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The Cryosphere Discussions

Interactive comment on "Greenland ice sheet albedo feedback: thermodynamics and atmospheric drivers" *by* J. E. Box et al.

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comment: While 11-year surface temperature change is not shown, the text implies that most of the ice sheet has also experienced substantial warming over this period.

response 1: A new Appendix Fig. A10. illustrates "summer spatial patterns of MAR simulated Tair change."

comment: Thus, the 11-year albedo feedback must be positive over most of the ice sheet!

response 2: A negative (albedo) feedback is possible in the presence of a positive (temperature) trend. Negative feedbacks dampen an initial perturbation not reverse them. Positive feedbacks reinforce an initial perturbation. The negative feedback stems

from increasing snowfall with increasing temperature (Fig 9a), confirmed over much of the ice sheet in the correlation between snowfall and albedo (Fig 10a) and Fig 10b that confirms increasing albedo with Tair.

Presentation of a “bulk feedback” is useful and while we contend that computing albedo feedback using interannual paired anomalies yields a more insightful result, bulk albedo feedback is now included throughout the paper, including in the abstract, the methods section, the Section entitled “Ice sheet albedo feedback” and a new figure 12 in the main article, and a new Table 3.

comment: Clearly processes other than those determining correlation (at zero lag) between albedo and temperature anomalies are driving longterm albedo change, and these processes must be accounted for in the quantification of albedo feedback.

response 3: Discussion of time dependence is made in text referring to Figs. 5 and 7.

With regard to zero lag being “near-instantaneous”, we assert that 3 month summer periods are more than sufficient for the interaction of temperature and precipitation to influence albedo. We made additional albedo correlation analysis with snowfall, rainfall, and temperature to establish causal links.

Neither Fernandes et al. (2009) nor Flanner et al. (2011) include a lag analysis in their data compilation. Our data compilation is similar, though is explicitly at the surface not the top of the atmosphere.

comment: “...more appropriately labeled a "sensitivity" in the caption for figure 11a.

response 4: We indeed use "sensitivity" in the caption for figure 11a (now figure 12a). If what the reviewer means is that instead of "sensitivity", what we call “feedback” in figure 11b (now figure 13b) is semantics we disagree on. The key to our albedo feedbac metric is evaluating the relationship between air temperature and albedo or net shortwave using paired annual anomalies, which we now more clearly define in the text.

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I regret the explanation of how was not more clear. The regression is not over time. New sentences below are inserted in the relevant paragraph that now reads:

The albedo sensitivity to near-surface air temperature (at ∼3m above the surface in MAR calculations), expressed here in units of % albedo K−1 is determined by regression between 12 annual samples of detrended anomalies of June–August average alpha and Tair. The regression is between annual pairs of anomalies rather than values in a time series. Similarly, we quantify the albedo feedback in units of W m−2 K−1 using summer average, Snet and Tair: Evaluating the magnitude of sensitivity in anomaly space rather than time space is an advantage of this regression based approach. The concept and the effect of temporal detrending is illustrated in Appendix A.

comment: Figure 11b shows albedo feedback in units of W/m2/K (also negative feedback over much of the ice sheet), but also appears to depict a short-term sensitivity rather than bulk feedback. The right half of equation 5 states that this feedback is: delta Snet / delta Tair. If the delta terms were derived from long-term change, rather than detrended anomalies, this equation would correctly give the total (long-term) albedo feedback, defined at the surface. Fig 11b clearly does not show the ratio of long-term deltas, because 11-year delta Snet and delta Tair are both positive over most of the ice sheet, and hence the feedback should also be positive. (delta Snet should be positive because of both decreasing albedo and increasing Sdown over much of the ice sheet).

response 5: see response 2

comment: The LEFT half of equation 5 is a flawed method for calculating the feedback because the product of trends in delta Sdown and (1 - delta alpha) will not yield the same answer as the trend in delta Snet (which is what you want). Instead, Snet should be calculated first, then the trend of this quantity determined (rather than multiplying the trends of the components that produce Snet).

response 6: see response 2 and we contend our formulation of albedo feedback is more insightful. Note that the Appendix now illustrates the effect of temporal detrend-

ing. New Appendix figure and a new main article Figure 5 also now illustrates the paired interannual regression upon which our formulation of albedo feedback is based. The main text now also includes more explicit description of our formulation of albedo feedback.

comment: The "sensitivity" shown in figure 11 is still potentially useful, and the authors provide an explanation for this phenomenon, where warmer years over the accumulation zone produce more snowfall which brightens the surface, at least in the short term. The long-term decline in accumulation-zone albedo (Fig 6), however, shows that other processes (e.g., increased snow metamorphism, as noted by the authors) dominate albedo change in the long-term. These dominant controls of albedo change must be accounted for in the quantification of albedo feedback.

