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Interactive comment on "Forty-year Simulations of Firn Processes over the Greenland and Antarctic Ice Sheets" *by* Brooke Medley et al.

Anonymous Referee #2

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GENERAL

First of all, I would like to apologize for the time it has taken for me to provide this review. This was an oversight on my part and in no way reflects a lack of interest in this paper or the subject.

This manuscript by Medley et al. discusses a new modeling framework that uses a combination of the MERRA-2 reanalysis and the Community Firn Model to arrive at estimates of SMB and its individual components over the Greenland and Antarctic Ice Sheets for the period 1980-2019.

It is an important and welcome contribution to the existing regional climate models and firn models available for estimating ice-sheet mass balance by means of altimetry or

the input-output method. It is encouraging that more of these models become available, and MERRA-2 combined with CFM is presented as a comprehensive modeling framework.

There are three main concerns with this manuscript, which I hope can be addressed by the authors in a thorough revision. But I am sorry to say that this probably entails a substantial effort. In fact, it would have been better from the start to divide this work into a paper that evaluates MERRA-2 over ice sheets, and a paper that deals with the CFM for elevation change and runoff estimates.

1) Evaluation of MERRA-2 against independent observations.

In a global sense, MERRA-2 has been evaluated in a large paper by Gelaro et al. (2017). However to my knowledge, the performance of MERRA-2 over Greenland and Antarctica has not been assessed against observations, like it has been done extensively for e.g. MAR (Fettweis et al., 2013, Agosta et al., 2019), HIRHAM5 (Mottram et al., 2017), RACMO (Van Wessem et al., 2018, Noël et al., 2018) or CESM for climate variables ánd individual SMB components (temperature, snowfall, surface melt, albedo, cloud cover, firn temperature, runoff, etc.). This paper does present SMB and its components but they are not verified, evaluated, or compared against existing estimates. This is worrisome, since MERRA-2 serves as the input for CFM which computes the surface elevation changes needed to correct for ICESat and other altimeters. Thus, the authors will have to make sure that the reader can be confident about the MERRA-2 output over the ice sheets.

2) Uncertainty analysis

The paper lacks uncertainty analyses. Uncertainty intervals of the SMB components are a standard deviation of the sample of annual values. But there is no estimate of the uncertainty of annual values, established for example by independent evaluation against observations (see 1). For the volume and height changes, no uncertainty estimates are provided. Most effort in presenting numbers is not in the numbers themselves, but in quantifying the confidence that we put in them. This is also crucial for establishing an uncertainty estimate of mass loss determined by altimetry. A robust uncertainty analysis must be added to the paper.

Quite a number of processes could be identified that give an uncertainty. Regarding the surface elevations changes from CFM, probably the most important ones are the uncertainty in SMB, snowfall, surface melt, and the assumption that the RCI period is representative for the long-term climate (see comments by reviewer 1).

Adding an uncertainty estimate allows for a fair judgement of the numbers presented in this paper, and for an comparison with existing literature (see also point 3), like for example the volume change estimates from Kuipers Munneke et al., which at first sight appear to be larger over Greenland than from MERRA-2.

3) Embedding in existing literature

While I believe that MERRA-2/CFM is a very valuable addition to the suite of models that are currently available to estimate SMB and firn changes over the ice sheets, the literature from those existing models is largely ignored in this paper. It would be insightful if the numbers in this study are compared to existing estimates of SMB and firn volume change (MAR, RACMO, CESM, HIRHAM, ...). Also it would be fair to refer to previous work into, e.g., quantifying seasonal cycles of firn processes (Ligtenberg et al., 2012), or volume loss over Greenland.

SPECIFIC COMMENTS

Page 1 line 10: suggest to include the time frame of the simulations in the abstract: "new simulations of firn processes (1980-2019)"

Page 5 line 126: how is temperature initialized for locations with SMB<0?

Page 7 line 191: why was the functional form of equations (11) and (12) chosen to be an Arrhenius-style rate equation? Is there a reason to suppose that the misfit between modeled and observed densification rates should take this form?

