Dark ice dynamics of the south-west Greenland ice sheet
A. J. Tedstone, J. L. Bamber, J. M. Cook, C. J. Williamson, X. Fettweis, A. J. Hodson, and M. Tranter

Summary
MODIS satellite imagery is used to examine fluctuations in the extent of impurity-rich bare ice (dark ice) along the western margin of the Greenland Ice Sheet. A threshold on MODIS blue and red reflectance is used to identify bare ice and dark ice. Potential drivers of bare ice variability are examined using outputs of the MAR regional climate model, including shortwave radiation, longwave radiation, and sensible heat flux, in an attempt to understand causes of variability. The authors argue that while outcropping particulates are a major factor in bare ice albedo variability, the presence of biological organisms may also play an important role.

General Comments
The topic covered by the paper is important to our understanding of factors contributing to fluctuations in the albedo of impurity-covered ice in the ablation area of the Greenland ice sheet. It overlaps somewhat with the recent study of Shimada et al. (2016), but extends the analysis to a full summer season and attempts to understand drivers of dark ice variability.

I feel the authors need better support for their arguments that biology is a major driver of bare ice albedo variability. There is no definitive proof for this and I don't think the authors have successfully ruled out melt-out of impurities, sub-grid scale variability in snow cover and/or superimposed ice, or even the presence of liquid water, as potential causes of the variability. The authors have suggested that microorganisms appear to require the presence of outcropping material at the surface. If this is the case on a large scale, outcropping dust should control local and inter-annual variations in albedo as well. The authors' arguments that local-scale variability in dark ice extent can be explained not by dust melt-out, but by microorganisms, is inconsistent with the apparent need for dust as a microbial nutrient source on a larger scale.

I think that much of the variability the authors attribute to microorganisms could be attributed to dynamics of melt-out at small scales instead. Inter-annual variations in dark ice extent can be explained by the presence of superimposed ice, perhaps not fully accounted for in MAR. Increases in "Dark Ice Intensity" over time could be related to changes in surface cover within a relatively large MODIS grid box as snow patches and areas of superimposed ice melt away, exposing dark material beneath. The fact that sensible heat flux is a relatively important factor, as is the number of days where temperature is greater than zero suggests that melting of snow and ice could be an important factor independent of biological organisms.

Therefore, there appears to be insufficient information to state definitively the cause of the variations in dark ice extent and intensity, although I think the authors have shown that local deposition from year to year can probably be ruled out as a contributing factor.

Given a lack of clear evidence supporting a biological source for inter-annual and intra-annual variability in bare ice albedo, I feel that the authors should reduce the emphasis on biological organisms as a source of variability and should also give credence to the possibilities mentioned above.

The authors should also address the possibility that the thresholds used here can falsely identify liquid water and possibility even snow or firn as ice or dark ice. The first is probably a minor factor, but the second could potentially lead to a misinterpretation of the results.

The work presented here provides a valuable investigation of variations in ice albedo and the presence of impurities in the ablation area of the Greenland ice sheet. I support publication of the study, provided the authors address the points provided in this review.

Specific Comments

P. 1, Line 1: The recent increases in runoff are not caused by reduced albedo but by changes in atmospheric circulation and atmospheric warming. Albedo changes resulting from these changes amplify melt. Please clarify.

P. 1, Line 7: Add "in the future" after "will evolve".

P. 2, Line 6: The statement that "surface melting is controlled by albedo" should be clarified. Other components of the energy balance certainly play a role in controlling melting. Albedo can only play a role with sufficient downward shortwave radiation. Melting can potentially occur during portions of the year when there is less solar radiation as a result of sensible, or longwave fluxes. Please revise this statement, e.g. "Surface albedo plays an important role in modulating surface melt as the surface darkens with warming temperatures…."

P. 3, Line 34 – P. 4, Line 4: Is there a reference to which the authors can refer here or are these unpublished results of the authors? Please clarify the source in the text.

P. 4, Line 8: Independent of these processes, there is also the possibility of consolidation of impurities at the surface due to melt, which the authors do discuss later in the manuscript. Perhaps change "inorganic particulate deposition" to "inorganic particulate deposition or redistribution".

P. 4, Lines 28-29: These are all good points, but perhaps now say what the authors think *can* be done using the thresholds used here.