response 7: see response 2

comment: Additionally, the authors partially attribute the long-term decline in accumulation zone albedo to *decreased* snowfall rates. Presumably, this snowfall decline has occurred against the backdrop of increasing temperature. Thus, the relationships between temperature anomalies/trends and snowfall anomalies/trends, and the subsequent impact on albedo sensitivity to temperature, require more thorough exploration and consistent explanation by the authors

response 8: see response 2

comment: Calculating the 11-year albedo feedback using trends in delta Snet and delta albedo, as explained above, would help to address some of these issues. The authors should reframe their current discussion of albedo feedback instead as a short-term sensitivity that includes some, but not all, of the processes relating albedo to temperature.

response 9: We now acknowledge that our definition of albedo feedback does not include lags. with text immediately after our Methods section equation for albedo feed-

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back.

comment: The analysis of albedo feedback is achieved using surface temperature from the regional climate model MAR. Why not use the reanalysis surface temperature instead for albedo feedback, since it is being combined with observed (MODIS) albedo? Does the MAR surface temperature produce a similar albedo sensitivity as ERA40 temperature? Additional methodological concerns related to interpretation of albedo data are listed below. In general, the methodological approaches could be explained and justified more precisely.

response 10: Two important reasons: 1.) MAR data are of significantly higher horizontal resolution, needed to resolve the narrow ablation zone. But, since the MAR interannual variability is driven by the ERA-INTERIM reanalysis, MAR and ERA-INTERIM show similar temperature changes over the ice sheet. Original text made this point: "MAR data are attractive for use here because they are available at higher spatial resolution than global re-analyses" and 2.) we validate and calibrate MAR surface air temperature, see Figure 3.

Additional comments:

comment: Section 2.2: I suggest mentioning the reasons for using MODIS product MOD10A1 instead of the albedo product MOD43C3 or MCD43C3. Also, why not use combined Aqua and Terra data, instead of only Terra data? This would average out some of the diurnal variability in albedo, which may or may not be significant.

response 11: a new sentence in Section 2.2 now reads "The MOD10A1 daily product is chosen instead of MOD43 or MCD43 8-day products to increase temporal resolution."

comment: p597,3: awkward wording

response 12: Agree. The text has been clarified and now reads: "Before computing monthly averages, the GC-Net $T_{\rm air}$ data are quality controlled by rejecting hourly data for which there is a disagreement $>$\,0.5\,K between the thermocouples

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and the thermisters."

comment: p597,11-28: Are the in situ measurements of incoming and reflected shortwave radiation corrected for station tilt (van den Broeke et al.,2004)?

response 13: We incorporate the same kind of tilt error minimization as van den Broeke et al. (2004). We total the solar data on a monthly basis which again allows this scheme to avoid the albedo ratio blowing up. See equation 1 and associated text.

comment: These measurements will also contain bias when a thin snow cover is present on the upward sensor dome and Sdown is still greater than Sup.

response 14: We acknowledge that there are other sources of error than station tilt with a new sentence that reads: "Other error sources than station tilt include, for example, bias when a thin snow cover is present on the upward sensor surface and S_down is still greater than S_up " are not compensated here."

comment: p597,12-14: Julian dates are mixed with monthly dates

response 15: Days of the year are converted to calendar dates.

comment: p598,1-4: Is this not accounted for when looking at the MOD10A1 quality flags (Schaaf et al., 2011)?

response 16: We don't use the quality flags to limit the data by SZA. Instead, "we limit these data by focusing on the June–August period when SZA is minimized..."

comment: Equation 1: Indeed, this is how any temporal average of albedo should be calculated(assuming it includes paired samples of SËĘup and SËĘdown).

response 17: Yes, it includes paired samples of SËĘup and SËĘdown. The text now states this.

comment: p599,14: "MODIS albedo algorithm does not provide a surface albedo estimate under cloudy skies" - This implies that you only use clear-sky remote sensing

retrievals. When averaging the GC-Net observations for comparison, do you also average these only under clear-sky conditions? Not doing so would seem to substantially bias the monthly-mean comparison with MODIS data, since surface albedo is higher under cloudy conditions.

response 18: New text now describes in more detail the application of an offset to compensate the 0.035 bias already mentioned 596-20 and how it is applied uniformly to GC-Net data to account for the LI-COR pyranometer bias. The data section "2.3 MODIS MOD10A1 validation" new text reads:

GC-Net albedos are measured under all-sky conditions while MODIS MOD10A1 data are retrieved exclusively for clear-sky conditions. All-sky GC-Net albedo data will thus include the effect of clouds; increasing calculated albedo because of cloudy cases. Consulting Fig 1 in which no significant bias is found between the all-sky GC-Net data and the MOD10A1 data, the effect of cloudiness on GC-Net data seems within the residual uncertainty between these independent data sets. Otherwise, there may be some offset in the absolute accuracy of the MOD10A1 data. Yet, that offset was not apparent in the Stroeve et al. (2006) comparisons in which the GC-Net data were selected using a cloud clearing methodology (Box, 1997). In either case, the absolute bias is less than the RMSE and here is not compensated further.

