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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.

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This paper address an interesting an important topic – the local and evolving albedo of a glacier / ice sheet surface. The study is framed as having implications for Greenland melt modeling and forecasting, which makes sense and is a crucial topic to continue to address. It is great to see additional high quality reflectance spectra from glacier surfaces (I know I'm biased, having collected some in the past, but still!), and the paper provides a lot of information about field data collection and quality control, thus providing more confidence in the next steps of analysis and discussion. However, as detailed below, I think that there are some finer points in the manuscript as well as

broader points in the discussion/analysis which I think need to be addressed for this paper to achieve the impact that it is aiming for.

Introduction

**Surface albedo is defined as "is defined as the ratio of reflected to incident solar radiation upon a given surface (Schaepman-Strub et al., 2006)." However, that paper explicitly calls out the ambiguous use of the term "albedo." Even in common usage (as bihemispherical reflectance), "albedo" can refer to either the entire spectrum of solar radiation or the visible part of the spectrum (IASC Mass Balance Glossary). I do appreciate the desire to be pithy and concise, but I think it would help comprehension if you were more explicit in the introduction and through with the types of reflectance being compared and the wavelengths implied.

**The first paragraph continues on to describe positive albedo feedbacks as observed in previous studies. The was confusing to me as the reader because those studies (as summarized by Stroeve) show "The downward albedo trends indicate both a reduction in the duration of snowcover over low albedo bare ice, and an expansion of the bare ice area (Box et al., 2012; Tedesco et al., 2011)." This paper, on the other hand, avoids snow altogether and instead describes change in albedo on clean/dirty ice surfaces and cryoconite. This juxtaposition was misleading to at least this reader. To me, it would have been clearer to me to immediately move on to the more relevant content of the 2nd paragraph. (Another way to say this is that many studies focus on the whole of the GrIS – to be self-consistent, this paper should be highlighting studies / processes which focus on what happens after bare ice is exposed.)

**Page 4740, Lines 13-15 affirm that negative albedo trends are "linked to a darkening of the ice surface from increased surface coverage of meltwater, cryoconite holes, and impurity-rich surface types (Bøggild et al., 2010; Chandler et al., 2014; Wientjes and Oerlemans, 2010)." It would be clearer to say that ablation facies have expanded relative to accumulation facies (i.e. Box: "the strength of albedo feedback is determined

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more by the surface albedo decrease associated with a loss of seasonal snow cover than the reduction in snow albedo due to snow metamorphosis because of the large difference between snow and bare ice albedo values.") rather than this sentence, which implies darkening ice surfaces, which is not previously substantiated in the literature as far as I am aware.

ASD Data Collection

**You mention the use of a foreoptic for solar illumination collection. Was no foreoptic used at all for data collection? And if not, were data collected with the sensor oriented in different directions horizontally so as not to bias measurements on particular sensor fibers? Also, was each spectrum really only 5 samples, or was each sample actually also a repeat of 'x' scanes (e.g. 25-50 scans), as is standard with ASD software?

**You say that αASD is averaged over the whole spectral range (of 325-1075 nm). Instead, for better comparison with MODIS as is done later, would it make more sense to use MODIS relative spectral response functions to weight the spectra? Or does the increased MODIS range (300-3000 nm) really change the comparison enough already to make this not worthwhile?

**Your regression αASD vs αMET as presented on Page 4746 / Figure 4 seems very skewed to the datapoint on the left. Afterall, αASD ranges from 0.45 to 0.6 beside that one 0.15 value, while the αAMET are well spread between 0.3 and 0.65. To me, this puts into question the regression (and use of a 40% variance statistic). While I think there is likely some truth to your statement that "The discrepancy is likely due to differences in exact sample locations and instrumentation", I think your regression puts the ASD and MET data into question more than reinforcing their intercomparison.

Melt

**Small note: Page 4748, Line 16 makes an assumption about net longwave radiation being negligible for surface melt. Since you go on to later talk about relative rates

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anyway, you might not need to state this assumption, and just acknowledge you are addressing shortwave flux only? Either that, or back up your assumption with a reference.

Distribution Discussions

**Page 4750, Lines 15-22: I might be misunderstanding, but these numbers don't all seem to match up. E.g. the difference between 5.8 and 7.0 is 1.2, not 2.3. Or am I misunderstanding? Same comment for the subsequent set of numbers, too.

**Fig 12: The term "spatial scale" is ambiguous to me. As I understand it, you are varying the spatial extent, not the spatial resolution. Apologies if I misread that. SO – this figure is possibly two populations that are snow & ice, as opposed to "ice" and "dirty". Could you address how you are sure that there is no influence of snow in the scenes? Also, why else would he the spatial extent matter (as opposed to being able to see things at different spatial resolutions, i.e. small patches of dark things wouldn't be resolved, etc.)

**Overall in figs & discussion: I think the term "bimodal distribution" really needs to be used with caution (as opposed to single peak with a shoulder, wide peak, etc.) as this has important implications for later interpretation.

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