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

Please note in the rebuttal below; In responses, "I" refers to J.E. Box 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: based on the albedo, S, and T they also determine what they call the albedo sensitivity (i.e., regression between 12 annual samples of detrended anomalies of June–August average albedo and T) and albedo feedback (i.e., not completely clear

in the text how to calculate it, but I assume regression of Snet / T (i.e., the trends in Snet and T over time?).

response: 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 feedback 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.

I regret the explanation of how was not more clear. The regression is not over time. New sentences below are inserted in the relevant paragraphs surrounding equation 6. Please see the revised text.

comment: The authors use the SSMI melt extent to partition melt volumes (although the methodology to do so remains unclear).

response: new paragraphs above and below the new equation 5 clarify the methodology. Please see the revised text.

"Major comments"

"Reproducibility of GC-net albedo trends"

comment: I tried to verify their analysis using the processing steps the authors describe in their paper ... I used quality controlled GC-net data received from N. Bayou dating from October 2011, but my data set is limited to fewer stations ... I have found significant discrepancies between my results and the results the authors present, which possibly could torpedo the the paper. ...Possible causes for the difference between my results and the paper's result, can be found that the authors [b] not properly explain their used methodology or [a] that they have a different GC-net data set.

response: The length to which the reviewer has gone to test our trend verification is admirable.

With regard to comment [a], it is true that I am using a different version of the GC-Net data than the reviewer. I have developed my own quality controlled version of the GC-Net shortwave data after having carefully installed and maintained the GC-Net stations for 11 years (1995-2005). The main difference in the data we are using would be in the data before year 2006. I use my own quality controlled data 2000-2005. It is likely we use the same data 2006-present, as I have received these from K. Steffen in the past year.

But it is not only the difference in the GC-Net data sets uses in which the differences are probably minor. The key difference I expect 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). So the reviewer comment "I am missing some processing steps" is true and important here.

Notice in Fig. 1 caption, it is written: "Fig. 1. Comparison of monthly averaged MODIS MOD10A1 albedo values less than 0.84 with observations from 17 GC-Net automatic weather stations.". So, I am being specific here.

Because we assume 0.84 is the maximum albedo value under clear skies. We do not consider values above this. MODIS MOD10A1 includes albedos above 0.99, most common in extreme northern Greenland, evidence of an inaccurate anisotropic reflectance correction for extremely high solar zenith angles. While our validation study includes May-September, we ultimately avoid dealing with the most highly uncertain albedo values by analyzing albedo sensitivity and feedback only June-August and excluding albedo above the reasonable clear sky value of 0.84. The text of the submitted paper deals with this rationale at length. See page 599.

[b] The reviewer takes the time to re-write our methodology. I confirm the reviewer understands how this paper has computed monthly albedo. One missing piece, that is

key, is the filtering effect of excluding MODIS values above the valid threshold of 0.84.

Also, notice that despite: a.) increasing GC-Net trends at some sites, for example, CP1 (The CP1 data is not "corrected") and b.) no gap-free time series; high correlations between GC-Net and MODIS MOD10A1 albedo are evident in the submitted and unaltered Table 1. This high correlation pattern is evident at the majority of sites. The sites are independent. Because of the high correlations between GC-Net and MODIS MOD10A1, it is convincing that the GC-Net albedo and MODIS MOD10A1 albedo measure a very similar climate signal, that is, not only a decreasing trend for valid pairs but inter-annually (high correlation)! The manuscript includes this rationale.

comment: page C316, bullet 2; The reviewer questions whether it is possible a reduction in RMSE by nearly a factor of two in comparison with Stroeve et. al. (2006) is obtained due to monthly aggregation and smoothing/filtering.

response: Again, firstly, comparisons are not made when MODIS albedos are above 0.84. This has a filtering effect on the GC-Net data because the statistics only consider paired (GC-Net and MODIS comparisons).

Secondly, The monthly RMSE values from Fig. 1 for the months June-August are averaged to obtain the RMSE value specified: 0.041 ± 0.011. The ±0.011 refers to the spread in monthly RMSE values.

Note also onward from line 5 on page 598 referring to the larger RMSE value reported by Stroeve et al. (2006) that, as stated in the paper, "We now understand that a dominant component of this assessed error is the failure of the MODIS data product to completely remove cloud effects."

comment: Negative feedback for 70% of the GrIS. How is it possible that over the period 2000-2011, when the albedo lowered over the entire GrIS (Fig.6a, Table 2), S increased (Table 2, Fig6b that shows also local decreases in some areas but not spatially consistent with Fig.11b) and T increased (No spatial pattern presented but general increase in Table 2), the authors still have a negative feedback for 70% of the area?

response: see review 1 response 2. and...

A new illustration in the new Appendix A (Figure A1) and new text above that helps clarify the concept by which we compute albedo sensitivity and feedback. Another useful new comparison related to this comment, finds negative feedback also when considering MAR-calculated albedo in place of MODIS albedo. Therefore, if both models and satellite show this, having negative feedback is not an artifact of our methodology. See new Figure A5 and related new discussion in the Appendix B.

