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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.

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General comments. The Discussion Paper by Ballinger et al. is an interesting and novel attempt based on in situ, Automatic Weather Station (AWS) observations and Regional-Climate Model (RCM) output to identify the prevailing atmospheric-circulation pattern that causes unseasonal melt events in the southwestern Greenland ice sheet, and whether sea-ice formation and related turbulent-heat production over Baffin Bay in early autumn is of importance.

The study is characterized by an accurate and well-structured methodology, but in my opinion, not also interpretations. As an example, the authors conclude that they "find no evidence to support the hypothesis that local open water and resultant turbulent heating

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has a demonstrable impact on inland ice melt events", even though the approach is purely statistical; the study does not disprove the hypothesis, but rather proves that it is not the dominant mechanism.

Structurally, the study suffers from obvious overlooks and requires attention. As examples, I note the allegedly examined AWS pressure and humidity according to the Abstract, even though the analysis is based on AWS air temperature and wind properties only, and the mention of a Sisimiut AWS in the Results and of an Upernavik AWS in the Discussion, without being previously introduced or presenting any related figure in the main body.

I recommend a major-revisions status, primarily to provide the authors with ample time to improve the structure and reasoning throughout the text. I also have a comment on the methodology concerning the AWS analysis that, in my opinion, needs to be addressed before publication.

Specific comments

The linguistic level is good, however sentences tend to be condensed, and would benefit by a more elaborate style. Since the study is not purely meteorological and aims potentially to appeal to a wider Earth-sciences audience, some effort should be invested into familiarizing readers with meteorological norms instead of just mentioning parameters, numbers, and units. Additionally, the reader's knowledge over methodological approaches is taken for granted. As a rule of thumb, a second-year Earth-sciences master's student should be able to follow without referring to textbooks for clarifications.

The study has a broadly clear structure, but the argumentation is not straightforward, and complicated – sometimes even speculative – discussion points are widespread in Results and Conclusions. Also, as a rule of thumb, referencing supplementary material in Results or other studies in Conclusions should be avoided, in order for the study to be self-standing.

Effort should be invested into describing the theoretical background and the study area, particularly its wind regime and mechanisms, as well as recent Surface Mass-Budget changes within the Introduction or before Data as a self-standing section, so that readers can easily relate afterwards to the Discussion. Recommended literature: Van Angelen et al. (2011), Van As et al. (2014), and the classic GIMEX-90/91 work. The inclusion of a description of the different areas of a glacier, as well as an Equilibrium-Line Altitude definition and how high it is in the study area, is also recommended.

The use of KAN_B observations as basis for the analysis needs to be motivated better than just saying that it records more above-zero Celsius air temperatures than all the other K-Transect AWSs. The way I see it, by the end of August, after the end of the melt season, the increased solar-zenith angle and ice-sheet surface albedo limit the influence of solar radiation on potential ice-sheet surface melt, which is thereafter driven by climatic drivers influencing downward longwave radiation (i.e. cloudiness) and turbulent fluxes (e.g. September 2010; Charalampidis et al., 2015). Also, katabatic-wind flow, driven by the balance of downward sensible heat flux and radiative cooling at the surface, becomes gradually colder and denser, and therefore more intense and laminar, thereby hindering turbulent mixing over large part of the AWS transect that is characterized by decreased surface roughness (Smeets and van den Broeke, 2008; see also specific comments for L208-209 and L211-218). Hence, melt events might occur in cases of weak katabatic-wind flow and concurrent northwesterly atmospheric advection from Baffin Bay, or moderate to strong katabatic-wind flow and concurrent southerly barrier winds along the ice-sheet margin driven by synoptic circulation.

Located only 1 km away from the ice-sheet margin, KAN_B should be the station most sensitive to both these circulation patterns. In both cases, the local boundary layer will become more humid (the role of humidity on energy balance and as a cause of surface melt should be explicitly described), and KAN_B might record positive air temperatures, in which case it is also worth inspecting all on-ice AWSs for positive air temperatures, which would be indicative of surface melt. (It should be explicitly mentioned in the

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text that positive near-surface air temperature is used as an indicator of melt at the ice-sheet surface.)

The statistical analysis should reveal which of the aforementioned cases is the most frequent (and not simply differentiate between regional meteorological processes acting during positive and negative KAN_B air-temperature events, as currently stated in L154-155. What goes on during negative KAN_B – and hence K-transect – air temperatures is not the focus per se; it is just used as means of comparison). The above theoretical introduction material along with Figure 2 alone proves, in my opinion, that synoptic circulation in the South of Greenland is the driver of unseasonal melt events mostly in the lower ablation area until the elevation of KAN_L. The persistent katabatic-wind regime acts otherwise as a shield against turbulent forcing from Baffin Bay. All the rest should be an elaboration around this key result.

It might be insightful to make a similar comparison of KAN_B with observations further away from the ice-sheet margin. I do not know what the result might be, but I am guessing weakening southerly wind away from the margin in case of synoptic-driven, barrier-wind occurrences, and otherwise more intense winds than whatever KAN_B is reporting at that time, directed toward the ice sheet.

