The Cryosphere Discuss., 8, C2075–C2085, 2014
www.the-cryosphere-discuss.net/8/C2075/2014/

Interactive comment on "Bimodal albedo distributions in the ablation zone of the southwestern Greenland Ice Sheet" *by* S. E. Moustafa et al.

Anonymous Referee #1

Received and published: 15 October 2014

1 Summary

Moustafa et. al. use a combination of i) ASD albedo measurements collected in the field, ii) broadband albedo data from AWS's and iii) MODIS MOD10A satellite albedo data to study spatio-temporal distributions in surface albedo values for the ablation zone of the Greenland Ice Sheet (GrIS). They find that seasonal ablation zone albedo values have a bimodal distribution with two alternate states. Based on this they state that changes in the GrIS ablation zone albedo are not only a function of ice crystal growth, but are controlled by the changes in fractional cover of snow, bare ice and impurity rich surface types.

2 General comment

Although Moustafa et. al. touch a very interesting and important research topic (i.e. the seasonal evolution of ice surface types in the GrIS ablation zone) that is very relevant for (future) surface mass balance estimations, there are several main issues and a variety of smaller comments that need to be clarified and corrected (see major and detailed comments below).
Generally, I do think that Moustafa et. al. are overcriticizing the existent SMB models while simultaneously overinterpreting their own results. For example, the latest generation of SMB models does take the major variability in ablation zone albedo into account (i.e., snowfall events, spatial variability (e.g. Van Angelen et. al., 2012). Therefore they should already account for the major albedo variability in this study, which I also tend to attribute to (degraded) snow and spatial variability. In that context, I do believe they are obtaining results that are very close to the results obtained by Alexander et. al. (2014). Moreover, by comparing the differences in melt relative to melt with unrealistic high ice albedo values of 0.7 they are overestimating the impact of their study.

3 Major comments

1. Moustafa et. al. state motivate their work based on the clear separation between the albedo schemes of ablation zones in current surface mass balance models (e.g. in RCMs) and the albedo values they observe. In their motivation they implicitly take two assumptions. Firstly, they assume that a constant and spatially uniform ice albedo is being used throughout the ablation season in the existent SMB models and, consequently, they assume there is no seasonal variation in the ablation zone albedo. Secondly, they assume that last winter's or wind redistributed snow does not play a role in their data and, consequently, they assume that they are observing only evolution of ablation zone ice surfaces.

Although I agree 100% with the importance of understanding/assessing the seasonal evolution of ablation zone albedo, I don't agree with this separation. Firstly, because recent SMB models (e.g. in RACMO; Van Angelen et. al., 2012) take the spatial variability in ice albedo into account (e.g., spatially variable ice albedo background map that accounts for spatial differences in dark material, etc.). Secondly, because snow in these models also results in a bimodal albedo distribution (e.g. Fig. 5 of Van Angelen et. al. (2012)) as short term snowfall periods or redistribution of blowing snow on top of the underlying ice can result in variations in ablation zone albedo (i.e., here the bimodal distribution is the result of the deposition/change/removal of the snow layer on top of the ice layer). Thirdly, they assume that the observed changes in albedo are completely independent of snow (redistribution) events, whereas I think the observed fractions of white ice are very closely related to these (earlier) snow events. Therefore, I do not see this clear separation between ablation zone albedo in SMB models and evolution of ablation zone albedo in this study.

2. Although I agree that SMB models don't have the evolution of ice surfaces (e.g. dust deposition/accumulation, cryoconite development, roughness evolution, etc.), they do have variations in ablation zone albedo (e.g. on top of a spatially variable ice background (Van Angelen et. al., 2012) or as a function of ponding water (MAR)) as a result of the deposition/redistribution of snow which also results in a bimodal distribution. I am convinced that large part of the albedo evolution in Fig.6 is closely related to this bimodal distribution (presence/absence of snow or at least the remnants in the form of white ice) which is also in the SMB models if they model it correctly. For example, if I look at Fig. 13, I have the impression that this 'bimodal distribution' is the effect of the presence of snow or degraded snow (which you call white ice), which perhaps is also in Alexander et al. (2014). Therefore, I think you should prove that this bimodal distribution is effectively not the result of the disappearing (already degraded) snow instead of

the evolving ice itself (i.e., completely independent of previous snowfall events) and/or what the contribution of (degraded) snow to this bimodal distribution is?

