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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.

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

Overview The paper presents a RACMO upgrade that is important to more accurately represent the melt process. The study gives some insight into surface mass balance sensitivity to albedo.

Major Critique The choice of the 16-day MODIS MOD43 albedo product over the daily MOD10 product is questionable. Had the study chosen the daily product, more robust RACMO2 performance statistics would be calculable from e.g. Figs. 5 and 6. I find no

rationale given for the choice of coarser temporal resolution data.

AC: First, we agree that the 16-day temporal resolution of the MCD43 albedo data is a disadvantage in comparison with the daily MOD10 albedo product, but we believe that the multi-day algorithm of MCD43 is more robust than the daily product of MOD10 albedo as the latter shows larger error dependence on solar zenith angles (SZA). Results of these analyses will soon be presented to the Cryosphere Discussions, but the Figures A1 and A2 in the appendix already show the error of both MODIS albedo products in comparison with AWS albedo in function of SZA. Comparison of both figures illustrates the SZA dependence of the MOD10 product.

Second, we acknowledge that Box et. al. (2012), "Greenland ice sheet albedo feedback: thermodynamics and atmospheric drivers", The Cryosphere, obtained better correlations with AWS albedo using the MOD10 product, but this was based on a 11 day median processing, which implies the resolution is reduced also to 11 days, which is a small advantage over the 16 day product, and will not correct for the SZA dependence.

Why is the larger melt season in 2010 not assessed in this study? With even lower albedo in 2010 than 2007, is the background albedo is low enough everywhere?

AC: 2007 is chosen for evaluation instead of 2010 because by the time of this study AWS data of the K-transect for 2010 was not available yet. MODIS albedo data of 2010 are incorporated to come to the final background ice albedo field. Since we are dealing with a climate model the goal is to simulate the average realistically. So there will be years where the albedo does not drop as strong as the BIA and years with lower albedo, such as 2010.

The study should develop insight into the causes and implications of the identified systematic equilibrium line altitude bias.

We have elaborated the discussion on the equilibrium line altitude bias: "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.".

The conclusions section needs improving. It should not spending valuable space speculating about future work. The earlier discussion section should point to future work.

AC: We have changed the conclusion section, such that it now gives clear statements on the implications of the sensitivity tests. Speculation of future work is moved to the results/discussion section. "The RACMO2 simulations for the year 2007 show a strong sensitivity of the SMB and the individ- ual components on parameter settings in the albedo scheme. GrIS total SMB varies between 177 and 444 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 Gt, with the strongest effect on melt (+100%) in the accumulation area. Refreezing and retention of melt water also have a strong feedback on the albedo and thus SMB components. A doubling of the grain size of refrozen snow results in up to 30% more melt in the southern accumulation region and a 50 Gt lower SMB. An increased retention capacity of the snow pack leads to a uniform 10% increase in melt, 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 Gt), however it will become more important if in a future warmer scenario a larger area

of bare ice will be exposed at the surface for a longer period each year."

Comments Black carbon is not the only important solar absorber. Some combination of multiple sources of impurities including terrestrial dust and microbiological components should be mentioned. The authors should discuss this complexity.

AC: Black carbon is a much stronger absorber than other impurities such as dust, and therefore can be regarded as the main absorber in a first order approach (Wiscombe and Warren 1980, Gardner et al, 2010).

Does the background ice albedo evolve AFTER being defined once using observations?

AC: No, in the current scheme we have used a geographically defined BIA that does not account for intra- and inter-annual differences in BIA. We agree the BIA can evolve, and therefore, in the future, we hope to develop an improved BIA parameterization that accounts for inter- and intra-annual differences. However, for now we believe the geographically defined BIA already provides a large improvement to the previous fixed BIA scheme.

pg. 1533 4,5: It is not clear from the literature if over a snow and ice surface that cloud cover is a dominant control on incident solar radiation and in turn absorbed solar radiation. Under cloudy skies, an increase in multiple scattering can render the cloud radiative effect small. Further, an increase in UV downward occurs under cloudy skies. The authors should discuss this complexity or remove this unsubstantiated claim.

AC: Cloud cover reduces the amount of incoming solar radiation by approximately 50%. The total radiation balance is shifted, with indeed higher downward long wave radiation fluxes, but in total incoming radiation is much smaller under cloudy conditions. So this statement is left as it is.

9: Stroeve, 2001 not Stroeve, 2007.

AC: corrected

11-12: “high resolution” is ambiguous, quantify the statement or remove “high resolution”

AC: 25 km or less added

17: replace”a particular” with “an” pg. 1534

AC: corrected

10: please use “downward and upward” instead of “incoming and outgoing”. These are hemispheric vertical fluxes.

