced in Fig. 5c, while surface albedo is clearly too high in MAR, by up to a factor of 2. This would imply that, all other things being equal, ablation would be significantly overestimated had albedo been correct. How can this be reconciled, are there compensating errors?

Textual comments

p. 1, l. 9: validated -> support

p. 1, l. 13: The period 1961-1990 is not commonly chosen as reference period because there was approximate balance, rather it is the only official climatological period that can be chosen before significant changes occurred in Greenland.

p. 1, l. 19: "stationarity assumption" Unclear, please explain.

p. 1, l. 20: "...only suggests..." Unclear, please explain. Does 'only' refer to 'suggests' of the 1920-1930 warm period?

p. 1, l. 20: last sentence of abstract contradicts earlier statement of "...unprecedented melt..." after 1990.

p. 2, l. 5: Please also cite studies that imply a connection between subglacial meltwater injection and frontal ablation of marine terminating glaciers.

p. 2, l. 10: considered -> mentioned (?)

p. 2, l. 11: could not be -> is maybe not

p. 2, l. 19: attractive -> useful, powerful, robust (?)

p. 2, l. 23: validated -> evaluated (please use this throughout manuscript: models are by definition an approximation of reality and can therefore not be validated)

p. 3, l. 12: factional -> fractional

C4

p. 4, l. 7: all of the. . .I think it cannot be claimed that 'all' observations are really used. Perhaps the greatest fraction? Can this be supported by a reference?

p. 4, l. 12: surface marine winds -> near surface winds over the ocean surface

p. 4, l. 13: "As this reanalysis assimilates much less data than ERA-40/ERA-Interim," Is this also true for the overlapping period? Reliable -> accurate, reliability -> accuracy. Has the increase in accuracy been published in literature?

p. 4, l. 16: "covering the half of the last century" Second half.

p. 5, l. 3: Confusing: here summer T at 600 hPa is mentioned, in line 6 summer T at 700 hPa.

p. 5, l. 7: 'drives'. I suspect that what you mean to say is that T at 700 hPa is a good predictor for melt variability in MAR?

p. 5, l. 13: "Surprisingly, the comparison is worse with the 2nd generation of the NCEP reanalysis, which is warmer than ERA-Interim in summer except at the South-East of Greenland" I don't see this in Fig. 1f: the southeast is also too warm?

p. 5, l. 17: ". . .too warm (see Fig. 1c) and too cold (Fig. 1g). . ." I assume 'warm' and 'cold' must be swapped in this sentence.

p. 5, l. 21: Why is the performance of 20CRv2c not discussed here? It was not corrected? Using the acronyms '20CRv2-corr' and 'ERA-20C-corr' is somewhat confusing because the 'c' in '20CRv2c' presumably also stands for 'corrected'.

p. 5, l. 31: gauged -> represented

p. 6, l. 13: "Both ERA-20C forced simulations also significantly underestimate precipitation along the south-western coast. . ." This does not become clear from Fig. 4d.

p. 6, l. 19: "However, both simulations underestimate precipitation along the south-east coast with respect to MAR_ERA-Interim." But these deviations are also mostly

C5

hatched, i.e. are not significant according to the definition used here?

p. 6, l. 26: the same -> similar

p. 6, l. 28: What does the '+40%' mean?

Caption Fig. 1 and elsewhere: Celsius degrees -> degrees Celcius

Caption Fig. 2: Please include explanation of the wind vectors in these plots, do they represent anomalies in the wind field?

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-268, 2016.

C6