3. You state the 'seasonal changes in GrIS ablation zone albedo are not exclusively a function a darkening surface from ice crystal growth' and I do completely agree. The current SMB models do not claim that either for the ablation zone, because the major variability the ablation zone albedo in these models is driven by the deposition/change/disappearance of snow. Consequently, these models also have the bimodal distribution with sudden changes (e.g. sudden disappearance of snow or the suddent albedo reduction due to localized melt within the snowpack). Again, here you will have to prove that the bimodal distribution is effectively not the result of the disappearing (already degraded) snow instead of the evolving ice itself (i.e., completely independent of previous snowfall events) and/or what the contribution of (degraded) snow to this bimodal distribution is?

4. Is the spatial or temporal variability of importance? I do have the impression that the spatial variability in 'ice albedo' is an order of magnitude more important than the temporal variability (E.g., the difference between pixel 1 and 2 is bigger than the within pixel variability, certainly if you assume that the biggest temporal variability is driven by appearance/disappearance of snowfall (e.g. 29/6 or 7/7)). If you then take into consideration that for example Van Angelen et. al. already accounts for the spatial variability in ice albedo, the uncertainty of the existing SMB's, which is the motivation for your research, reduces significantly. I certainly think this should be discussed in your paper.

5. Although I agree with the Short Comment of Pope that it is good that you provide so much information on the processing of the ASD data, I think large part of this processing (e.g. section 3.4 and Fig. 2-3) can be moved to a supplementary material as I think it diverts the reader of your main message. Moreover, some of this processing should be improved (e.g., the use of different wavelengths, etc.)

to avoid wrong interpretations (see detailed comments).

6. The hypothetical albedo distributions based on ASD albedo (400-700nm) values for distinct surfaces and the fractional surface coverage area from Chander et. al. (2014) is prone to many assumptions that are wrong or difficult to justify. Therefore,it is very difficult to draw any conclusion from it. In my opinion, it is an interesting thinking exercise, but it stays far from the real bimodal distributions that will be much more blurred due to i) broadband values instead of overestimated albedo differences in the 400-700nm, ii) uneven standard deviations for different surface types (see detailed comments). This blurring is also what I tend to see in Fig. 10 and therefore I would remove this analysis as it will give you an overestimation of the real bimodality.
7. By comparing melt rates relative to the early summer ice melt rates, the underestimation in the existing SMB models and the importance of this study is overestimated. For example, existing SMB models with a fixed ice albedo use values of typically 0.5-0.45, but that locally go much lower (e.g. Van Angelen et. al. 2012), whereas Moustafa et. al. compare melt relative to melt with albedo values of 0.7. Off course, this will result in strong increases in melt rate when compared relative to unrealistic high ice albedo values with unrealistic low melt rates.

4 Detailed comments

p4738 L 7 “excluded in surface mass balance models” : Depending on how you define a surface mass balance, I do not completely agree. For example, Van Angelen et. al. (2012) included a spatially variable background albedo in RACMO which accounts for the spatial variability in surface properties once the snow is gone. Also MAR, for example, allows for ponding water, etc..

p4738 L20: “are not exclusively a function of darkening from the surface from ice crystal growth”: that is absolutely true, but the existent models also do not claim that. See major comments 1-3

p4739 L22-28: Any idea what the effect of increased roughness on the changes in albedo is?

p4740 L7: Add “and crevasses and other types of roughness begin to form” ?

p4740 L24-28: You are perhaps too optimistic about satellite albedo estimates and too hard for the RCM albedo description. This should be more nuanced.

p4749 L28: "relatively smooth terrain" I understand what you mean, but this might be confusing for the non-experienced reader. I would change it to "lack of surface roughness in the RCMs"

p4749 L29: Perhaps it is worth to mention the Van Angelen et. al. (2012) already has a spatially variable ice albedo scheme.

p4741: L9-29: I think this section, which provides a complete summary of your manuscript is perhaps too long as it reads more as an abstract.

p4743 L8: “in close proximity”: is close proximity enough when you have only a 1.1m footprint? If there is only close proximity, you are sampling different plots for each transect overpass (and I think you are anyway). Therefore, and given the large fine scale spatial variability (as seen in Fig. 5), you are obtaining transects which are very difficult to compare.

