The Cryosphere Discuss., 8, C2137–C2144, 2014
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Interactive comment on "Bimodal albedo distributions in the ablation zone of the southwestern Greenland Ice Sheet" *by* S. E. Moustafa et al.

Anonymous Referee #2

Received and published: 23 October 2014

Bimodal albedo distributions in the ablation zone of the southwestern Greenland ice sheet S. E. Moustafa, A. K. Rennermalm, L. C. Smith, M. A. Miller, and J. R. Mioduszewski

Summary: The study reports, for the first time, high resolution in situ albedo data collected in the ablation area of the Greenland ice sheet. These data are used in conjunction with surface cover estimates from another study to reproduce spatial and temporal variations in the albedo of the ablation area. The authors find that the ice-covered portion of the ablation area exhibits a bimodal distribution of albedo values, which changes over the course of a season, as darker materials are progressively exposed. Observations at a local scale are related to the distribution at larger scales

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obtained from MODIS data, which also has a bimodal distribution, and also changes over the course of the 2013 melt season. It is suggested that a sudden increase in the concentration of impurity rich materials associated with melting can trigger an abrupt shift in albedo, further enhancing melt by rapidly altering the distribution of absorptive vs. reflective materials.

General Comments: This study is an important contribution to the understanding of the albedo of the ablation area of the Greenland ice sheet, an important parameter for estimates of Greenland ice sheet mass loss. It provides a first detailed study of spatial variations in ablation area albedo using in situ observations, and reveals new information about spatial and temporal variations in the albedo of the ablation area.

I think this study should be published in the cryosphere, subject to the authors addressing the suggestions below. I consider the revisions to be relatively major, but most of the suggested revisions affect the interpretation of results, rather than the methodology used.

1. It is stated in the conclusions that abrupt shifts in albedo can occur as a result of changing dust concentrations. Abrupt shifts primarily occur for MODIS data, and seem more likely to be associated with changes in snow cover, while the observed changes at local sites (which are presumably snow-free) are actually more gradual. The conclusion that abrupt changes in albedo can be associated with sudden changes in impurities does not seem to be supported by the data and should be revised.

2. The authors suggest that "white ice" albedo is greater than 0.6, similar to the albedo of snow (Figs. 8 and 9). Thus the bimodal distribution of albedo over ice may be similar to the bimodal distribution for areas including both snow and ice. These numbers are higher than the cited range for bare ice (p.4740, line 10) of between 0.3 and 0.6. If the authors are certain that "white ice" is indeed ice and not firn or snow, the observation that ice albedo can be higher than 0.6, and that changes in ice albedo can occur over the course of a season, are important findings of this study and should be emphasized.

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If there is a possibility that there is snow cover present in the study area, the manuscript should be revised throughout to consider this possibility. In the case of MODIS data, I think that snow likely plays a role in the albedo distributions. The authors should more thoroughly discuss differences in the distributions and changes in the distributions for local data vs. MODIS data.

3. The authors should discuss the discrepancy between the observed distribution along the transect over which ASD measurements were taken, and the bimodal distribution inferred from the surface types of Chandler et al. (2014), which appear to have very different peaks. Figure 1 suggests that the transect passes over relatively bright areas, while the MODIS pixels and the area of Chandler et al. (2014) may cover a wider range of values. Also, it was noted that during sampling with the ASD, streams and cryoconite holes were not sampled, which would seem to reduce the frequency of dark surfaces sampled. The discrepancy should be noted in the results and discussed in the discussion section.

4. The procedure used for calculating broadband albedo values (P. 4743, Lines 14-16) appears to involve simply averaging albedo over a series of spectral intervals, which would assign too much weight to albedo values where incoming solar radiation is small. The best way to calculate broadband albedo would be to integrate incoming and outgoing shortwave radiation and divide the total outgoing amount by the total incoming amount. Please recalculate broadband albedo values if possible.

5. In some cases, the authors use "broadband albedo" and in other cases use "visible" albedo values for a smaller wavelength range. It would seem that broadband albedo would be more indicative of changes in absorbed energy and hence the energy available for melt. The authors should explain why different wavelength ranges are used, use broadband albedo in all cases, or perhaps compare differences in results for "visible" vs. "broadband" albedo if there are substantial differences.

6. I suggest moving the discussion of melt rates (P. 4750, Line 13 – P. 4751 Line 6, and

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perhaps Lines 7-11) to a separate section (Section 4.3) that follows the discussion of albedo distributions (Section 4.2). This would allow the manuscript to flow better and would allow the authors to introduce the bimodal distribution of albedo before presenting results regarding its influence on melting.

