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The Cryosphere Discussions

Interactive comment on "Sensitivity of Greenland Ice Sheet surface mass balance to surface albedo parameterization: a study with a regional climate model" *by* J. H. van Angelen et al.

J. H. van Angelen et al.

j.h.vanangelen@uu.nl

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AC: We are grateful to the reviewer for providing detailed and constructive comments, which helped to improve the paper. All issues raised are addressed below.

This paper presents a sensitivity analysis of the surface mass balance of GrIS, after implementation of a new albedo calculation in the climate model RACMO2. While the work is important, and I see no major issue with the science undertaken, the description of results and conclusions is often too vague to be convincing. Adding precisions and discussions throughout the text will address this issue.

AC: Throughout the manuscript more interpretation and discussion on the findings is

added. The conclusion section is improved such that it contains better quantified statements. See also author comments on Jason Box comments.

Specific comments: -p34, l.6: IMAU has been doing work on K-transect since early 1990, surely there is earlier work than Van de Wal 2005, to credit on this?

AC: There is indeed earlier work. However Van de Wal 2005 gives the best and first complete overview of the mass balance data and location of the measurements as used in this study.

-p36, l.10-12: rephrase to avoid repetitions of "new albedo", "RACMO2" and "discussed/discusses". If the new albedo scheme is fully presented elsewhere, make it clear where (e.g., Kuipers Munneke, or Gardner and Sharp??)

AC: The section is rephrased to make clear that the scheme is fully presented by Kuipers Munneke, based on the theory presented in Gardner and Sharp.

-p36, eq 1: consider giving an expression for alphaS? -p36, eq2: specify what "t" is.

AC: Expressions for αS, dαu and d$\alpha\tau$ are given and discussed by Kuipers Munneke. The expression for dαc is given because the correction for black carbon is not applied for Antarctica. "t" is specified.

-p38: How is the bare albedo related to the model described in section 2.4? A word of introduction/explanation would be useful.

AC: An explanation is already given on p38: "The BIA is used if the density of the top two layers of the snow model is equal to the density of ice (910kgm$-$3)".

-p38, l.21: What is the justification for taking the averaged of the lowest 5% of data to define the BIA - and what is the sensitivity of the BIA field to this value?

AC: We added some text to justify the 5% approach. The reason is that if the absolute minimum is chosen, the impact of one very low value will have an unrealistic big impact on years with higher minimum albedos. In the attachment Figure A3 shows the differ-

ence between the lowest measured value and the 5% approach. Figure A4 shows the standard deviations of the lowest retrieved albedo value each year. Which can be interpreted as sensitivity of the BIA field and is for most regions less than 0.1. “Since the current albedo scheme does not account for intra- and inter-annual differences in BIA, the 5% approach was chosen to represent the multi-year BIA as it is more robust to outliers than the minimum background albedo and corresponds better to the multi-year minimum albedo, as taking the minimum albedo will lead to negative albedo biases in years that don’t reach the overall minimum. For example, the lowest 5% albedo values for the 2000-2011 BIA correspond with the BIA values observed in 2007, whereas the minimum background albedo is observed in 2010.”

-p39, l.1: "reasonable stable" is vague. Consider providing a range of values?

AC: Changed to: “Although ice albedos as observed by MODIS are reasonably stable from year to year, some inter-annual variability remains, with a maximum spread of 0.15, but for most locations less than 0.05”.

-p40, l.1: Check units in table 1, which indicate a change from 1000 to 2000 mm.

AC: Corrected to 1 and 2 mm.

-p41, l.1-3: the authors are comparing their results with those using the density dependent albedo scheme: would it be possible to show the latter on Figure 6a? Alternately, why not showing the results using the previous albedo scheme for year 2007 and/or timeseries 2004-2009, on Figure 3 (consistently with figures 5-6), rather than for year 2006?

AC: We have displayed the year 2006 in figure 3, because in this ‘normal’ year, the albedo at station S9 was clearly too low for the density dependent albedo scheme, which is the main reason for the implementation of the grain size dependent albedo scheme. For a better comparison with the density dependent albedo scheme, we have added a line in Figure 5 representing that scheme.

-p41, l.4: how much higher is the minimum albedo?

AC: Added: “up to 0.15 in 2006”

-p42, l.7: be more precise ("too high")

AC: Changed: “is approximately 200∼Gt too high”

-p42, l.26-27: shame that no more is said on that point! If the authors were able to quantify this a bit more, it would be a great addition to the paper. Right now, it sounds interesting, but "probably" make it sound speculative.

AC: The word probably is removed; since it is certain that a warmer snow pack at the end of the season will lead to earlier onset of melt the next season. Unfortunately a good quantification of this process requires more simulations, which is not feasible at the moment, but will be studied when a scenario run for a warmer climate is performed.

-p43, l.2: "almost 20%", or 18%? ...

AC: Changed to 18%

-p.43, l.11: again, total melt increases 5%, runoff increases 7%. Suggest using "similar amount", rather than "same".

