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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.

J. E. Box et al.

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responses to: Anonymous Referee #2, review 6, C314–C322, 2012

Note in the rebuttal below;

1. In responses, "I" refers to J.E. Box

2. Text from the submitted paper is pasted in, sometimes without the exact formatting (LaTex superscripts, subscripts, Greek characters are spelled out, for example alpha or delta) that is in the submitted paper. The proper formatting will, of course be made in the re-submitted paper.

comment: I see a_bulk as a first order feedback and both a_* and a_a as second order

feedbacks

response: I agree and now include text to that effect, including revised Abstract text that now reads: "We find that while albedo feedback defined by the change in insolation and temperature \emph{over time} is positive over 97{\%} of the ice sheet, when defined using paired annual anomalies, a negative feedback is evident over 63{\%} of the accumulation area. This second order negative feedback damps the accumulation area response to warming due to a positive correlation between snowfall and surface air temperature anomalies."

In the methods section I now clarify the first and second order aspects with:

"While feedback;b may more explicitly represent the energy system to the first order, computing feedback from pairing of anomalies (Fig. 5) yields a insight into the importance of, for example, snowfall in a second order role of modulating albedo sensitivity to Tair.

In the results section "4.5 Ice sheet albedo sensitivity to surface air temperature", the process is more accurately defined with additional words that refer to anomalies and the second order process:

"Over of the ablation area, the 2000–2011 albedo anomalies exhibit a negative correlation with the Tair anomalies (Fig. 11b), suggesting that in anomalously warm periods years, the ice sheet albedo is anomalously low. In contrast, a positive correlation is evident over much of the higher elevation accumulation area, suggesting that during anomalously warm periods, the albedo increases. While a positive α* may be counterintuitive, it's realism as a second order process is consistent with the positive correlation signal between anomalies in Tair and snowfall (Fig. 10a).

A sentence in the conclusions section reads:

"Evidence of a second order accumulation area negative feedback is supported by observation of a positive correlation between snowfall and surface air temperature anomalies in this region. "

comment: I am very skeptical about the conclusions they draw from a_* and a_a.

response: I am glad reviewer 2 raises this issue further because addressing the issue has improved our paper. See the next comment and its response.

comment: there is just too much uncertainty in the data to trust the second order effects or deviations from the trend.

response: The reviewer evokes problematic MODIS MOD10A1 land surface temperature (LST) data into the critique. I initially used MODIS MOD10A1 LST data and subsequently abandoned it. I now bring this aspect back into the paper for readers benefit beginning with a new Data section: "2.3 Land Surface Temperature"

New section 4.5 results text reads: "When MAR Tair are replaced with clear-sky retrieved MODIS MOD11A1 Tsurface, the positive (and negative) α* regions are of smaller magnitude. Though 22 % of the accumulation area still registers positive α* values, the high uncertainty of the associated shallow trend is neces- sary to acknowledge. Using Tsurface, only 5 % of the accumulation area has α* values above +0.3 % K$-$1. For two reasons Tsurface is less sensitive to positive thermal perturbations than the all-sky Tair data: 1.) Tsurface variability near and above 0 âŮęC is capped by the latent heat sink of melting; and 2.) Comparisons with GC-Net AWS indicate that MODIS MOD11A1 Tsurface is consistently lower than Tair. Tsurface is more representative of cold perturbations to the surface because when Tsurface is retrieved by MODIS, the sky must be clear. Clear sky conditions are more often associated with surface radiative cooling than all–sky conditions. Using MAR Tair instead of the Tsurface, roughly twice, or 46 % (25 %) of the accumulation area registers values above 0.0 % K$-$1 (+0.3 % K$-$1), respectively.

The conclusions section now includes the new statement:

"When MODIS MOD11A1 land surface temperature data are used to evaluate temper-

ature sensitivity, as an alternative to near-surface air temperature data from MAR, a weaker response is evident. [See Appendix Fig. A4, attached. Compare Appendix Figs. A2 and A4.] Surface temperature variability represent relatively low surface and air temperature clear sky conditions. Further, surface temperature is capped near and above 0âŮęC. We thus conclude that surface temperature data are less desirable for the assessment of albedo sensitivity than all–sky near-surface air temperature data provided by in-situ observations or calibrated regional climate model output.