P. 4, Lines 29-30: Are the authors saying that some of the variability in extent or intensity could then be associated with grain size evolution and the presence of water? Please clarify.

P. 5, Line 1: Clarify how the maximum area was defined, e.g. using daily MODIS reflectance values.

P. 5, Line 8: Explain why pixels 1 km from the ice sheet margin were removed.

P. 5, Lines 11-12: What is meant by "all the pixels", the number of pixels or fraction of pixels?

P. 5, Line 13: Clarify that this is the percentage of all daily cloud-free observations that were classified as "dark" in each JJA period.

P. 5, Line 15-16: It is a bit confusing to refer to this as intensity and to have a lower number indicate a larger intensity. Can't this just be referred to as the average reflectance? Then a lower reflectance is associated with a darker surface.
P. 6, Line 6: Include a reference for the ECMWF reanalysis: (Dee et al., 2011) doi:10.1002/qj.828
P. 6, Line 15: Is the daily energy for melt-out "MOF"? Define MOF here. Based on the authors statements it doesn't seem that the MOF is necessarily a proven measure of the conditions needed to produce melt-out. If so it should be made clear that the MOF is suggestive of the conditions needed to cause melt-out, but does not necessarily indicate whether melt-out is occurring or not.
P. 7, Line 4: Clarify that this "extension is relative to the study of Shimada et al. (2016), which only examined July.
P. 7, Line 8: Change "time lag…" to "time lag between t_B and the first identified occurrence of dark ice of 10-15 days".
P. 7, Line 10: Anticyclonic days don't seem to be shaded gray in Fig. 4.
P. 9, Line 6: Change "magnitude of dark ice" to something like "extent and intensity of dark ice" or "extent and reflectivity of dark ice".
P. 9, Line 8: Clarify "years when the ice went dark". Perhaps "years when D_E was higher" would be more specific.
P. 9, Line 22: Change "Not only was winter snowfall" to "Not only was 2014-2015 winter snowfall…" for clarity.
P. 11, Line 24: Briefly not how the weathering crust forms.
P. 12, Line 27: Should "decimeter" be "decameter"?
P. 12, Line 21 – P. 13 Line 2: I am not totally convinced by this argument. Much of this could be explained by the presence of superimposed ice, sub-grid scale exposure of bare ice, or even the presence of firn that is mis-classified as bare ice. I don't think the authors can rule out melting as a primary cause of the observed variability, especially since they do not utilize measurements or estimates of melt here. I think the authors should be more careful to acknowledge that melt could be responsible for the observed variability, but that the results also suggest that other factors could be involved.
P. 13, Lines 17-25: The variability the authors are discussing seems consistent with the hypothesis of Shimada et al. (2016) except with regard to the changes in dark ice intensity during 2012 and between 2012 and 2013. The statement that "our results reveal a different spatio-temporal pattern" is therefore a bit confusing. As for previous section, the changes in intensity during 2012 could be explained by sub MODIS-grid-scale processes such as melting of snow patches, collecting meltwater. 2012 was a high melt year while 2013 was a low melt year. During 2013, ice is exposed for a much shorter length of time, and the presence of superimposed ice, or again, patches of snow covering the ice could explain the lack of dark ice during that year.
P. 17, Lines 8-9: The surface must be a mixture of impurities and biological materials, or could even be abiotic. How is the material assumed to be algae?
Figure 1: It would be useful for the reader to include numbers indicating the value of D_E for each image.
Figure 2: Mention t_B in the caption.
Figure 3: Note that the snow depth is from MAR. It would be interesting to also see t_B in this figure, to allow for a comparison with MAR.

Technical Corrections
P. 4, Line 15: Change "cloud" to "clouds".
P. 4, Line 28: Change "precisely identify precisely" to "precisely identify"
P. 5, Line 18: Add "(D_N)" after "normalized darkness" for clarity.
P. 5, Line 27: The phrase "with any…only allowed to be cloudy" is confusing. Perhaps just change to "excluding cloudy days".
P. 6, Line 12: Place a parenthesis around ($T>0$) for clarity.
P. 6, Line 13: Change "were been" to "were"
P. 9, Line 3: Change "not explicable by" to "cannot be explained by"
P. 9, Line 14: Change "snowfall which occurs" to "snowfall that occurs"