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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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General comments

This study analyzes the albedo changes over the Greenland Ice Sheet (GrIS) during the period 2000-2011 derived from monthly composites of MODIS MOD10A1 data and compares them with changes derived from in situ albedo observations. Secondly, the authors compare the observed albedo changes with changes in air temperature (T), shortwave radiation (S) + other energy fluxes, meltwater and precipitation (snow/rainfall) derived from the MAR regional climate model in order to understand the albedo changes. In this process, the authors also use the SSMI melt extent to partition melt volumes (although the methodology to do so remains unclear). Finally, 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 ΔS_{net} / ΔT (i.e., the trends in S_{net} and T over time?).

Although the study addresses a very relevant scientific question that falls within the scope of TC, I have several comments mainly related to used methodology for calculating feedback, the obtained results and their discussion. Together these comments render the study unconvincing for publication and certainly need extra research, clarification and/or discussion. The comments are discussed in the major and specific comment sections, whereas some additional technical comments are included afterwards.

Major comments

- Reproducibility of GC-net albedo trends: the authors find large reductions in the MOD10A1 albedo over the ice sheet and verify that these reductions are real and not related to degrading MODIS instrument sensitivity. In this framework the authors perform a trend analysis based on the regression of the GC-net AWS in situ albedo with time. Using my experience with these data sets, I tried to verify their analysis using the processing steps the authors describe in their paper (Note: I have summarized them in the GC-net processing steps section below) and reproduce their results based on revised GC-net data. I used quality controled GC-net data received from N. Bayou dating from October 2011, but my data set is limited to fewer stations (only CP1, DYE2, JAR1, JAR2, JAR3, Saddle, Summit and Swiss Camp as not all 17 stations already had the necessary quality control assessment applied). Unfortunately, I have found significant discrepancies between my results and the results the authors present, which possibly could torpedo the paper.

For example, I have not found the consistent decreasing albedo trend for the in situ albedo. Where the authors find *95% of monthly May-September cases with a decreasing trend exceeding 2xσ of the regression residuals* (i.e., 2xRMSE), I only see a decreasing trend larger than 2xRMSE for a few station-month combinations (See table that is attached as a supplement).

Possible causes for the difference between my results and the paper's result, can be found that the authors not properly explain their used methodology or that they have a different GC-net data set. The former is certainly possible as I noticed several stations with monthly AWS albedos > 0.84 (e.g., positive trend in CP1 data is related to positive offset in albedo data since 2005), but the authors never explain that they correct for this, so I did not do it either. The difference in GC-net data set, on the other hand, could introduce differences but never should give such large discrepancies. For example, I also have an older GC-net data set, which gives only small differences with the newer data set. So perhaps, I am missing some processing steps, or somewhere else an error occurred (I rechecked my code, but did not find errors here). Anyway, the lack of consistent decreasing albedo trend for the in situ albedo really troubles me and certainly requires additional discussion because when the declining trend for the AWS is not so clear, it torpedos the whole discussion of the results, as several changes might be related to degrading MODIS instrument sensitivity or the quality of the MOD10A1 product.

- GC-net albedo vs. MOD10A10 regression: on p599 and in Fig.1 the regression between GC-net and MOD10A1 monthly albedo values is discussed and 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. I also rechecked this assumption for my data set (CP1, DYE2, JAR1, JAR2, JAR3, Saddle, Sum-

mit and Swiss Camp) and I obtain (when the MOD10A1 albedo values $> .84$ are removed as indicated in caption of Fig.1):

  - RMSE=0.069 for daily AWS albedo and daily (not filtered) MOD10A1 data
  - RMSE=0.059 for monthly AWS albedo and filtered MOD10A1 data

  This certainly not a reduction in RMSE by *nearly a factor of two because monthly instead of daily means are considered and the outlier rejection described in the previous section greatly reduces spurious temporal variability.*

- 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? This is unlogical and contradictory with the rest of the results, as lower albedo and more absorbed energy should result in a positive feedback when you consider that the GrIS shows higher T's for this period (Table 1)! The only possible explanation for the negative feedback could be the reduction in T, because when $\Delta\alpha$ and ΔS_{net} are positive, ΔT should be negative in order to get a negative feedback. Although decreases in T are mentioned in literature (Hanna et al. Increased Runoff from Melt from the Greenland Ice Sheet: A Response to Global Warming. J Climate (2008) vol. 21 (2) pp. 331-341), a decrease in T over 70% of the area seems a lot if you consider the T trends in Hall et al. (Greenland ice sheet surface temperature, melt and mass loss: 200006. J Glaciol (2008) vol. 54 (184) pp. 81-93). So if the authors observe negative trends in T for large parts of the GrIS, they should certainly include these results and they should be reviewed by experts in this domain. Another explanation for my doubts about the negative feedback could be that I did not completely understand what the authors quantify as feedback, as it is not very clear in the text how it is calculated. So I assumed what should be the real feedback: trends in S_{net} and T over time.

