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

Anonymous Referee #1

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This study explores observed and modeled trends in albedo of the Greenland Ice Sheet. Significant darkening is observed during the last 30+ years, though the trends are confined to the ablation and melt zones of the ice sheet. Modeling studies are applied to attribute causes of the observed trends. One of the chief causes was determined, largely through elimination of other potential causes, to be exposure of buried impurities, a process that is not represented in the model that was applied (MAR). Overall, this is an informative and comprehensive study that pursues multiple lines of reasoning and analysis to narrow down possible causes of the darkening. The paper is very well-written and logically organized. The comments included below are generally minor, and I see no major hurdles for publication in The Cryosphere.

Major comments:

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A closely related study that explores recent (2001-2013) albedo change in the dry snow zone of Greenland was recently published in Geophysical Research Letters (Polashenski et al, doi: 10.1002/2015GL065912). Consistent with this study, Polashenski et al concluded that recent changes in deposition of dust or black carbon could not be causing substantial surface darkening trends in the dry snow zone, and furthermore that a substantial portion of the trend in dry zone albedo seen in MODIS data is actually an artifact associated with degradation of the MODIS Terra sensor. It would be helpful to cite this study in the context of (e.g.,) discussions in section 3.4.1, section 5 paragraph 1, and perhaps in Conclusions.

The description of AEROCOM model results (section 3.4.1 and Figure 6) and application of these results to exclude a significant aerosol deposition trend, need improvement. Specifically:

- The description beginning on p.5609, line 12 describes AEROCOM outputs from 14 global models, but text later in this paragraph suggests that only one model (the GISS ModelE) was actually used in the analysis. Is this correct? Similarly, the Figure 6 caption indicates "AEROCOM standardized deposition fluxes", but it is not clear if all 14 AEROCOM models were used here or only the GISS model. Results from 1 model are obviously less robust than results from 14 models. If only one model was used in this analysis, the description should be amended to refer to the GISS ModelE, rather than than AEROCOM suite of models.

- p.5609: Please mention the range of years that were simulated in these runs, whether or not interannually-varying aerosol emission inventories were used for the simulations, and whether or not interannually-varying sea surface temperatures were used to drive the model(s). All of these details influence the usefulness of this model analysis for determining whether or not real trends in aerosol deposition have occurred on the ice sheet.

- section 3.4.2: As the authors note, the lack of trends in MODIS fire counts is not

necessarily an indication of trends in fire-derived aerosol emissions. For example, fires could have become more intense, larger, and/or more persistent (longer duration), despite exhibiting no trend in count. The authors also state "Notably, we were not able to find studies specifically looking at trends in boreal forest fire emissions". To derive a more meaningful assessment of boreal fire emission trends, the authors could analyze satellite-derived black carbon emissions data from either the Global Fire Emissions Database (GFED) (http://www.globalfiredata.org/), and/or the Fire Inventory from NCAR (FINN) (http://bai.acom.ucar.edu/Data/fire/), both of which are gridded datasets that are freely available for download via the URLs listed above.

Finally, it is somewhat unusual for a co-author to post a comment asking for clarification on his or her own paper. This may indicate that more communication between the co-authors is needed.

Minor comments:

abstract, line 21: "known underestimates in projected melting": I suggest inserting "past" before "projected", or changing "projected" to "hindcasted".

5599,10: "... recent work assessing simulated albedo over Greenland..." - It would be helpful to provide a clue to the reader here that a discussion of this evaluation is presented later in the manuscript.

5600,26: "between" -> "Between"

5601,2: "In MAR these values of albedo are set to 0.65 and 0.55" - If any observational studies were used to justify these choices, please cite them.

Equation 6: This implies that an infinitely thick melt pond would have an albedo of 0.40. Should the albedo instead asymptote towards something more like 0.07, the albedo of open water?

5601,22: "... the albedo in MAR is a weighted, vertically-averaged value of snow albedo and ice albedo" - Please provide an equation or reference that describes this weighting.

In particular, the meaning of "vertically-averaged", in the context of surface albedo, is unclear to me.

5602,1: "in which case the albedos of snow and ice are adjusted based on the cloud fraction modelled by MAR." - This implies that the spectral weights shown in Equation 1 are adjusted for cloudiness (correct?). If so, I suggest clarifying precisely how this is achieved, perhaps earlier in the text where Equation 1 is described. If the technique is described in another paper it may be adequate to simply reference that paper.

5602,16: Is "GLASS-MODIS" any different than "MODIS" albedo (e.g., product MCD43C)? If so, how is it different? If not, I suggest simply referring to this as "MODIS". Either way, please list the MODIS albedo or reflectance product and version/collection from which GLASS is derived.

5603,9: "MODIS and GLASS" -> "MODIS and GLASS albedo"

5604,20: "Notably, strong negative summer snowfall anomalies from 2010 to 2012 are simulated by MAR..." - Are these strong anomalies also present in station data and/or re-analysis data?

5606,10: "summer albedo from GLASS decreased..." - And by how much did MAR albedo decrease over this time period? It would be helpful to include this in the paper.

5606,17: "This hypothesis is supported..." - Did Wientjes (2011) argue that *recent* increases in dust deposition to this region of the ice sheet have led to significantly decreased albedo, or that exposure of dust deposits buried long ago have led to the albedo decrease? It seems that only the latter would be consistent with the main explanation put forth in this paper. Please clarify.

5612,7: "info" -> "in"

5614,14: "oft he" -> "of the"

5616,8: "The MACC model shows no significant trend..." - And does the GOCART

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model show a significant trend?

5616,11: "Neither model captures trends in exposed silt/dust" - In fact, neither model even represents this process, much less captures the trend.

5617,22-29: The relevance of these results is a bit unclear to me. In fact, I think the whole discussion contained in this paragraph could be shortened, as readers may get bogged down in this discussion and it does not appear to be critically relevant for the main results of the study.

Figure 2 caption: Please indicate that figures (b-d) show MAR-simulated results (whereas figure (a) shows an observed trend).

Figure 3 caption: Please clarify (and double check) whether these maps depict MAR - GLASS, or GLASS - MAR trends. "... positive values indicating those regions where MAR trend is smaller in magnitude..." - I suspect this is incorrect. The map show sharply *negative* values in southern Greenland.

Figure 5: Please use larger axis labels. Please also list the spectral ranges used to define "visible", "near-infrared", and "shortwave-infrared".

Figure 6: As commented above, please clarify whether this depicts all AEROCOM models, or just the GISS model.

Figure 8: Does "RU" (legend) refer to Eurasia or Russia, and is this consistent with the caption? Are these annual, or summer-only fire counts? An alternative approach for this figure would be to include separate panels for North America and Eurasia, and show absolute fire counts instead of standardized quantities. By standardizing both, the relative magnitudes of Eurasia and North America fire counts becomes lost.

Figure 9 caption: In the next to last sentence "(bottom)" and "(top)" appear to be reversed. More melt under RCP85 should lead to a larger drop in albedo, so I would expect RCP85 to define the bottom of the envelope. Also, "GIS" should be "GrIS" for consistency with the rest of the paper.

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Figure 10: The caption refers to shades of grey, whereas the figure shows colors. Please clarify. Also, the red lines (GLASS albedo) are not readily apparent in this figure.

Figure 11: It would be helpful to note or indicate whether or not these trends are statistically significant.

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