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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.

Anonymous Referee #1

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This study applies in-situ and remote sensing observations to quantify trends in albedo of the Greenland Ice Sheet. Finding large reductions in albedo over much of the ice sheet, the authors identify meteorological and radiative causes of the reduced albedo, and also quantify an albedo feedback. The data reported in this study are important and potentially useful. I believe, however, that the interpretation and quantification of "albedo feedback" are seriously flawed. There are also methodological concerns related to filtering of MODIS data for comparison with GC-Net data and associated significance/uncertainty evaluation. The discussion also comes across as a bit disjointed.

It appears that the authors quantify a near-instantaneous "sensitivity" of albedo anomalies to local temperature anomalies, achieved through regression of *detrended* albedo and temperature anomalies, and equate this with "albedo feedback". This sensitivity

is shown in Fig. 11 (and is more appropriately labeled a "sensitivity" in the caption for figure 11a). Using this analysis, the authors conclude that "the albedo feedback is negative over 70% of the ice sheet" (abstract). However, the 11-year albedo change is negative over nearly the entire ice sheet (Fig. 6a). 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. Thus, the 11-year albedo feedback must be positive over most of the ice sheet! Clearly processes other than those determining correlation (at zero lag) between albedo and temperature anomalies are driving long-term albedo change, and these processes must be accounted for in the quantification of albedo feedback.

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).

Additionally, 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).

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.

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

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.

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.

Additional comments:

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.

p597,3: awkward wording

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

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

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

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

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.

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

p599,22: Why apply a strict minimum albedo threshold of 0.31 when lower values were measured at the GC-Net sites?

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

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

p603,12: Wording

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.

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 on this approach.

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

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.

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

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.

p606,13: optical optical

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.

p607,5: reword or clarify the meaning of "In the likely example".

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

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

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

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.

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 occurred with long-term warming.

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

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

Table 2: Mention the source of these data.

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

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 GC-Net)?

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

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

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

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