Thus it may be true that a less certain albedo sensitivity is found when using clear sky land surface temperature to represent the effects of temperature on albedo, it's lower sensitivity leads the reviewer to the conclusion that there is too high uncertainty. Using instead near-surface air temperature (that is calibrated to be more absolutely accurate using GC-Net data), we find a higher sensitivity that makes our analysis sufficiently certain to detect a positive sensitivity in anomaly space and yes, it is second order, but that doesn't mean it's useless nor hopeless.

comment: Significance of a_* and/or a_a? ... the authors never show any R2 or significance test for a_* or a_a, so I really question how statistically important they are and if they really "reveal an interplay of physical mechanisms"

response: It is a valid critique to suggest that we should examine statistical significance. Therefore, a "confidence" metric is now incorporated, defined as 1-p*100%, where p is the typical probability of false positive assessment that the trend is not equal to zero. By subtracting, p from 1.0, the metric is a bit easier to follow as the "confidence the trend is not zero". While it's generous to make this test with a small sample, the fact that a Student's t-distribution is used, designed for small population distributions, it is not hopeless. We use the 80% confidence threshold to assess significance.

See the revised attached figure 13.

The Figure 13 figure caption now includes a final sentence that reads: "Stippled areas have trends that are not statistically significant above 80% confidence."

The main text now includes discussion associated with Figs. 13a,b that reads:

“Positive * that exceeds 80% confidence occurs over 11% of the accumulation area. At or above 95% confidence, that area fraction is 3 %.”

and the signal is stronger for albedo feedback evidently because of some support from the downward shortwave data. The text now includes:

“Negative albedo feedback that exceeds 80% confidence occurs over 26% of the accumulation area. At or above 95% confidence, that area fraction is 9 %. Given that this fraction exceeds the statistical Type-1 Error margin (20% or 5 %, respectively), by nearly a factor of 2 in the case of the trends at 95% confidence, the conclusion of statistically significant association is robust, albeit over 9% of the accumulation area.

comment: “Statistical[y] significant conclusions?”Although I can completely agree with the authors hypothesis of increased albedo due to increased snowfall on the interior of the GrIS, I just don’t agree that the data are showing it on a statistically significant way.

response: I expect that a.) the previous response among the other considerations of b.) the spatial coherence of the positive sensitivity or negative feedback, c.) the correlation between snowfall and temperature anomalies, and d.) physical mechanism link with ocean evaporation are sufficiently satisfactory to not “torpedo” the paper.

The reviewer’s skepticism understandably was maintained by the misleading use of clear-sky-derived and therefore relatively cold and insensitive to melt land surface temperature instead of all-sky air temperature.

Minor Comments

comment: the manuscript [is] written in a careless and unsystematic way,

response: I’ll be defensive: On the contrary, I can assure you that much care was taken and the paper is not presented randomly. I have gotten very supportive statements from co-authors and other readers of the manuscript. This claim from the reviewer is

obviously hyperbolic and indicates frustration which I can understand.

comment: response to reviewer 2 (they are not numbered): "The key difference is that comparisons are not made when MODIS albedos are above 0.84, a threshold established from maximum clear sky snow albedo of 0.84 (Konzelmann 1995). This has a filtering effect on the GC-Net data because the statistics only consider paired (GC-Net and MODIS comparisons).": you should mention that in the text and not only in the caption.

response: After the maximum clear-sky albedo value is established in the manuscript being after the (Konzelmann 1995) meansurements, the text now includes:

“Comparisons are not made when MODIS albedos are above 0.84.”

comment: specific comment 3, critique of nearest neighbor.

response: I understand your criticism. Unnecessary noise and a weak sampling strategy would be a shame. I have verified that indeed we are “Averaging MODIS data within 10 km of the GC-Net AWS location”.

Further, I earlier was mistaken in how I described how our data are regridded. For this I am sorry and at the same time very happy the reviewer pressed about this point. In fact, we don’t use a nearest neighbor interpolation from the 500 m MOD10A1 data to the 5 km grid of this study. We use bilinear interpolation. The manuscript now accurately states:

“The data are interpolated to a 5 km Equal Area Scalable Earth (EASE) grid using the NSIDC regrid utility (http://nsidc.org/data/modis/ms2gt/). The interpolation method employs a bilinear trend surface through the surrounding four closest 500 m grid cells.

comment: 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.": this is just complete unscientific. These QA flags have been developed to assure quality. If you neglect them and just limit based on SZA, then you should at least show that this produces also good quality.

response: To the last sentence, the paper clearly shows a respectable agreement with in-situ observations. Figure 1 is an example. The results and discussion of Table 1 are another example of how we establish data quality.

comment: response 29: response 29 + 41: "We don't use a 15 km area surrounding the station point. Instead, we use the nearest 5 km grid cell.: so why you state on p.7 "Averaging MODIS data within 10 km of the GC-Net AWS location yields RMSE values of ...? This is just contradicting!

response: I have verified that indeed we are "Averaging MODIS data within 10 km of the GC-Net AWS location". I corrected the text that mentions nearest grid.

comment: response 11: "The MOD10A1 daily product is chosen instead of MOD43 or MCD43 8-day products to increase temporal resolution": if you use 11-day running statistics then you do not increase temporal resolution a lot, unless you consider 11 days much better than 16 days?

