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The Cryosphere Discussions

Interactive comment on "The darkening of the Greenland ice sheet: trends, drivers and projections (1981–2100)" *by* M. Tedesco et al.

Anonymous Referee #2

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This work extends prior efforts to evaluate trends in Greenland ice sheet albedo by incorporating earlier AVHRR data as well as MODIS data. The results show a trend in declining albedo since 1996. Further discussion attributes this decline mostly to melt processes, with a modest portion of the decline, which is not matched by modeled albedo, attributed to exposure of light absorbing impurities, largely by process of elimination.

This discussion of potential causes of decline is valuable, and I believe the authors are correct in their assessment that LAI exposure must play some role. The line of support for the conclusions has a few weak points, however, which need to be patched up prior to publication. Most importantly, I find that the uncertainties in the modeled albedo are likely too large to firmly attribute any discrepancy to another mechanism. I think

the authors must at least evaluate and discuss the alternate hypotheses – namely that 1. the discrepancy between modeled and observed albedo decline is simply due to modeling error or inaccurate parameterization of the melt processes in the model and not due to any melt-exposure of light absorbing impurities. 2. Error in sensing albedo may exceed the discrepancy. Since the conclusions are based on a test site within the 'dark band' I think it should be possible to clarify #2 (see also specific comment on this below). #1 will require more thorough evaluation of the model's ability to represent melt processes, particularly its simulation of bare ice albedo. I found the ice sheet – wide comparison between MAR and station data inadequate to understand specifically whether the model is properly handling the very large albedo declines due to melt and exposure of bare ice. Specifically I request the authors show that the discrepancy between modeled and observed albedo can be rigorously differentiated from agreement between the two albedo values in the 'dark band' by placing error bars on the model values (and remote sensing values, though these are likely smaller) and defending these error bars. My suspicion is that this will be challenging, based on the disagreement with in-situ albedo values RMSE about 0.04-0.05 and few in situ observations in the ablation zone, but I think it must be addressed.

I also think it would be valuable for the authors to more clearly quantify what fraction of the albedo change is likely due to enhanced melt, vs. the fraction (residual) that is being attributed to impurities. The other anonymous reviewer appears to have interpreted that impurities dominate – and this does not appear to me to be true.

After these revisions the paper should be publishable.

Specific comments

Abstract: In reading the abstract I note that the model projections of albedo decline are at a smaller rate than you find in the 1996-2012 interval. You nicely expand this later in the paper to suggest that the models are likely underestimating albedo decline. This seems an important enough conclusion to try to work it into the abstract, lest the reader be left to wonder why the large rate discrepancy exists

Pg 5597 Line 13 - Suggest continuing with 'light absorbing impurities' or LAI throughout the manuscript. Simply 'impurities' is not sufficiently descriptive for a reader who jumps in to a later section of the study without reading the introduction.

Pg 5600 Line 16 – Though not central to the paper, this discussion of densification is inaccurate. A large fraction of this densification actually happens due to wind processes which break and round grains forming windslab of density typically around 0.3-0.4. The remaining densification happens by grain sliding. Additionally, and perhaps more importantly, snow which has recaptured meltwater (held it until it refroze) frequently exceeds densities of 0.55 in the percolation zone, at least in discrete layers.

Pg 5604 Lines 5-15 This discussion of potential MODIS degradation does not appear to be up to date. A recent publication by Polashenski et al. 2015 suggests the degradation is larger. Lyapustin et al., 2014 also suggest larger degradation. If the rate of 0.0059/decade from Stroeve et al., 2013 remains accurate however, this still means that more than 25% of the ice sheet wide albedo decline rate during the MODIS era (.02/decade 1996-2002) could be attributed to MODIS degradation. This is significant and should be discussed as such. I agree that the degradation is likely insignificant in the 'dark band'

If the reader mis-understands your conclusion (as I did first read through, when I skipped to the conclusions) to be based on ice sheet wide discrepancies between MAR and GLASS, the degradation appears to be a serious issue for you conclusion. Perhaps you can help the clumsy reader a bit with more clarification in your conclusions.

Pg5604 section 3.1, line 20... the trend stated here (-1702mmWE/decade) seems enormous for a trend in snowfall. I don't think mean snowfall was 1702mm WE to begin with. Please clarify/correct.

Line 23 Low 2010-12 albedo is attributed to low snowfall. Why not melt? Melt extents were greater than typical these years- the statement seems sort of offhand here when other options remain available.

Pg 5605, sect. 3.2 Line 2 Fort → For

Pg 5606, sect 3.3 Line 17 This sentence seems to confuse your central thesis that exposure of LAI deposited long ago is causing the albedo decline, by suggesting that local dust sourcing is to blame, without also mentioning the theory you later focus on. The paper would be strengthened by either dropping this sentence for later discussion or bringing both processes into the discussion here.

Pg5607. Lines 19-22 This discussion seems to need strengthening. The conclusion that the discrepancy arises from impurity deposition depends on the assumption that both MAR and the observations are behaving as designed. But - is MAR accurately handling bare ice? I think you need some in-situ evidence that it does or some more concrete support to the statement that bare ice albedo doesn't typically drop below 0.45 (p5605 line28). The conclusion you come to is likely correct, but in the absence of this evidence the reader is left to question whether the discrepancy could be caused by something else.

P 5608 Lines ∼25 This discussion could be strengthened with a reference to core data. See McConnell 2007.

P5609 This discussion, and conclusion at bottom of 5611 showing no evidence of an increase in aerosol deposition could be supported by citation of the recent Polashenski et al paper in GRL.

P 5613 line 5 and many other locations throughout. An albedo trend is stated without discussing what months of the year this applies. I think it would be helpful to the reader to clarify at each location what, exactly, the trend being discussed is – or is it possible to categorically state you are referring to JJA albedo only throughout the paper ?

5614 line 9 – 10 . This statement is true if you are referring to large discrepancies in the ‘dark band’. It is not true if discussing ice sheet wide trends. There the discrepancy between MAR and GLASS is too similar to the Terra degradation quoted to distinguish the two. Here and throughout clarification is needed to focus on the dark band case (which supports your case) vs. whole ice sheet treatment.

5614 line14 oft he → of the

5616 line 22 “the value.. was estimated. . .” By who? Should there be a citation?

5618 conclusions about grain growth with BC ‘doping.’ This section is accurately discussed as exploratory and fine to include if the authors choose, but it seems very weak to me. These models likely don’t have the necessary physics to alter grain growth based on absorption, and a gross change to albedo is a very crude way to explore this. I’m not sure the conclusion made here “grain sizes are typically only about 1% larger for dirty snow” is very defensible based only on this work, and an uniformed reader might over extend this very preliminary result. Mostly though, I think this exploration is a distraction in this work.

5619 line 25. I think CESM actually does handle melt concentration based on observations by Doherty et al., Please verify this statement.

Interactive comment on The Cryosphere Discuss., 9, 5595, 2015.