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Interactive comment on "Greenland Ice Sheet late-season melt: Investigating multi-scale drivers of K-transect events" *by* Thomas J. Ballinger et al.

Anonymous Referee #1

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—— General Comments ——

In this study, Ballinger et al. investigate potential mesoscale and synoptic drivers of late season melt events in the K-transect sector of southwest Greenland. They find that synoptic scale processes are the primary driver, with late season K-transect melt events characterized by southerly flows of warm, moist air from lower latitudes onto the ice sheet. In contrast, local ocean-atmosphere interactions over Baffin Bay are not found to have any direct role in driving late season melt events.

This study provides novel and valuable insights into the variability of GrIS melt, identifying an important link between synoptic scale moisture transport and unseasonal melt events, which has not been previously explored. Overall, the analysis is well done and the manuscript is well written. My main concerns are: (i) the robustness of the results

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given the small number of years analyzed, (ii) lack of clarity regarding the initial hypothesis of marine layer forcing (what exactly would be expected if melt events were driven by marine layer forcing, and how do the actual results differ?), and (iii) potential issues with the interpretation of turbulent heat fluxes and associated arguments about the origin of warm, moist air that is transported from the south onto the GrIS. I discuss each of these below, along with other minor comments and suggestions for improvement.

—— Specific Comments ——

L32 “For the unseasonal melt period preceding the DOA”: Which unseasonal melt period? This seems to imply a sustained period of unseasonal melt before the DOA, whereas the unseasonal melt usually occurs in transient events. I suggest rephrasing as simply “For the period preceding the DOA”.

L40 “While thermal conduction and advection off south Baffin Bay open waters impact coastal air temperatures, consistent with previous studies”: Where is this shown in the manuscript?

L47 “The Greenland Ice Sheet (GrIS) surface mass balance (SMB) decrease has contributed roughly 0.5 mm year-1”: It’s the overall mass balance decrease (SMB + ice discharge) that has contributed 0.5 mm/year, not the SMB.

L50 “west Greenland waters” -> “ocean waters west of Greenland”?

L83 “recent, temporally-anomalous GrIS late melt events spanning the end of summer to freeze-up of adjacent ocean waters”: Which recent events? Please provide a few examples and references.

L112 and L156: Can you provide some further explanation as to why KAN_B was selected to build T+/T- composites, thus limiting the study period to only 5 years? Especially since the objective is to study melt events, why choose a station on the tundra instead of a station on the glacier? KAN_B has a much shorter data record than all the other K-transect stations, so the selected study period 2011-2015 is only a small sub-

set of the available data. With the short study period and with composites sub-divided into D_60 and D_30 bins, the D_30 composites comprise only 13 days of data.

The next closest station, S5, has data going back to 2003, which would allow for a more robust analysis over a 13 year study period. If you repeat the analysis using S5 to build composites over 2003-2015, do the results shown in Figs. 3-6 and S4-S5 change very much? I think it would be preferable to use station S5 and the longer study period for these figures, or at least mention whether the KAN_B-based results are robust when S5 and the longer period are used. The only place I see a need for a shorter study period is in Table 2 and Fig. 2, where all the K-transect stations are compared over an overlapping period, but if KAN_B were excluded then I think the overlapping period could still extend back to 2008 instead of 2011.

L163: It would be helpful to summarize from Table 2 the frequency of T+ events at KAN_B (or S5, if it were used instead of KAN_B to build composites), and mention what percentage of days each represents, i.e., the D_60 bin has 69 events (∼46% of days) and the D_30 bin has 13 events (∼9% of days).

L176 “IVT PR is then classified using the SOM technique to produce a matrix of moisture transport patterns, or nodes, that typically occur over the Greenland region”: This is very vague. Some additional description should be included for the benefit of readers who are not familiar with SOM - for example, mention that it is an unsupervised machine learning algorithm and that each daily IVT PR field is classified with its closest matching node from the SOM.