Specific Comments 1. Consider revising “ablation zone” to “ablation area” throughout. 2. P. 4738, Line 7: The statement that the role of distinct surface types on surface albedo is “excluded in surface mass balance models” is not true. The MAR and RACMO models, for instance, account for the presence of bare ice. Perhaps a statement such as “not represented in detail…” or “represented in a relatively simple manner” is more accurate. 3. P. 4739, Lines 9-12: The feedback also involves a melt-induced increase in the percentage of the surface covered by bare ice, impurities and meltwater, which further enhances melting. Please include these effects. 4. P. 4739, Lines 15-16: Again, while some processes such as the transport of dust are generally not included in RCMs, some processes, such as the presence of surface water and bare ice are accounted for (though perhaps in a relatively simplistic manner). Please clarify. 5. P. 4740, Line 25: Alexander et al. (2014) indicate some discrepancies between MODIS albedo products. 6. P. 4740, Line 26: “Physically unrealistic” seems too extreme. The latest version of RACMO (van Angelen et al., 2012) uses (realistic) MODIS background albedo. The schemes employed by MAR account for the presence of bare ice and capture the change in albedo as bare ice is exposed. Please revise. 7. P. 4740, Line 30: The word “poor” suggests that there is something wrong with the schemes used. Perhaps, “relatively simplistic” is more accurate. 8. P. 4742, Lines 21-22: Since the foreoptic was used, perhaps it would be better to indicate the field of view with the foreoptic? 9. P. 4742, Line 22: Is the diameter of the spot 1.1m or is the area 1.1m2 ? Please clarify. 10. P. 4743, Line 14: Perhaps “albedo spectra” should read “spectral albedo values”? 11. P. 4743, Lines 14-16: As noted in the general comments, the statement “Broadband αASD …” is ambiguous. Is broadband albedo calculated by averaging albedo values over each spectral interval provided by the spectrometer. 12. P. 4744, Line 11: This is a bit unclear. Change “MOD10A1 albedo” to “MOD10A1 albedo for pixels". 13. P. 4744, Line 17: Please define "similar results". For example, results would be expected to be similar for the distribution of albedo values and temporal changes in albedo. 14. P. 4745, Lines 10-13: These two sentences probably could be removed as the information seems redundant. 15. P. 4745, Lines 18, 19: Are the "average" values in parenthesis the average range of diurnal variability? Please clarify. 16. P. 4745, Lines 19-21: Since CC is derived from observed incoming SW, it seems self-evident that they would be well correlated. Perhaps this sentence should be removed. If the authors wish to keep it, the phrase "yet on average, remained low" is unclear and should be revised. Also, since CC is based on a combination of modeled clear-sky SW and observed SW, perhaps this should read "Derived CC reveals" rather than "CC simulations reveal". 17. P. 4745, Lines 26-27: Could hysteresis also result from changing surface conditions over the course of a day? 18. P. 4745, Lines 20-26: Given the high range of variability that is observed along the transect, the comparison shown in Figure 4 seems unnecessary. It appears that the average αASD over the entire transect is being compared with the albedo at the stations, but this is somewhat unclear. I suggest removing this figure, or alternately comparing station measurements with αASD measurements within a small radius of the weather stations. 19. P. 4745, Line 25: Table 3 is only mentioned here in passing. Please provide more discussion of the data shown in Table 3 or alternately, remove it. 20. P. 4746, Line 1: Here it is stated that Top Met Station measurements are excluded, but it appears that the measurements continue to be mentioned in the results and discussion section. Please clarify. 21. P. 4747, Line 10: Why are "visible" albedo values used here while albedo values for the entire spectrum are used in other analyses? See general comment 5. 22. P. 4747, Line 15: Please provide a few more details regarding how cryoconite hole albedo was parameterized. 23. P. 4747, Line 25: Perhaps the authors can refer to the figure from Chandler et al. (2014) that shows fractional changes in surface types, for clarity. 24. P. 4749, Lines 2-7: The additional terms (such as net LW radiation and sensible and latent heat fluxes) could be mentioned for clarity. 25. P. 4749, Line 17: Do the authors mean that the station values are "distinctly different" from average αASD? 26. P. 4750, Line 1: Are the values for 28 June and 14 August switched here? It may be better to report the trend over this period as the variability is rather high. 27. P. 4750, Line 2-4: This claim can only be made for the month of June. Please clarify. 28. P. 4750, Line 23: Mention turbulent heat fluxes in addition to longwave radiation. 29. P. 4750, Lines 23-24: "Relative melt rates between..." The calculations capture the fact that melt rates are substantially different, but not the magnitude of the difference between melt rates. Please revise for clarity. 30. P. 4750, Lines 26-27: To the contrary, there seems to be a wider range of ablation rates for "dark" rather than "white" ice. Please revise. 31. P. 4751, Lines 1-6: Could it be that sensible heat flux from stream water, which is not accounted for in radiative estimates, can lead to increased melting? 32. P. 4751, Line 9: Clarify that these are computed frequencies for the nearby region of Chandler et al. (2014). 33. P. 4751, Line 18: As noted in the general comments, please mention differences between the appearance of the distributions for αASD vs. those derived from the data of Chandler et al. (2014). 34. P. 4752, Lines 18-20: For the MODIS data this could easily be a result of snow melt exposing impurity rich ice below, since the albedo values for "white ice" and snow are similar. In fact this seems to be a more plausible reason for a sudden shift in albedo. 35. P. 4753, Lines 2-4: Perhaps note that MODIS albedo (shown in Fig. 6) is observed to decrease nonlinearly, with a smaller rate of change towards the end of the season. Also, since an alternative explanation is provided in the subsequent sentence, "mitigates" should be replaced by "may mitigate". van den Broeke et al. (2011), p. 378, attribute a gradual decline in albedo over the course of a season to the gradual removal of snow patches from the surface. Perhaps this is also a possibility at this location, unless the authors observed no snow patches during the field expedition. 36. P. 4753, Line 5: Replace "ground albedo" with "surface albedo" to avoid confusion with the albedo of tundra in proglacial areas. 37. P. 4753, Lines 17-22: Alexander et al. (2014) also used a lower resolution of 25 km, which may limit the ability to distinguish between dust and ice in some areas. However, the results of this study suggest that "white ice" can have an albedo similar to that of snow, which means that the distribution for ice may be similar to that for areas covered by both snow and ice. 38. P. 4753, Line 25: Tedesco et al. (2011) and Box et al. (2012) discuss the role of grain size metamorphism at higher elevations, where it may play a role in changing snow albedo. However, both studies indicate that the exposure of bare ice (i.e. a change from light to dark albedo) likely plays an important role in changes in albedo in the ablation zone. What is different about the findings here is that a shift from high to low albedo is observed for areas that are apparently snow-free. Please revise. 39. P. 4753, Lines 27-28: For the MODIS data there do seem to be abrupt transitions, perhaps associated with the addition and removal of snow. In the case of the observed and computed albedo distributions, the changes seem more gradual, perhaps in association with impurity changes. Please note these differences. 40. P. 4754, Lines 7-11: As noted above, the observed abrupt shifts in MODIS distributions may be a result of snow addition or removal rather than changes in impurities, so the statement that changes in deposition of impurities will likely result in abrupt shifts in albedo seems to be a bit of a stretch. 41. P. 4755, Line 1: "of which these processes..." is awkward. Please remove or include in a new sentence.

