The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-115-RC3, 2018

The Cryosphere Discussions
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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 #3

Received and published: 7 August 2018

General comments:

This study examines a dataset (APP-x) that has not been considered in such detailed analysis in the past. The authors examine spatio-temporal trends in absorbed short-wave energy (and other parameters) for a time period (1982-2015) during which the Arctic land-ice-ocean system underwent major changes. The study is an original and timely contribution that is of particular value because it compares changes over land with those over the oceans, and evaluates the magnitude of trends (and indirectly, feedbacks) in detail. The paper is brief and conveys a number of key points in a small space, which is both a strength and a weakness. In regards to the latter, some of the discussion of underlying causes is too simplistic at best. Other parts of the paper would also benefit from a more in-depth, nuanced discussion. Both of these issues

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Discussion paper

are discussed in more detail below. Overall, the paper is an important and significant contribution.

Specific comments:

(1) The analysis and discussion of albedo differences and trends over sea ice (beginning on p. 4, bottom) is much too simplistic and misleading, since it implies that seasonal evolution and inter annual trends are driven entirely by changes in ice thickness. However, prior to the onset of melt the differences in snow albedo on different ice classes (with the exception of very thin ice) are likely insignificant. Much more important in this context are development, areal fraction, and optical properties of ponds on sea ice. It is here that major contrasts between different ice age (MY vs. FY ice) and ice thickness are expressed. Moreover, these processes are dominant for the months of June and July, such that the discussion of Fig. 2 (June) in terms of ice thickness classes is not really appropriate. Here, a more rigorous discussion of the observed trends in terms of the seasonal cycle of ponds on sea ice (Perovich and Polashenski, 2012; Polashenski et al., 2012; Rösel and Kaleschke, 2012a) is needed. In particular, the work by Rösel and Kaleschke (2012a,b) is highly relevant because it discusses the role of spatio-temporal variations in ponding on sea ice in the context of sea ice concentration and extent anomalies based on remote sensing data. A closer examination of their findings may help explain some of the spatial patterns seen in Fig. 2 and the inter annual variations shown in Fig. 1. It is also relevant in the discussion of reductions in albedo in the month of June (p. 4, l. 18) which is as much or more a function of ponding as of reduction in ice concentration.

(2) The authors attribute changes in albedo over land to changes in snow cover duration during the snow-covered period, and to changes in vegetation and cloudiness after loss of snow cover. This may be too simplistic and requires further analysis. First, while the albedo contrasts between clouds and snow cover may not be as large as those between land surface and clouds, they cannot be ruled out as important without further analysis. For example, the spatially coherent trend for the month of April

towards reduction in absorption of shortwave energy in NW Siberia may well be due to changes in cloudiness rather than duration of snow cover (which persists well beyond April). Without specific references to the published literature or some additional analysis of a particular subregion in a case study it is difficult to accept the explanation offered by the authors wholesale. Second, some of the discussion of snow and ice albedo variations needs to be reviewed and potentially revised. For example, on p. 4, l, 11ff a difference between snow albedo over sea ice (0.6) and land (0.7) is seen as being important. Where do these estimates come from? The paper by Sturm et al. cited here only discusses tundra snow. Both values are low if they refer to dry early spring snow before the onset of melt. Moreover, I am not aware of data showing that the albedo of snow covered sea ice is that much lower than that of tundra snow. This needs to be either corrected or further substantiated.

(3) The discussion of the spatial patterns of trends could be expanded a bit, ideally by referencing either published work or at least a slightly more in depth analysis for a subregion. It is asserted that spatially coherent trends such as that in Greenland or Siberia are driven by trends in cloudiness. This appears plausible, but would benefit from some more detail. However, this raises the question as to what spatially heterogeneous trends are driven by (e.g., April over much of the landmass, or June in much of North America). Are the trends for individual grid cells or small aggregations of grid cells significant if they are neighboring on grid cells with opposite sign in the trend?

(4) The previous comment relates to a significant shortcoming in the manuscript that should be easily remedied. Specifically, the discussion of the methods employed in deriving the different data sets and their analysis is currently much too superficial. First, it would be preferable to separate the description of the datasets used and the analysis methods employed from the reporting of results. Specifically, l. 24 on p. 3 would be a natural break. Then, while reference to the paper by Key et al. (2016) to describe the data product is fine, the current paper needs to provide more information on how the data sets were generated in particular as relevant to the specific variables

(albedo and absorbed shortwave energy, for example) discussed here. For example, how has broadband albedo been derived from spectral radiances and what are the associated errors and uncertainties associated with such a derivation? With the ocean regions located at higher latitudes than the land areas, does this introduce a potential bias because of lower solar zenith angles relative to sensor zenith angles? With a lack of bireflectance distribution function (BRDF) data over melting sea ice as opposed to snow cover over land this may introduce significant uncertainties as well. Further details for the MERRA reanalysis products should also be provided in a restructured methods and data section.

Specific edits: - p. 1, l. 4: For clarity throughout it may be better to refer to sea ice albedo feedback and snow albedo feedback (or at least clarify at the start that ice albedo feedback only refers to sea ice) - p. 1, l. 12: reads a bit awkward, maybe change to something like "the timing of the seasonal transition from high to low albedo . . . shifting towards greater insolation associated with summer solstice" - p. 3, l. 27: change to "confidence of the trends is calculated" - p. 4, l. 2: Not sure how Perovich et al. 2002 is relevant here since this paper does not discuss land or terrestrial snow albedos but multiyear sea ice. - p. 4, l. 3: These two references only touch on the radiation budget obliquely; citation of a paper with actual analysis such as Pistone et al. 2014 or Perovich et al., 2007 that provide actual attribution would be more appropriate. - p. 6, l. 7: change to "Figure 2. However," - p. 6, l. 12ff: The way this metric is described here and in the figure caption is confusing. Are you plotting the time period during which albedo falls into the interval {0.25,0.4}? Or are you plotting the days for which the albedo first drops below 0.4 and 0.25? Please clarify. - p. 11, l. 25 & 26: these papers are out of alphabetical order - Fig. 5: Units on vertical axis should be MJ/day

References cited: Perovich, D. K., B. Light, H. Eicken, K. F. Jones, K. Runciman, and S. V. Nghiem (2007), Increasing solar heating of the Arctic Ocean and adjacent seas, 1979 – 2005: Attribution and role in the ice-albedo feedback, Geophys. Res.

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