response: Touché. And as stated in the text: "The daily MOD10A1 product is chosen instead of the MODIS MOD43 or MCD43 8-day products to *increase* temporal resolution." emphasis added.

comment: response 27: "we consider only June-August data in our main analysis": so then leave all the rest out. Discussing a trend for October albedo (p13-14), which have extremely low confidence is just saying look at it, but actually you should not look at it. This confuses the reader and creates false conclusions, certainly for readers that are not familiar with the data.

response: it's not unfair nor confusing to the astute reader (like this reviewer) to mention May or October when I make clear "we consider only June-August data in our main analysis". About October, I would say "lower" confidence rather than extremely low confidence. It's clear something real is going on because the October 2010 anomalous

low is connected to the earlier low albedo record setting melt year. See August and September of the same year.

comment: 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": this is just dangerous as it gives a false impression of significance, because not all readers have always the background knowledge to correctly draw such conclusion.

response: "False impression" is not an entirely fair charge. 68% confidence is "likely" in my view.

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

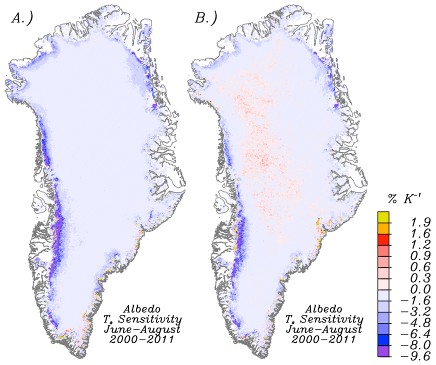

Fig. A4. Same as Fig. A2, but using MODIS MOD11A1 land surface temperature instead of near-surface air temperatures calculated by MAR.

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Fig. 1.

Effect of Temporal Detrending on Calculating Albedo Sensitivity

Temporal detrending prior to anomaly calculation leads to differences in calculated albedo sensitivity to temperature and albedo feedback. Box et al. (TCD, 2012) feature the detrended results because they believe the detrended results more closely represent the sensitivity and feedback processes.

Detrending enhances the positive sensitivity while decreasing the negative sensitivity. The reason for this has to do with the fact that the albedo decreasing trend is stronger than the temperature increasing trend. Nonetheless, evidence of positive sensitivity remains in the non-detrended data.

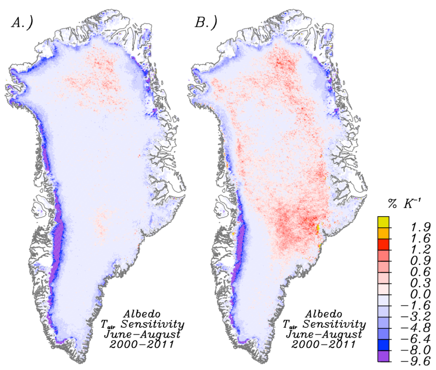

Fig. A2. (A) (a) summer spatial patterns of ice sheet albedo sensitivity based on MODIS albedo observations and MAR simulations of T_{air} (B). Same as A but with temporal detrending of albedo and temperature prior to regression. The positive scale is 1/3rd that of the positive scale. The regressions are detrended to minimize spurious correlation.

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Fig. 2.

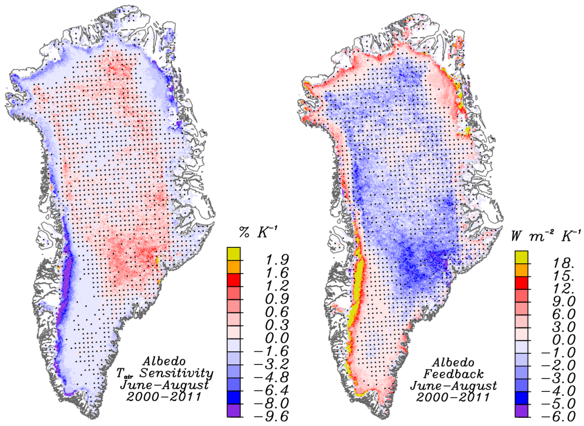

Fig. 13. **(a)** summer (JJA) spatial patterns of MODIS MOD10A1 albedo sensitivity to the MAR simulated surface air temperature. The positive scale is 1/5th that of the negative scale. **(b)** summer spatial patterns of ice sheet albedo feedback based on MODIS albedo observations and calibrated MAR simulations of $S\downarrow$, and T_{air}. The negative albedo feedback scale is 1/3rd that of the positive scale. The regressions are detrended to minimize spurious correlation. Stippled areas have trends that are not statistically significant above 80% confidence.

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Fig. 3.