Page 7 line 211: it is interesting to see that E_0 is found to be zero. It implies that the correction factor R_0 reduces to b^{beta_0}, i.e. with no dependence on temperature. For the reader, it would be interesting to show this result rather than the current figure 4. The present figure 4 is of limited interest, since the correction procedures forces the data points to the 1:1 line by design. It would be more interesting to show the validity and behavior as an Arrhenius plot:

R_0 = b^(beta_0) exp(-E0/RT)

ln(R_0) = beta_0 ln(b) - E_0 (1/RT)

With E_0 = 0, plotting ln(R_0) against ln(b) would show beta_0 as the gradient and 0 as the intercept of a linear plot.

For R_1 it is more difficult. A normal Arrhenius plot would look like

ln(R_1) = (-E_1/R) * (1/T) + beta_1 * ln(b)

which is a linear y = mx + d with 1/T as x and beta_1*ln(b) as a constant. However, the latter is not constant but depends on b. Perhaps it is possible to select a subset of $\sim$20 firn cores with a narrow range in b so that a constant value for (beta_1*ln(b) can be assumed, and ln(R_1) can be plotted against 1/T as a linear function.

Page 9 line 270: I can understand that you have looked only at the significantly related predictors, and not at their physical interpretation. But I think that mean northward wind speed is an awkward predictor especially in the Greenland context. Could the performance of the model be retained while dropping V_0?

Page 10 line 297: I am struggling with the fact that no meltwater-related output from MERRA-2 was used in this manuscript, only snowfall. Is MERRA-2 not designed to provide this output? Is it of insufficient quality? Was it forgotten? Regarding runoff, was the runoff output from MERRA-2 used, or from the firn model? This remains unclear. In any case, is the runoff from the firn model comparable and similar to the runoff from MERRA-2?

Page 10 line 300: The degree-day modeling approach is confusing, and quite rough. The approach by Van den Broeke et al. (2010) was devised only in cases where daily mean temperatures temperatures are available, to compensate for the lack of representation of the daily cycle. I have tried to understand why such low T_0 are required to get a good match at an annual scale. I guess this is only possible if MERRA-2 has a strong cold bias, or a very poor representation of the daily temperature cycle, or if the degree-day approach actually fails over the entire range of melt-temperature combinations on both ice sheets. I would like the authors to comment on this. More generally, I wonder if surface melt in MERRA-2 is poorly represented, or if it was forgotten or otherwise impossible to obtain this output variable.

Page 13 line 382: a map showing the absolute change in FAC would be interesting here.

Page 14, section 3.2: To my opinion, it is important to emphasize that the surface melt estimates from Greenland and Antarctica are not independent numbers based on the outcomes of a physical model. Rather, these are the results of a degree-day model calibrated to observations (Antarctica) and another model (MAR, Greenland). Especially for Antarctica, it is presented (lines 427 and further) as if the melt estimates are an independent result of MERRA-2 and CFM, whereas in reality the degree-day method is tuned to reproduce the numbers by Trusel et al. over a part of the RCI.

Page 15, line 444: RFI -> RCI (?)

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Agosta et al., 2019. Estimation of the Antarctic surface mass balance using the climate model MAR (1979-2015) and identification of dominant processes, The Cryosphere

Fettweis et al., 2013. Estimating the Greenland ice sheet surface mass balance contribution to future sea level rise using the regional atmospheric climate model MAR, The Cryosphere

Gelaro et al., 2017. The modern-era retrospective analysis for research and applications , version 2 (MERRA-2), Journal of Climate

Kuipers Munneke et al., 2015. Elevation change of the Greenland Ice Sheet due to surface mass balance and firn processes, 1960–2014, The Cryosphere

Ligtenberg et al., 2012. Quantifying the breathing of the Antarctic Ice Sheet, GRL

Mottram et al., 2017. Surface mass balance of the Greenland icesheet in the regional climate model HIRHAM5 : Present state and future prospects. Low Temperature Science. Series A. Physical Science.

Noël et al., 2018. Modelling the climate and surface mass balance of polar ice sheets using RACMO2 - Part 1 : Greenland (1958-2016), The Cryosphere

Trusel et al., 2013. Satellite-based estimates of Antarctic surface meltwater fluxes, GRL

Van den Broeke et al., 2010. Temperature thresholds for degree-day modelling of Greenland ice sheet melt rates, GRL

Van Wessem et al., 2018. Modelling the climate and surface mass balance of polar ice sheets using RACMO2, part 2: Antarctica (1979-2016), The Cryosphere

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