What's happening is that the observed increasing melt is despite the negative temperature-albedo feedback. Without the negative temperature-albedo feedback, the increase in melting would be even greater! The paper makes the point that the negative temperature-albedo feedback helps maintain the ice sheet.

New text in the Appendix reads: When MODIS albedo are replaced with albedo calculations from MAR (Figs A6a,b), the negative feedback areas are stronger in magnitude. The core of the negative correlation is co-located where MAR output indicates the strongest correlation between snowfall and air temperature (Fig. 10a), indicating increased snowfall with increasing temperature is a key source of the negative feedback. The effect of temporal detrending has a weaker effect when using only MAR data.

It is also is worth mention that temporal detrending influences the magnitude of the calculated positive and negative albedo sensitivity and feedback. The new Appendix includes figures that illustrates the impact. In the main paper, as we state, we feature the detrended results instead of the not-detrended results because we expect the detrended results more closely represent the sensitivity and feedback processes.

comment: I did not completely understand what the authors quantify as feedback, as it is not very clear in the text how it is calculated. response: Additional text is included

to be more explicit about how the regressions are applied to quantify albedo sensitivity to temperature and albedo feedback with insolation and temperature. New statements include:

To be clear, the regression is between annual pairs of anomalies rather than values in a time series.

The evidence of negative albedo feedback at high elevations should not give the impression that with warming the albedo increases in the accumulation area. Rather, the negative feedback means that the accumulation area albedo has a damped response to warming.

Further, as stated above, "A new illustration in the main article (Figure 5) and figure 1 in the new Appendix A and new text helps clarify the concept by which we compute albedo sensitivity and feedback.

In addition, we compute a bulk albedo feedback, over time. See the new Figure 12.

comment: Stating that in warmer years, the albedo increases (p610-5) and that the accumulation area gains in brightness during warmer years (p610-21) is somewhat misleading.

response: Actually, it is the discovery and therefore is not misleading. It's just counter-intuitive until understanding the significance of (Fig. 10a) and new Appendix B text and figures such as those related to main article Figs. 11-13 and Appendix Figs. A5 and A6.

comment: pg C318 bullet 2, "if albedo feedback is defined as..."

response: The alternative formulation of albedo feedback that you suggest is now made here referred to as a bulk feedback throughout this study.

"Specific comments"

comment: 1. p.595-28: Stroeve (2001) discusses AVHRR data, whereas several more recent papers (even by the same author) use MODIS data, which the authors also use in their manuscript. Reference to these MODIS papers therefore is certainly useful.

response: 2. This paragraph reviews significant albedo climatology findings from past publications. The later Stroeve papers were concerned with albedo product validation and are mentioned in the discussion paper data section 596-20, for example.

comment: p596-7: Mention which MODIS data set was used in Box et al (2006) as large differences might occur between MODIS products.

response: The Liang et al. (2005) data set is now mentioned.

comment: 3. p597-19 + 599-6: the nearest neighbor algorithm only resamples one 500m pixel to a 5km pixel. In combination with a 10km radius this results in MOD10A1 values derived from 25 MOD10A1 pixels (2 border pixels (10km) + central pixel = 5x5 pixels = 25 pixels), whereas using the mean of 20x20km=40x40pixels=1600 pixels would be much more accurate. I therefore recommend another resampling approach (e.g. mean value per resampled pixel) which certainly is less error prone.

response: we understand your recommendation but keep our approach which has an advantage of more precise co-location of MODIS albedo data with GC-Net AWS data in comparisons we make.

comment: 4. p598-3: only June-August data are mentioned here, but given that SZA is the highest in June, April-May data should be as accurate as July-August, so it is certainly useful to include these in the analysis too for completeness.

response: SZA is lowest in June not highest. I think you mean sun angle there. The recommendation to include more months, April-May is worthwhile for another study that does not just focus on the key melt season months. June-August comprise >80% of the surface melt water production, according to MAR. There is little melting April-May.

comment: 5. p598-3: only June-August data are mentioned here, but for some analyses also May is included (Figs1-3). Try to be consistent throughout the article in order

not to confuse the reader.

response: This is not a bad recommendation. However, Figs. 1-3 will not be changed because the careful reader will notice that the paper later focuses on the melt period June-August.

comment: 6. p598: which quality flag was used for the processing of the MOD10A1? See also p599-18.

response: The quality flags are not used to parse the data. Instead, we process the data as described in the paper in which we use an 11-day running average, as specified in the paper, discussion paper pg 598.

comment: 7. p599: MOD10A1 is assumed to be clear sky albedo, whereas no clear sky analysis for the GC-net data is performed. Consequently, GC-net albedo certainly will be biased (higher albedo for cloudy moments).

response: 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: 8. p600-2.4: add introduction why the MAR data are included in this manuscript.

response: This section 2.4 now leads with new text that reads: “A source of climate parameters distributed over all of the Greenland ice sheet is needed in this study to evaluate the albedo sensitivity and feedback.

comment: 9. p600-17: just sufficient to resolve spatial gradients : explain and argument.