42. Figure 1: The text within the inset is hard to read. Can the text or inset be made slightly larger? Also, there is no scale bar for the inset. 43. Figure 1, Caption: Indicate that the yellow boxes show MODIS pixel extents. 44. Figure 4: As noted above, I suggest removing this figure. If the figure is included or revised, the x and y axes should be adjusted to have the same range, and the graph should be made square so that both axes are scaled equally. The difference in the axes results in the appearance of a weak relationship between the variables, although there is some correlation indicated by the statistics. 45. Figure 6: The circles used to indicate individual MODIS measurements don't show up on the legend. If possible please revise the legend. 46. Figure 9, Caption: Mention that the distributions are computed for the site of Chandler et al. (2014) 47. Figure 11, Caption: Mention in the caption what the melt rates are relative to.

Technical Corrections: 1. P. 4738, Line 13: Change "30 August." to "30 August 2013."

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for clarity. 2. P. 4739, Line 22: Change “Large-scale” to “The large-scale”. 3. P. 4739, Line 25: Change “lack of” to “a lack of” 4. P. 4741, Lines 18-19: Suggest changing “surface type’s fractional area” to “fractional area of surface types”. 5. P. 4741, Line 22: Change “changing albedo and surface type coverage’s impact on” to “impact of changing albedo and surface type coverage on” 6. P. 4744, Line 24: Change “were” to “was”. 7. P. 4745, Line 10: “information” can be removed. 8. P. 4745, Line 26: Fig. 3a is referred to here, but αtop measurements are not provided in Fig. 3a. Please revise. 9. P. 4745, Line 4: Should the reference be to Fig. 3a rather than 3b? 10. P. 4748, Line 4: Change “data is” to “data are”. 11. P. 4748, Line 26: Remove “to” from “$\geq$ to”. 12. P. 4749, Line 15: Change “αASD spatial range” to “The spatial range of αASD”. 13. P. 4749, Line 25: Change “by” to “for”. 14. P. 4750, Line 6: Change “temporal variability show a general agreement” to “there is a general agreement with regards to temporal variability” or something similar. 15. P. 4750, Line 9: Change “within 10 m” to “within a 10 m” 16. P. 4752, Line 11: Change “GrIS’s” to “GrIS”

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