AC: corrected

15: use “ablation rate” instead of “mass loss” pg. 1536, 10-11, “discuss” twice in once sentence is awkward

AC: corrected

pg. 1537, 3, “too expensive for this RACMO2 implementation” 8: quantify “very low” 11: remove “even”

AC: very low changed in: “too low to have a significant impact on the snow albedo”

pg. 1538, 24: be more explicit by what is meant by “missing”. The solution chosen seems to produce erroneously low values (green areas) between 1500 m and 2000 m on the N and NE ice sheet (Figure 1). Averaging the non-missing data by elevation bins to fill in the gap using an elevation regression to solve for albedo should âĹijeliminate this gap.

AC: By missing we mean gridpoints without enough measurements to come up with a background ice albedo field. We agree that values in the N and NE are unrealistic. However given that these regions do not become snow free in the near future it will not have any effect on the results. In future simulations we will correct these values to 0.55, being the value used for snow covered areas.

pg. 1539, 1-2, quantify the statement “Although ice albedos as observed by MODIS are reasonably stable from year to year, some inter-annual variability remains.” “reasonably stable” and “some inter-annual variability” are vague. Once quantified, does the variability exceed the specified or expected accuracy of the data?

AC: Changed to: “Although ice albedos as observed by MODIS are reasonably stable from year to year, inter-annual variability is present, with a maximum spread of 0.15 (see attachment), but for most locations less than 0.05. For example, at the location of S6, the lowest MODIS-derived ice albedo values range between 0.34 and 0.44 in the period 2004-2010. This variability is partly due to measuring uncertainties (mainly due to cloud cover), and partly a real phenomenon, possibly associated with delayed supraglacial runoff of meltwater (Van den Broeke et al., 2008b)”.

3: replace “MODIS-derived ice” with “MODIS MOD43”

AC: For readability MODIS if prettier than MOD43. Therefore at the first mentioning of the MODIS data we explained the MOD43 product and stated that afterwards MODIS is used to refer to the data.

9: remove “the”

AC: corrected

10: Year 2010 is a larger melt year, especially because of low albedo (Tedesco et al. 2011, ERL) and thus would be a better test if the background albedo is low enough? I wonder if the prescribed background albedo is not low enough to handle year 2010. Year 2011 was also anomalously low for albedo (Box et al. 2012, TC). Even though the model tests are “computationally expensive and time consuming” (this reads more like an excuse) they are worth while to really know if the model upgrade can handle extreme years, especially that are likely to occur in future years as melt increases further.

AC: We agree that by using geographically defined BIA that does not account for intra- and interannual differences in BIA, the current model improvement perhaps fails to handle extreme years (e.g., the 5 percentile BIA is chosen to represent multi-year BIA, not extreme BIA). However, developing a BIA that accounts for intra- and interannual differences will require redesigning the current albedo parametrization, which we are working on already. Nevertheless, we believe the current BIA already provides an important improvement to the albedo scheme

13: specify “MOD43” as the chosen MODIS albedo product.

AC: Done:

lines 16, 17, 20, and 24: “MOD43” instead of “MODIS”

AC: See earlier comment

Fig. 5. The chosen MODIS data, lacking temporal resolution, make the comparison less effective than using the daily MODIS product (Stroeve et al. 2006, RSE). Provide some rationale why the higher temporal resolution data was not chosen.

AC: see comments above.

pg. 1540, lines 3, 15, and 21: “MOD43” instead of “MODIS”

AC: Changed

26: quantify “good agreement” ideally using correlation, average difference, and RMSE. If time resolution is a fundamental problem, use the nearest samples in time to the MOD43 data then these statistics may be calculable.

AC: In this study MOD43 data is used to construct a background albedo field. We feel that a direct comparison between MOD43 data and RCM output is not feasible given that MOD43 data has only clear sky measurements, and the daily product of MOD43 is questionable. Average difference and RMSE are given for the comparison between RACMO2 and the AWS data.

pg. 1541, 7: replace “MODIS uses a” with “MOD43 data incorporate a” Table 1. State

“MODIS MOD43” instead of “MODIS”

AC: Changed

9: other reasons than the frequency of summer snow events are likely such as snow patches and whether or not the AWS radiometer is over one or over an area of bare ice. Discussion of this and the problems comparing point AWS measurements with area data (MODIS) is warranted here.

AC: Discussion on this is extended: “Another source of discrepancies is the fact that MODIS observes a larger area compared to an AWS, and small snow patches or bare ice areas can influence the AWS measurements”.

10: “when” instead of “where”

AC: Changed

Is “preceding simulation” or the “control” the same? If so, just use “control”

AC: No, it is not. Preceding refers to the order of experiments in Table 1.

Fig. 7. It’s too hard to see the difference between the different experiments. Plot instead the difference with the “preceding simulation” or the “control”, same units, just a difference map.