Box, J. E. (1997). Polar day effective cloud opacity in the Arctic from measured and modeled solar radiation fluxes, MA thesis, University of Colorado, Boulder, Colorado.

comment: Section 2.4: Which quality flag threshold is applied to filter the MODIS data? Some quality filtering is needed.

response 19: We don't use the quality flags to limit the data by SZA. Instead, "we limit these data by focusing on the June–August period when SZA is minimized when the albedo retrievals are made.

comment: p599,22: Why apply a strict minimum albedo threshold of 0.31 when lower

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values were measured at the GC-Net sites?

response 20: What was written is unclear. We don't limit MODIS data to 0.31. The revised text now reads: "Minimum ice sheet albedo values in the MOD10A1 product over the part of Greenland that our land mask classifies the surface as permanent ice are 0.31. It is important to be explicit about the use of a mask because some lower albedo debris-covered area may be excluded."

comment: p600,16: What is the justification for stating that the MAR data resolution is "just sufficient to resolve spatial gradients in Tair and SËĘdown"?

response 21: "just" is removed and a citation is added "MAR data are attractive for use here because they are available at higher spatial resolution than global re-analyses, sufficient to resolve spatial gradients (Franco et al. 2012) in Tair and S âĘŞ."

comment: p600,27: "with absolute accuracy" - Is there a specific accuracy that is being targeted?

response 22: "absolute" is replaced with "maximum"

comment: p603,12: Wording

response 23: The sentence has been broken into two to increase clarity.

comment: Equation 4: Earlier (p601,14) it is stated that "energy fluxes that heat the surface are positive in this budget". Thus Equation 4 is inconsistent with previous text and Equation 2.

response 24: Equation 2 is now correctly balanced. Sign convention is now consistent.

comment: p604,1-4: This approach is unclear. If only melt-extent (and not melt volume) is determined from the microwave sensing, how is melt volume partitioned to Snet? Second, how is the partitioning to Snet accomplished? Partitioning seems somewhat arbitrary since it is net surface energy that governs the melt, and Snet is often greater than net surface energy. I assume it is the anomalies that are used. Please elaborate

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on this approach.

response 25: At the top of page 604 in the discussion paper, a new sentence is inserted in the relevant text, to add clarity:

Passive microwave-derived melt extent (Tedesco, 2007) permits defining surface melt duration (delta T) such that melt volume attributable to Snet may be partitioned using time between melt volume attributable to other SEB terms. The precision of the melt duration calculation is 1 day. While partitioning in this way is somewhat arbitrary since it is the net surface energy that governs M, and Snet may be greater than net surface energy for periods shorter than three months, the change in the ratio of melt volume attributable to Snet, expressed as, Snet Δt(Lf)$-$1/M, yields the temporal change in relative magnitude of Snet melting to total melting and indirectly the changing importance of albedo feedback to M. This attribution to albedo feedback ignores the relatively small changes in S âĘŞ.

comment: p604,4-9: Elaboration on the methodology for computing the albedo sensitivity and feedback is needed.

response 26: Additional new text and a new figure 5 illustrates the concept better. I apologize the lack of clarity in the submitted version lead to the review hanging on this definition. Albedo feedback, as it is now more clear, I expect, is defined clearly now.

comment: p605,9: I would assume October albedo measurements over Greenland have extremely low confidence since the solar zenith angle is extremely high during this month at these latitudes.

response 27: Agree, this is why we consider only June-August data in our main analysis. Anyway, a new sentence follows the sentence you bring into question, and reads: “Though October albedo measurements over Greenland have extremely low confidence since the solar zenith angle is extremely high during this month at these latitudes. “

comment: p605,25: A 90 or 95% (2 sigma) confidence interval should be used instead of the 68% confidence interval (1 sigma) used here.

response 28: We stay with the 68% confidence interval (1 sigma) even though its optimistic and leave it to the reader to decide if that confidence interval satisfies them.

comment: section 4.2: Here and in Table 1 it might be worthwhile reminding the reader that the MODIS data for each location are actually a 15km(?) area surrounding the station point.

response 29: We don't use a 15 km area surrounding the station point. Instead, we use the nearest 5 km grid cell. New text in this section reads: "Comparisons are made with the nearest MODIS 5 km grid cell."