Are the SOM dimensions and training parameters the same as in Mattingly et al. (2016), or is anything different? It’s not clear from this description. It would also be helpful to include in the Supplemental Materials a figure showing the SOM nodes and the wet/dry/neutral groups, so that readers have a better understanding of the SOM classification used in this study.

L178 “As in Mattingly et al. (2016), similar wet (anomalously high), neutral (near cli-

matological median values), and dry (anomalously low) IVT patterns are aggregated, then their frequencies are composited and tested following methods previously summarized": What is meant by "tested" in this context? In the cited paper, the clustering of nodes into wet/dry/neutral groups was subjective, based on visual inspection. I don't recall any testing of this grouping procedure.

L208-231 I find it difficult to follow the reasoning in these two paragraphs that leads to the conclusion that "late season melt inferred from T+ events may be driven by synoptic patterns as opposed to local marine forcing" (L234). Why exactly do we reject the hypothesis that local marine forcing drives the T+ events? Is it simply because the K-transect wind directions in T+ events are offshore instead of onshore? What about the other aspects discussed here – such as K-transect wind speeds, sensible and latent heat fluxes over ocean and GrIS, near surface winds in eastern Baffin Bay, etc. – how do they contribute to this argument?

These paragraphs should be revised to clarify: if local marine forcing were driving the T+ events, then what would we expect the results here to look like? How do the actual results differ from that?

L214-218 and Figures S4-S5: Sensible and latent heat fluxes in Figures S4 and S5 are described here as positive in the upward direction. Is this correct? I would have expected sensible heat fluxes over GrIS to be directed from the atmosphere to the ice sheet surface (i.e. negative SHF if the upward direction is positive). In particular, in the K-transect region during T+ events, if air temperature is greater than 0 C but the melting ice surface is at most 0 C, how can SHF be directed upward?

L240 "In both T+ cases, low-level winds circulate poleward over north Labrador Sea areas of upward, turbulent heat flux (Figs. S4 and S5), aiding the heat and moisture transfer (as shown by heightened IVT values in T+ relative to T-) over western Greenland during D60 and D30 (Fig. 5)": Following from my previous comment about the direction of SHF and LHF in Figures S4 and S5, I am wondering if turbulent fluxes over

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the north Labrador Sea are actually upward and if this region is actually the source of the heat and moisture transported onto western GrIS? Or do the heat and moisture originate from further south, around 30-35S? Subtropical origin seems more likely and is more consistent with the IVT fields shown in Figure 5.

L254 “higher 500 GPH values and on-ice lower tropospheric mean winds”: The phrasing here seems confusing. “Higher” refers to GPH being higher in T+ relative to T- events, but does “lower” refer to wind speeds in T+ relative to T-, or does it mean “lower troposphere”?

L267 and Figure S6: Since each station-year is a separate data point, why not include all years of data for each station in Fig. S6 and related discussion? Only using the 2011-2015 period gives a very limited perspective on whether the temperatures are typically correlated or not, since we see statistically significant correlations in only three of five years.

—– Typographic Corrections and Figure/Table Formatting —–

Headers for sub-sections 2.2-2.4 should be in bold for consistency with the other headers.

Table 2: I find the labels here a bit confusing. Although each row is labelled as a relative measure (e.g., “S5 vs. KAN_B”), the columns “T+ n[-60, -31]” and “T+ n[-30, -1]” are not relative to KAN_B, they are the actual counts for each individual station.

Figures 2 and 6: It’s difficult to distinguish between the dark and light red/blue colors in the bar charts.

Figures 3-4: It’s difficult to distinguish cyan arrows over blue background. It’s also difficult to see the wind vectors for the K-transect stations - perhaps these arrows could use heavier line weights and/or different colors?

Figure 5: Many of the GPH contour labels are cut off at the edges of the figures.

L518: The last four references are not in alphabetic order with the rest of the bibliography.

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