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Interactive comment on "Arctic Climate: Changes in Sea Ice Extent Outweigh Changes in Snow Cover" *by* Aaron Letterly et al.

Anonymous Referee #1

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This study explores and compares the contributions of recent (satellite-era) changes in Arctic sea ice and snow cover to changes in absorbed solar energy at the surface, and finds that sea ice losses have contributed to a greater increase in solar heating than reduced Arctic snow cover. Similar analyses have been performed before, though this study carves out a niche by focusing exclusively on the Arctic (60-90 degrees) and comparing the terrestrial and sea ice contributions within this domain. An important point highlighted by the authors is that the major seasonal transition in albedo associated with sea ice loss is occurring near the summer solstice, whereas the terrestrial snow transition is shifting away from the solstice, implying greater solar forcing via sea ice loss in the recent past and near future. Overall, the paper is concise and very well-written, though there are several important aspects of the analysis that need to be revisited and/or clarified before publication. After these issues (described below) are addressed, I would support publication of this manuscript in The Cryosphere.

Major issues:

(1) The discussion on p.7 lines 4-14 describes how lower-latitude changes in albedo drive larger changes in absorbed solar energy than equivalent albedo changes at higher latitudes. This is true for annual-mean albedo changes, but it is not true for the summer-solstice-season changes that are the focus of this section. This can be seen clearly in the authors' own Figure 5, which shows greater daily-mean solstice insolation at 80N than at 65N. The discussion on p.7 lines 4-14 is therefore largely inconsistent with Figure 5 (the latter of which is correct, I believe). The authors need to amend the discussion and re-consider potential causes of the statistics described on p.7. Differences in cloudiness and cloud trends may be a logical starting point for resolving this discrepancy.

(2) The study focuses largely on changes in net shortwave flux at the surface, and the authors acknowledge at different points in the text that changes in clouds and sea ice thickness could contribute to net shortwave changes, in addition to the more obvious contributions of changing snow and sea ice coverage. Section 2 could be improved with a bit more quantification of how large these other contributions are. Perhaps such quantification is beyond the scope of the study, though I encourage the authors to consider ways in which they could quantify the contributions of these different drivers of shortwave flux trends. It seems that the cloud contribution could be isolated and quantified via existing APP-x data, though isolating the influence of sea ice thickness/age would be more challenging.

Related to the above point, the discussion on the top of p.4 highlights a trend of lower albedo over Greenland's near-coastal regions. Is this trend caused more by reduced snow cover over the (quite small) non-glaciated portion of Greenland, or more by darkening of the perennial ice surface?

(3) A companion analysis of MERRA2 data is referred to very briefly at the end of section 2. This is a nice addition to the study, but it would be helpful if this analysis was developed more. In particular, it would be helpful to state the shortwave trends obtained from MERRA2 and show companion figures to Figs 1 and 2, so that similarities and differences between the two products can be seen more clearly. Presenting results from two or more products will give the study more credence.

(4) Figure 5 shows that the "Low Albedo midpoint" over oceans actually occurred *before* the solstice in 2015. If this is now the norm, it implies that future trends towards earlier melt will, as with terrestrial snow, cause the high-low albedo transition to move away from the solstice. In other words, the lag between snow and sea ice melt, in combination with melt trends, may have caused the snow/sea ice forcing differences to have *already* peaked. In light of this (and if I have interpreted correctly), the authors may want to add a bit of nuance to the abstract and associated discussion.

(5) The analysis of feedbacks in section 4 should either be removed, or methodological details of this analysis need to be clarified. Personally, I think this section could simply be removed, as it does not add much to the study, and is likely sensitive to methodological choices. If it is retained, more detail is needed on methodology, including how the spatial and temporal averaging of temperature (in particular) was conducted. Were monthly or annual trends in T used? Were gridcell-level or Arctic (or global) T trends used? (e.g., how exactly were the presented June feedback numbers calculated?) Personally, I think feedback analyses are only meaningful when large areas (e.g., hemispheric or global) are used for temperature averaging. More detail would also be needed on the technique used to determine: (d alpha_p / d alpha_s).

(6) Conclusions, p8,13-17: This passage could be important, but needs clarification. To be clear, Flanner et al (2011) assumed constant seasonal cycles of the albedo of multi-year and first-year sea ice. Thus interannual changes in sea ice extent and transitions from multi-year to first-year ice did contribute to area-averaged albedo changes in that study, but changes in ice albedo due to thinning or earlier ponding (etc) did not. p.8

line 15 states that "...here we find that the inclusion of inter-annual changes to surface albedo result in a significant change to the surface shortwave energy budget...". Do the "inter-annual changes to surface albedo" refer to changes in the albedo of the ice itself? If so, I do not recall seeing quantification of this contribution in the analysis (though I think it would be very useful!). The subsequent sentence also needs clarification: "Since 2010, for example, average ocean albedo in the study area during late June has been as low as mid-September albedo in 1982-1985." Are you referring to the ocean-wide albedo, or to the albedo of the sea ice itself? If the former, this is likely due simply to the reduction in June sea ice extent, and this would have been accounted for in the Flanner et al study. If not, this is a useful finding, but one that should be reported and further developed earlier in the study.

Minor comments:

General: I appreciate the focus of this study on the "Arctic", defined here as 60-90N, but it is important to note that any conclusion about the relative magnitudes of sea ice vs. snow changes will be sensitive to the latitude bands selected. For example, if the latitude threshold for the analysis was adjusted equatorward, the relative contribution of land snow changes would clearly increase. I think this point should be acknowledged more clearly.

p1,21: A slightly clearer way to say this would be "September sea ice extent decreased by 45% from..."

p2,30: 1972 should be 1979, in reference to the Flanner et al study.

p6,22: This either needs "although" after the comma, or it should be two sentences.

p6,24-30: Please list the solstice DOY, for reference.

Figure 1: It looks like there is only one point per year in this figure, so I assume you mean "Annual Absorption" instead of "Monthly Absorption".

Figure 2: Could you speculate on the cause of the negative trend in April non-land

albedo?

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