comment: p606,13: optical optical

response 30: now only one optical is given. Thanks.

comment: p606 L26-29: It appears that the interannual variability between GC-Net and MOD10A1 are not significantly correlated (r = 0.7 in 16 of 25 cases with a very small N)! This is critical in establishing that what MODIS observes on an interannual scale is also observed by GC-Net. High correlation coefficients presented in 4.1 indicate that MODIS and GC-NET both capture a seasonal pattern but not necessarily interannual changes.

response 31: The text reads: "correlation coefficients above 0.7 in 16 of 25". New text now reads: "For example, in June, at JAR1 in the ablation area the correlation is 0.938. At Saddle where there is little to no evidence of surface melting in June, the correlation is 0.919." In the dry snow area (i.e., Summit) where melt is rare if occurring at all, the albedo is relatively constant though the year and the interannual variability is low. In this case, the uncertainties in the measurements explain why low correlations can occur for the AWS's.

comment: p608,5: reword or clarify the meaning of "In the likely example".

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response 32: The text now reads: “In an example with realistic boundary conditions, ”

comment: p607,9: statistical significance needs to be established for the accumulation area trend

response 33: Agreed. New text reads: “The residuals to the linear fit for the accumulation area are small (0.006), suggesting a highly significant declining trend.”

comment: p608,1-12: Can the cold content not be extracted from MAR, instead of making the assumptions listed?

response 34: Only a “realistic example” is given instead of needing to also validate the MAR land surface model.

comment: p608,22: Can original sources be cited instead of the "Arctic Report Card executive summary"?

response 35: To my knowledge and after a search now, it is only the Arctic Report Card and work therein that concludes the shift in the Arctic ocean system. Arctic Report Card is anyway not “grey literature” it undergoes an anonymous external review.

comment: p609: The second paragraph includes repetitive statements and inconsistencies. First, it is mentioned that snowfall decreases in the ablation zone because of increasing rain/snow ratio with increasing T. Later, the role of "summer snowfall on increasing surface albedo in the ablation area" is mentioned.

response 36: It is not exclusive that rain to snow ratio does increase in a warming climate and snowfall does increase ablation area albedo. Anyway, it should be more clear with the following new text ...”Over the ablation area there is a negative correlation between snowfall and $T_{\rm air}$ anomalies because MAR correctly simulates precipitation falling as rain instead of snow in warm summers.”

comment: p610,5: "...suggesting that during warm years, the albedo increases." - As described above, this has to be reconciled with the long-term albedo decline that has

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occurred with long-term warming.

response 37: The issues of looking at albedo sensitivity and feedback using anomalies has been addressed.

comment: section 5.2: This very short sub-section seems out of place for Conclusions.

response 38: the sub sections clearly distinguish the topic they cover and it is worth mentioning what conclusions were reached about MAR.

comment: Section 5.4: It would be helpful to report albedo feedback averaged over the ablation and accumulation zones.

response 39: Agreed. Averages over the ablation and accumulation areas are now reported in the main text and conclusions.

comment: Table 2: Mention the source of these data.

response 40: The Table 2 caption now includes the following new text: "Ice sheet summer (June–August) surface climate variability 2000–2011 from the MAR regional climate model and MODIS MOD10A1 data partitioned among ablation and accumulation areas by annual surface mass balance."

comment: Fig 1: Mention what area the MODIS data are averaged over (e.g., single pixel, 15km averages, etc).

response 41: The Fig. 1 caption now includes the following new text: "Comparison of the nearest monthly averaged MODIS MOD10A1 5 km grid cell albedo values"

comment: Fig 2: Why does this analysis only extend from 2000-2005, when all other analyses extend through 2010 or 2011 (e.g., Fig 3 which also compares MAR data with GCNet)?

response 42: This was a residual from an older analysis. Indeed, the comparison is through 2010. The caption text has been corrected.

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comment: Fig 6: It would be helpful to add a subfigure showing the change in net shortwave energy at the surface

response 43: Appendix Fig. A11. now illustrates the “Summer spatial patterns of MAR simulated Snet change 2000-2011.”

comment: Fig 10b: Expand/clarify the caption describing this figure.

response 44: It is now Fig 11b. And the caption now has more specifics, reading: “Summer (June–August) spatial patterns of the correlation between 5km averaged MODIS MOD10A1 albedo and 25 km averaged MAR MAR T_air data.

comment: Fig 11: Should not be labeled an albedo feedback if it is calculated with zero-lag de-trended anomalies. (See earlier explanation).

response 45: see response 3.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C532/2012/tcd-6-C532-2012-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 6, 593, 2012.