p4743 L14 “spectra > 1.0” based on the assumption that you have an equal amount of outliers in each side of the mean, you will underestimate the final albedo, because you tend to remove only the postive (>1) outliers. Can you comment on that?

p4743 L15: To obtain broadband albedo you should never (!) average over the entire spectral range, but you should apply a weighted average based spectral response curve and the amount of incoming radiation per wavelength. Otherwise you will obtain albedo data that are not comparable to the albedo values derived from broadband sensors (See for example Table 3). Given these large discrepancies (0.1), I also think it is very difficult to interpret the melt rates calculated based on these visible albedo values.

p4743 L24-25: Can you give an idea (+ add it to the text) of the amount of observed tilt as it can give an indication of the albedo uncertainty

p4744 L5: (e.g. Lhermitte, S., Abermann, J., Kinnard, C. (2014). Albedo over rough snow and ice surfaces. The Cryosphere, 8(3), 1069–1086. doi:10.5194/tc-8-1069-2014 or Warren, S., Brandt, R., Hinton, P. (1998). Effect of surface roughness on bidirectional reflectance of Antarctic snow. Journal of Geophysical Research, 103(E11), 25.)

p4744 L16: Although the differences between data sets are logical (I expect higher albedos for 300-1100nm than for the entire SW spectrum), it complicates comparison as the absolute differences between the data sets are almost bigger than the spatial and temporal variability. Therefore, it is important to include a rough correction for the different spectral ranges (e.g. based on a reference spectrum) .

p4744 L17: What do you mean by similar results and do you effectively expect that? For example, based on the spectral differences I do expect for the 300-1100nm data a higher albedo for the white ice and a lower albedo for the dark ice compared to the broadband albedo values.

p4744 L18: Are mean ASD data per MODIS pixel a reasonable assumption? I have my doubts. Firstly, because the MODIS observations have footprints that ofetn are much larger and include data from neighboring pixels (i.e. pixel 2 data in pixel 1 data and vice versa). It is true that the MODIS pixels data are resampled to a fixed grid, but this does not remove the larger footprint effects (See for example Dozier, J., Painter, T. H., Rittger, K., Frew, J. E. (2008). Time-space continuity of daily maps of fractional snow

cover and albedo from MODIS. Advances in Water Resources, 31(11), 1515–1526. doi:10.1016/j.advwatres.2008.08.011). So this implies that both pixel 1 and 2 are often not that separable and certainly not allow a clear separation of the ASD measurements. This should be discussed.

p4745 L3-13: What is the temporal resolution to calculate CC? Every second, minute, 15mins, hourly? And how do you define variability (range? standard deviation?)

p4745 L6: How do you account for surfac albedo values in the Iqbal model as the Clear sky incoming radiation is strongly dependent on the surface albedo (e.g. Sedlar, J., Tjernström, M., Mauritsen, T., Shupe, M. D., Brooks, I. M., Persson, P. O. G., et al. (2010). A transitioning Arctic surface energy budget: the impacts of solar zenith angle, surface albedo and cloud radiative forcing. Climate Dynamics, 37(7-8), 1643–1660. doi:10.1007/s00382-010-0937-5)

p4745 L18-19: 662 and 239: is that average incoming radiation or average variability?

p4746 L3-14: You spend a large amount of text on discussing why you are only using 3 of six transects. I think that is not completely relevant for your story and could therefore be moved to supplementary material.

p4746 L9: “reduced the amount of longwave radiation” I would expect clouds to increase the longwave radiation? Or do you mean longer wavelength SW radiation? Anyway, I think it is best to remove all cloudy ASD observations from your data set.

p4746 L20-26+Fig.4: I would not draw any conclusion from this figure. First of all, there is no linear relation apparent at all (six points with a ASD albedo of 0.5-0.6 and a highly variable MET albedo + one clear outlier) so any interpretation is not very meaningful. Secondly, how come you have 5 points (base) and 4 points (top) for the ASD data if you only have three useful transects?

p4747 L9: Why do you suddenly restrict your wavelengths to only the 400-700nm range? This makes again any comparison very difficult and would overestimate the albedo differences between white and dirty ice compared to the values of the broadband albedo. This will cause overestimation in all your later results.

p4747 L11: Which bimodal distribution? This is completely unclear in this part of the text if you haven't read the next parts yet.

p4747 L18: By taking the 400-700nm albedo data you overestimate the differences in albedo between white and dark snow and you tend to separate the bimodal distribution much more than would occur in reality in the broadband spectrum.