AC: We changed figure 7 to a difference map with respect to the SSM/I data. The SSM/I data is displayed in a separate figure.

pg. 1542 It is good to see Table 2, is valuable.

13-16: Important results that deserve highlighting in the abstract and conclusions.

AC: Conclusions on processes and the impact on SMB are highlighted in the conclusions section.

pg. 1543 1st paragraph, important results that deserve highlighting in the abstract and conclusions.

AC: Stated in the conlusion.

29: remove “gradually”

AC: Changed

pg. 1544 2-3: quantify “good” and “regional differences remain”. Good and bad are not useful in technical writing. Instead, make the effort to quantify allowing the reader to judge for themselves about the model performance. If the differences are smaller than the noise then you can conclude “insignificant” difference which I think is what is meant by “good”. Anyway, back up such statements using quantities.

AC: Throughout the manuscript the comments on agreement between model and observations are quantified as good as possible. In this specific case: “For this final simulation there are still discrepancies of up to two weeks between the observed and modeled length of the melt season for 2007 (Fig. 8g), but the modeled average over the GrIS (17.4\%) is matching well with observations (17.3\%) (Table 2). The pattern of the regional differences, i.e. an underestimation of melt duration for the southern ablation region and an overestimation higher up the ice sheet, suggests that albedo is likely not responsible for the discrepancies shown; changing the albedo scheme settings alters the number of melt days in only one direction, i.e. a longer or shorter melt season for the entire ice sheet.”.

20: “contribution” instead of “rise”

AC: Changed to increase in “contribution to SLR”

22: It may not be possible to make this conclusion now that a more realistic albedo scheme is used. Is the conclusion still valid?

AC: This conclusion is still valid. Although the albedo scheme is changed, the feedback mechanism between high temperatures, low albedo and high melt rates is still standing.

instead of Figure 9 in which an unscientific statement “well represented” is used and it’s

not easy to see difference in color, make a scatter plot (perhaps not show it, excluding Fig 9) and state the mean bias, correlation and RMSE, allowing the reader decide about model performance. Incidentally, it’s controversial to smooth the gridded data in this type of mapping. Instead, plot the grid cells individually and assign them a color, adding realism to the representation of this digital data. The digital data will look more like Legoland of a facsimile than a smooth(ed) continuum.

AC: Figure 9 is left as it is in the manuscript, since it nicely reflects the high variability in SMB at constant height intervals. The option of using filled colored symbols is not chosen, because it is difficult to implement the stake measurements in the same figure.

pg. 1545 2: instead of using adjective “good”, provide a quantitate assessment and allow the reader to judge model performance.

AC: This section is better quantified: “Figures 10 and 11 compares the averaged observed surface mass balance along the K-transect for the period 1991 to 2010 with results of RACMO2 using the CONTROL settings. The observed SMB ranges from -4 m w.e. at an altitude of 500 m to +0.5 m w.e. at 2000 m. Between 700 and 1500 m a gradient in SMB of 3.8 m/km is measured. The CONTROL simulation has a gradient of 3.2 m/km, a significant improvement compared to the 2.3 m/km in the density dependent albedo simulation. 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) and firn ice is not accurately known."

19: "MODIS 16-day albedo retrievals" instead of "MODIS satellite retrievals"

AC: Changed

use "scheme" instead of "parameterization" consistently. The model upgrade is more than simply a new parameterization. Therefore, "scheme" (or algorithm) seems better because it suggests more than a line or 2 more code which a parameterization can often fit into.\

AC: throughout the text, parameterization has been changed to scheme, when referred to the complete scheme.

24: Figure 10 does not make this reader think about "well represented". A word choice like "better represented" is a start instead of this vain and unscientific statement. Discussing a % reduction in RMSE with values presented is even more desirable.

The improvement in representations of the gradient is quantified using a linear fit. "Between 700 and 1500 m a gradient in SMB of 3.8∼m/km is measured. The CONTROL simulation has a gradient of 3.2∼m/km, a significant improvement compared to the 2.3∼m/km in the density dependent albedo simulation".

pg. 1546 first sentence is a hypothesis and therefore doesn't belong in a conclusions section. Move it into previous section. Reserve the Conclusion section for conclusions.

Expand the conclusions to capture more such as the very useful details in Table 2 and section 3.2 in which the implications of the model upgrade are felt and conclusions can be made about, for example, what is the ranked order of importance in model improvements.

The last paragraph referring to future work does not consist of Conclusions, only an outlook to work that may or may not happen. Move this to a previous section of just

remove it and instead spend words on the present study, its implications.

AC: Throughout the result section more words are spend on the interpretation and quality of the results. The conclusion section now only contains conclusions and is more elaborate to include findings as shown in Table 2 and section 3.2

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C1366/2012/tcd-6-C1366-2012-supplement.pdf

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