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The Cryosphere Discussions

Interactive comment on "Investigating the Local Scale Influence of Sea Ice on Greenland Surface Melt" *by* Julienne C. Stroeve et al.

Anonymous Referee #1

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I have read the manuscript "Investigating the local influence of sea ice on Greenland surface melt" by J.C. Stroeve et al. This work evaluates statistical and physical links between Arctic sea ice conditions and subsequent melt events observed over the Greenland Ice Sheet. Through a statistical framework, the authors find strong covariability between Baffin Bay and Davis Strait melt and freeze onset and ice sheet melt occurrence within close spatial proximity to the aforementioned oceanic regions. The physical associations presented between local sea ice cover and the ice sheet appear to substantiate the author's statistical findings. In particular, composite turbulent flux and wind field analyses show transport of warm, moist air from the ocean (evident in early melt years) onto the ice sheet that subsequently enhance glacial melt, especially

at lower elevations around the western and southern margins.

Overall, this manuscript is well-written and concise and I believe the quality of analyses presented are consistent with manuscripts published in The Cryosphere. This paper would make a contribution to the growing body of local/regional sea ice-ice sheet interactions within a rapidly changing North Atlantic Arctic environment. I would recommend acceptance pending the completion of a relatively small number of revisions, which I have detailed below by line numbers in the submitted manuscript.

Minor Comments:

Line 56: Add ". . .and mid-tropospheric height" after SLP to reflect coincident mid-level circulation changes in the Arctic. Papers such as Bezeau et al (2015) Int J Clim (doi:10.1002/joc.4000) could also be cited here.

Line 60: Relevant recent work by Ballinger et al. (2017) Clim Dyn (doi:10.1007/s00382-017-3583-3) similarly notes poleward advection of warm air masses delays autumn freeze onset in Baffin Bay, and impacts Greenland coastal temperature signals, and would appropriately fit here.

Line 129: Do MAR 850 hPa winds, which use ERA products, compare more favorably relative to observations than MERRA low-level winds? As 500 hPa geopotential height and 10m winds are obtained from MERRA it would seem appropriate to use a similar product for 850 hPa winds.

Lines 132-133: Clarify whether MAR output for 2002 is forced by ERA-40 and ERA-I or just one of these datasets.

Line 158: List the threshold of statistical significance used throughout the paper.

Line 193: Change "didn't" to "did not"

Lines 195-198: The authors should explicitly state what advantages SVD offers beyond more traditional bivariate correlation between two data fields? Such justification would

be helpful given that simple and partial correlation techniques are also utilized in the paper.

Line 239: Should this be Eq. 1? I do not see a second equation listed in the manuscript.

Line 241-242: When compositing by anomalous melt and freeze years using a +/-1 sigma threshold, it appears that only 3 early melt and 4 late melt onset are considered (as mentioned in Fig 12). If the sigma threshold is relaxed to increase sample size (perhaps to +/-0.75 sigma) does this substantially alter lower tropospheric wind patterns?

Line 280: Change “hPA” to “hPa.”

Table 3: Does simple correlation reference a specific technique (i.e. Pearson’s or Spearman’s)? Clarify this in the caption and table.

Figures 9a/b: Graphic is somewhat confusing with time series plots stacked directly on top of each other. I would suggest that panels be clearly separated into a two-panel plot (labeled as a-d for instance) with y axis labeled accordingly on the correlation time series.

Figure 12: Are these winds from AIRS or MERRA? The manuscript explicitly mentions use of MERRA 10m winds (line 160), but not from AIRS.

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