p4747 L22: I think it is not very realistic to fix *s* to a fixed value as I expect the white ice values to have much higher standard deviations than the darker surfaces due to the non-linearity of albedo decrease to increasing impurity/melt. You also can see this in Figure 8. Moreover, why do take a standard deviation of 0.09, when your observations show much larger standard deviations (e.g. Table 3)

p4747 L24-28: I think that using fractions from another study over another year determined over a very small footprint may help to provide a nice thinking exercise, but give very little indication of what is actually happening in reality. Moreover, as you tend to overestimate the differences between, for example, dark and white ice (see previous two comments) I believe your modeled bimodal distribution is overestimating the bimodal distribution observed in reality. Therefore, I would recommend to remove this analysis from the manuscript.

p4747 L10: 463m is indeed the resolution for the zenith observations but the final effective resolution will almost always be different depending each overpass (see also earlier comment)

p4749 L14: Is it day-to-day variability or are you just sampling different sites? Based on my earlier comment, I tend to believe the latter.

p4749 L15 "spatial range? You mean spatial variability?

p4749 L23 "uneven decline" I understand what you mean, but it is a very confusing way

to formulate it.

p4749 L26 "inconsistent decline" What is inconsistent about it? It is completely consistent to me. A steady decline + noise + some snowfall events

p47450 L3-4: "The general darkening observed in α_{base}". Sorry, but I do not see that general darkening, as it seems to be a darkening followed by a increase in albedo again.

p47450 L4-6: "temporal variability shows general agreement". Sorry, again I do not see that general agreement. α_{MET} decreases followed by an increase, whereas α_{ASD}'s only decline. α_{MODIS}'s seem to be fairly constant over the period when α_{ASD}'s decline.

p4750 L13-22: Isn't an overestimation (factor 2) of the difference in melt rates (observed difference light-dark=2.31 10 -7m/s, vs. calculated difference light-dark=4.63 10 -7m/s) resulting in an overestimation (factor 2) of the effect of albedo difference on increased melt rates?

p4751 L8: see my earlier comments, but I believe you severely overestimate the bimodal distribution (e.g. by too high difference between white and dark ice, by underestimating the standard deviation (especially for white ice), etc.)

p4751 L12-17 "darker surfaces progressively populate" Is it dark surface that grow or is it just the (degraded) snow that disappears? Similarly, is the dichotomy not the result of disappearing snow and thus possibly already included in the SMB models?

p4751 18-21: I do think the results in Fig.11 are overestimating the melt rate effects (see my previous comments)

p4751-4752 L22-2: Aren't you here also stating that the difference is due to the presence/(dis)appearance of snow? Consequently, it could already be in the SMB models.

p4752 L3-7: If I am correct MOD10A is giving direct beam albedo (i.e. black sky albedo), which is strongly dependent on the solar zenith angle. So how much of that variation would be caused by variations in SZA?

p4752 7-9 + Fig.14: I do think the results in Fig.14 are overestimating the melt rate effects (see my previous comments)

p4752 L27 “due to fluctuations in diurnal shortwave fluxes”: What do you mean by that? Isn’t the unsteady declince driven by small snowfall/redistribution effects, etc?

p4753 L9 I don’t agree with these assumptions and I think they tend to overestimate your bimodal distribution (see earlier comments)

p4753 L14: “abrupt shifts” Could these shifts not just be the shift from (degraded) snow to ice, or from dry snow to wet snow? And isn’t that exactly what Alexander et al. formulate?

p4753 L24-27: “and not solely grain size metamorphism” I do agree, but neither Box or Tedesco, nor any other SMB model, do claim that either. Both Box and Tedesco clearly indicate that the longer exposure of ice and the lower summer snowfall was responsible for the lower albedo values in the ablation zone.

p4753-54 L28-2 I do agree, but as you can see in the figure, the initial drop or the partial snow variability is much more important (albedo variability of 0.2) than the the subsequent decrease due to darkening (albedo variability of maximum 0.1)

p4753 L4:’Substantial’ are the differences also equally substantial if you compare to a more realistic reference albedo values of 0.4-0.35 that would be used as a background ice albedo for this region?

p4753 L17 “Previous studies have ...” I do not agree (see earlier comments)

Interactive comment on The Cryosphere Discuss., 8, 4737, 2014.