response: The text now reads: “just sufficient to resolve mesoscale spatial gradients in”

comment: 10. p601-13: additional MAR variables used in this study: be complete as also precipitation is used but not discussed here.

response: The text now reads: “just sufficient to resolve mesoscale spatial gradients in SâĘŞ, Tair, precipitation, and several other variables mentioned later.”

comment: 11. Eq.4: what is RN? It is never mentioned in the text.

response: 603-17. RN is now defined in the following new text: “RN is the net radiation (Lnet + Snet),”

comment: 12. p604-firstParagraph + p607-20: explain how this partitioning is done.

response: See new text in the paragraphs immediately before and after the new equation 5.

comment: 13. p603: albedo sensitivity and feedback: elaborate on how to calculate. For example, it is unclear what Snet and T mean and how to calculate them.

response: albedo sensitivity and feedback now have new and additional description in

the paragraph before equation 6 and in the new Appendix.

comment: 14. p603: sensitivity (i.e., regression between 12 annual samples of detrended anomalies of June–August average albedo and T) at zero lag is a dubious concept when detrended series are used and the trend contains high RMSE values. For example, locations with a negative albedo trend for increasing T (i.e., albedo decreases with increasing T) can show a positive detrended albedo anomaly values.

As such, it seems that albedo increases with T which is not the case. The sensitivity or detrended anomaly only shows that the decrease/increase in albedo is lower/higher than one would expect using the observed trends. This however says nothing about real sensitivity, nor does it explain any causality.

response: In response to “sensitivity at zero lag is a dubious concept when detrended series and the trend contains high RMSE values”, the reviewer is referred to the elevation profiles (Figures A4) which by averaging over area may increase confidence. If that is insufficient, we also include new Figures A6 that reinforce the notion that the negative feedback is not the result of a problem with the MODIS data in that MAR reproduces the negative feedback, more strongly, with its own parameterized albedo. If the reviewer would rather examine non-detrended results, those are now included as well throughout the Appendices.

The second part of the reviewers comment “albedo increases with T which is not the case”. Figure 11b illustrates that in anomaly space, albedo increases with increasing air temperature. Additional Appendix figures 8 and 9 further support the notion that albedo perturbations are positive in warm years, as does Fig 10a. What we present is the real sensitivity. Comparing the time series and finding a declining albedo trend is not at odds with our result.

As to the issue of whether at lag-zero the results are meaningless, a 3 month summer period is sufficient for albedo to respond to temperature and snowfall perturbations within that period. While, we do acknowledge that some lag effects are missed by out approach with new text immediately after equation 6 that reads: “This definition of albedo feedback does not include lags which may contribute to albedo change. Indeed, there is some evidence discussed relative to Fig. 6 whereby residual low year 2010 albedo may have pre-conditioned year 2011 for low albedo

Causality is inferred in numerous places throughout the text such as in discussion related to, especially Figs. 8 and 9 and Table 2. See section “4.4 Important role of NAO in 2000–2011 surface climate trends”

comment: 15. p603: sensitivity/feedback assumes a sort of causality, but if use zero lag correlations are used, this causality is never captured as it is just a snapshot of things happening together.

response: The 3 month period considered is sufficiently long that it is not a snapshot. There is sufficient time in this season, for example, for albedo to respond to temperature. The correlations illustrated in Figs 11a,b make clear that there is a cause-effect relationship, clearly over the ablation area; more snow leads to higher albedo or higher temperature leads to lower albedo. It may be interesting to examine the effect lags, though, from my perspective, a GCM with accurate albedo parameterization would be needed. Lag experiments could be the subject of another paper. In this paper, we examine albedo sensitivity and feedback empirically, using data that we show are absolutely accurate in comparison to (and calibrated with) independent data.

Causality is inferred in numerous places throughout the text such as in discussion related to, especially Figs. 8 and 9 and Table 2.

comment: 16. p603-10-21: this is part of an introduction to the concept or part of the discussion, and seems a little misplaced here.

response: agreed, the equation 3 for Snet is now listed immediately after it is first mentioned. The subsequent albedo definition follows. The ordering is now more coherent.

comment: 17. p604-26 + p605-17 + p606-2 + p607-8: be consistent in regression

significance(vs. 2) and how to mention/explain it (using vs. RMSE).

response: on pg 605, additional text more consistently reads: “decline from 0.817 to 0.766 over the same period is larger than 2σ of the regression residuals and also exceeds the absolute albedo RMSE.”

on pg 606-2, additional text more consistently reads: “Significance is designated here more strictly where the trend measured by the linear regression slope has a magnitude that exceeds 2σ of the residuals from the regression”

on pg 606-2, additional text more consistently reads: “For 58% of the ice sheet area, the 12-yr albedo change exceeds the absolute albedo RMSE of 0.041. Over 87% of the area the change exceeds 2σ of the regression residuals.”

comment: 18. p606-last paragraph + p612-10: having correlations of 0.7 with maximum 12 observations, is a very weak statistical relation to infer anything about interannual differences.

response: 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.”

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C545/2012/tcd-6-C545-2012-supplement.pdf

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