AC: In absolute numbers both melt and runoff increased by 23∼Gt. Since runoff is smaller than melt the percentage change is higher. For clarity “(23∼Gt)” is added after same amount.

-p.45, l.1: "somewhat underestimated" => be more precise. Also, the justification for the underestimated SMB not being related to albedo is incomplete: how about the albedo at S6 (1000m)? Figure 5 show that for year 2007, it is lower than observed. How much of the underestimation can be attributed to this? By how much is the sensible heat flux overestimated at S6 and S9?

AC: Text is changed with a more quantified explanation of the underestimation and

explanation using numbers for the offset in SHF by RACMO: “Between 1000 and 1700 m elevation, the total SMB is still underestimated by 0.5-1.0 m w.e. As a result, a discrepancy between the equilibrium line altitude in RACMO2 (around 1800 m in Figure 10) and the stake measurements of 200 m is present. This offset is probably not related to albedo, since the remaining bias between measured and modeled albedo at station S9 (1500 m) is too small to explain a discrepancy of 0.5 m w.e. Ettema et al. (2010a) demonstrated that the sensible heat flux (SHF) is overestimated by up to 20 W m$-$2 at S6 and S9 for the summer months (JJA), equivalent to $\pm$40 cm of ice melt. SHF can thus be regarded as a realistic candidate to explain the offset as seen in Fig. 11, especially because the overestimation of SHF by RACMO2 is not present at station S5. It should also be kept in mind that, especially in the area around the equilibrium line, surface mass balance estimates derived from stake measurements have a large uncertainty, because the density of the melted (superimposed) ice and firn is not accurately known.”

-p.45:, l.24: use a more objective statement than "well represented". Figure 10 shows an improvement in the gradient for the lower region of GrIS (<900m), it also shows very little difference at higher elevations: any insights into why this is? Is that directly related to the use of BIA?. The authors need to add some discussions to convince that the new albedo scheme is what led them to this improvement. For instance, they state earlier (p.45, l.11) that "the use of a background ice albedo field improves the results in the lower ablation region". So, what does Figure 10 becomes, for any other of the sensitivity runs?

AC: More elaborate discussion is added throughout the results section and better quantified statements are given in the conclusions. Figure 10 presents results for a 20 year period. It is computationally too expensive to perform all the sensitivity experiments for such a long period. In the lower ablation region the BIA is the dominant parameter in the albedo scheme. That said, the gradient along the k-transect will be similar to the density dependent albedo scheme for the other experiments

-conclusions: it would be nice to have a clearer statement of which parameter is the most important in assessing the albedo.

AC: The conclusion section contains now statements on what the influence is of the different parameters and the importance of each: “The RACMO2 simulations for the year 2007 show a strong sensitivity of the SMB and the individual components on parameter settings in the albedo scheme. GrIS total SMB varies between 177 and 444$\sim$Gt. The introduction of 0.1 ppmv black carbon to the albedo scheme has the strongest impact on total SMB with a drop of 164$\sim$Gt, with the strongest effect on melt (+100\%) in the accumulation area. Next to that refreezing and retention of melt water has a strong feedback on the albedo and thus SMB components. A doubling of the grain size of refrozen snow results in 30\% more melt in the southern accumulation region and a 50$\sim$Gt lower SMB. An increased retention capacity of the snow pack leads to a 10\% increase in melt uniformly over the ice sheet, but since all the extra melt water is refrozen, the impact on SMB is negligible for a single year. Finally the reduction of the ice albedo by 0.05 is of minor importance (SMB -20$\sim$Gt), however it will become more important if in a future warmer scenario a larger area of bare ice will be exposed to the surface for a longer period each year.”

-Again, a lot would be gained from exploring more the interaction between albedo/refreezing/retention of melt water. But if it is not feasible to run one more simulation (too check how much more melt would be produced the following year), then, as it stands, there isn’t much point bringing this point back into the conclusion (and in the abstract, for that matter).

AC: Unfortunately exact numbers on the feedback mechanism can not be given. But the process itself of more refreezing resulting in a warmer snowpack by latent heat release is clear and therefore left in the abstract.

Typos etc: -p33, l.11: "balance" -p35, l.5: replace "by" with "at" -p35, l.7: replace "the implementation" with "implementing" -p37, l.11: suggest using " ... Summit, Greenland

(Flanner et al, 2007) -p37, l.21-23: suggest removing the sentence "The colored lines show ..... and0.2ppmv (red)", belongs to figure captions. -p38, l.16: add a dash to "16-day" -p38, l.27: delete 0.52 (already specified in that sentence). -p41, l.1-2: shorten or remove first sentence (figure caption). -p41, l.24-27: suggest removing this broad description of what to find on figures 7 and 8. A reference to appropriate figures (as is done on line 9, p42) seems sufficient. -p.44, l.21: insert "rather" after correctly. -Label S5 / S6 /S9 on Figure1 and Figure 9?

AC: All typos are corrected.

Interactive comment on The Cryosphere Discuss., 6, 1531, 2012.