- Definition/discussion of negative feedback: the explanation/defintion of negative feedback at high elevations could mislead the readers, as it can give the impression that with global warming the albedo increases in the accumulation area. The presented results (or how I interpret them), however, shows that in a warming climate the albedo lowers over the entire GrIS (Fig6a), coincident with (perhaps?) a local decreasing trend in T (70% of the area as in previous comment?). In this context 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, because as a reader might consider a warm year (e.g., 2010) and conclude that this results in an albedo increase over the accumulation area, but that is not true if you consider the trends in Fig6a.
- Definition of feedback in Eq.5: if albedo feedback is defined as the combination of trend $\Delta S * (1 - \Delta\alpha)/\Delta T$, then it results in negative values if ΔS is negative, whereas the feedback should be $\Delta[S * (1 - albedo)]/\Delta T$, which can still result in a positive feedback even when ΔS is negative.

Specific comments

1. p.595-28: Stroeve (20o1) 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.
2. p596-7: Mention which MODIS data set was used in Box et al (2006) as large differences might occur between MODIS products.

3. p597-19 + 599-6: the nearest neighbour 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=1600pixels would be much more accurate. I therefore recommend another resampling approach (e.g. mean value per resampled pixel) which certainly is less error prone.
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.
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.
6. p598: which quality flag was used for the processing of the MOD10A1? See also p599-18.
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).
8. p600-2.4: add introduction why the MAR data are included in this manuscript.
9. p600-17: *just sufficient to resolve spatial gradients* : explain and argument.
10. p601-13: *additional MAR variables used in this study*: be complete as also precipitation is used but not discussed here.
11. Eq.4: what is R_N? It is never mentioned in the text.

12. p604-firstParagraph + p607-20: explain how this partioning is done. It is complete up to reader to guess how this works.

13. p603: albedo sensitivity and feedback: elaborate on how to calculate. For example, it is unclear what ΔS_{net} and ΔT mean and how to calculate them.

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/higer than one would expect using the observed trends. This however says nothing about real sensitivity, nor does it explain any causality.

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.

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

17. p604-26 + p605-17 + p606-2 + p607-8: be consistent in regression significance (σ vs. 2σ) and how to mention/explain it (using σ vs. RMSE).

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.

19. p610: *during warming years the albedo increases* + *accumulation area gains in brightness during warmer years*: this is completely contradictory to the earlier ob-

tained results showing a T increase and albedo decrease over both the ablation and accumulation area.

20. p612: *Instrument sensitivity is not detected*: the instrument degradation can still be present when there is also an real albedo decline. Moreover, when the GC-net data do not show the consistent decline in albedo (as in my results), the instrument sensitivity can even be *the explanation* for the overall albedo decline.

21. p613: *the brightening effect of summer precipitation, pronounced in anomalously warm years* + *a result of surface brightening resulting from increased snowfall* : brightening when there is an overall negative trend in albedo?

22. caption Fig.1: *albedo values less than 0.84*: this processing step is never mentioned in the text and creates confusion on how the data was handled.

23. Fig2: why only 2000-2005, if have both MAR and GC-net data for the complete 2000-2010 period are available?

Technical comments

1. p599-25: ... are estimated by Knap and Oerlemans (1996).

2. Fig6.: introduce a clear color distinction between positive and negative changes to add clarity

3. Fig9-10: introduce space between color separators of legend and negative sign of legend. Now this looks as one line.

GC-net processing steps

1. Calculate monthly albedo by:
   (a) Sum hourly SWu as ΣSWu and SWd as ΣSWd based on paired samples only.
   (b) Monthly albedo = $\Sigma SWu/\Sigma SWd$
2. Post process calculated Monthly albedo values:
   (a) If $\Sigma SWu > \Sigma SWd$, then Monthly albedo = NA
   (b) If less then 90% of hourly values are available for the ΣSWu and ΣSWd calculation, then Monthly albedo = NA
   (c) If the difference between Monthly albedo - Monthly albedo MOD10A1 $> .2$, then Monthly albedo = NA
3. Regression of Monthly albedo per month with time (years) and only using the results when at least 7 years of observations can be used in the regression.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C314/2012/tcd-6-C314-2012-supplement.pdf

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