Nevertheless – and this is my major methodological concern – it is not clear in the text how the KAN_B temperature events are defined. Are these definitions based on hourly or daily observations? Is there a time window plus/minus delta_t around a warm/cold KAN_B event t within which the conditions at all other stations are evaluated? Is delta_t selected larger or smaller depending on the duration of the KAN_B event t? Eventually, how sensitive are the statistics, and how conclusive the implications, between hourly and daily analysis? Please, elaborate.

Technical comments

Please, make use of dashes, or hyphens if appropriate, throughout the text to facilitate readability in case of several nouns in a sequence, e.g. in the Abstract alone

L27: air-temperature episodes; L29: open-water duration; L30: sea-ice advance; L31: sea-ice growth; L38: late-season, ablation-zone melt events, but also L129: "Surface*hyphen*atmosphere features" Also, differentiate between dashes and minuses.

Use the more appropriate "area" instead of "zone" throughout the text.

I recommend the use of "observations" instead of "data", while differentiating between in situ and remotely sensed, and "simulations" for modeling products.

I recommend the use of "average" instead of "mean".

Except for "Buffin DOA", complete "Buffin" as "Buffin Bay" throughout the text.

Capitalize the first letters of every word when an abbreviation is introduced, so that the reader's eye can make a quick connection, i.e. L30: Date Of sea-ice Advance (DOA).

L27: delete "significant", and reserve it exclusively for describing correlations.

L32: "unseasonal melt events. . ."

L34: "Southwest" or "southwestern"

L34: The influence of synoptic and mesoscale systems on the above- and below-freezing near-surface air-temperature events. . .

L35: "AWS" -> "the"; "The in situ observations. . ."; Why are pressure and humidity observations mentioned here? I was unable to spot them later in the study. Why is wind speed and direction not mentioned?

L36: "against" -> "with"

L35-38: I suppose "MAR, RACMO2, and ERA-Interim are used to provide context to the in situ observations by explaining the air-mass origins and the (thermo) dynamic drivers of melt events." As it is written now, it gives the impression that the in situ observations need to be calibrated against RCMs.

L41: Delete "consistent with previous studies". Try not to refer in the Abstract and

Conclusions to other studies.

L42: Consider removing “pressure-gradient driven” from the Abstract, and describe briefly the mechanism in the Introduction.

L42: “Katabatic-wind regime”, since the word “katabatic” is an adjective referring to a descending object, in this case, wind.

L41-42: I think this is an overall complicated explanation. I think the primary obstacle of Baffine-Bay marine-air intrusions over the ice sheet should be intuitively the persistent katabatic wind regime that intensifies in the beginning of autumn. Additionally, barrier winds might be present, in which case melt might occur due to air-mass transport from the Atlantic. I propose restructuring as: “. . .are obstructed by the persistent katabatic-wind regime flowing downslope from the ice sheet, and the occasional occurrence of barrier winds along the ice-sheet margin.”, or something along these lines.

L44: “Substantial mass losses. . .”

L46: “have become sensitive” or “are becoming increasingly sensitive”

L48: Since all Van or Van de Author have been categorized under V in References, consider capitalizing the first letter of “Van” in the in-text occurrences, as well as that of the first author in the References, in order to allow the reader to navigate through easily.

L53: Replace “play a key role” by “are of importance”

L54: “surface melt at the southwestern GrIS.”

L55: “Conflicting evidence have been presented in literature over the past decade regarding. . .”

L56: “Regional-climate simulations. . .”

L60: “noted that. . .”

L61: Replace “which” by “and”; replace “by” by “to”.

L63: “in Ilulissat and Nuuk, approximately 200 [Unit] to the north and south of Kangerlussuaq, respectively. . .”

L66: Replace “coupled” with “correlated”.

L67: Please, delete “robust”. Statistical quantities are a matter of context, and their interpretation is dependent on the datasets and the research question itself, which in this instance are not present; “(from Markus et al., 2009)”

L69: “The authors found that significant, positive correlations between Baffin and Labrador SST and coastal SAT often persist. . .”

L70: “onset of freeze” should suffice; “Applying a similar correlative approach on. . .”

L71: “while utilizing melt/freeze product. . .”

L74: “Both studies indicated. . .”; replace “upper-level” with “high-altitude”.

L75: Replace “at the limits of” with “toward the end of”.

L81: Replace “research studies” with “literature”.

L86: Delete “automatic weather stations”, since AWS has already been introduced.

L99: “Daily observations are available over the 1979–2015 period at a. . .”

L101: Delete “day of sea-ice advance”, since DOA has already been introduced.

L102: Bliss et al. (in review)

L112: “in this study (Fig. 1).”

L113: “KAN_B that is situated approximately 1 km away from the ice-sheet margin. . .”

L117: Delete “tundra (KAN_B only) or glacier ice”

L116-117: The observational distance from the surface decreases during winter sea-

son due to the accumulating snowcover around the AWSs by as much as two meters depending on the location along the transect, while it increases again each melt season until the complete ablation of the accumulated snowpack. In the case of KAN_U that is located in the lower accumulation area, this distance tends to become shorter over the course of a few positive mass-budget years due to the incomplete ablation of the snow cover, but also due to the sinking of the AWS tripod during ablation in the temperate firn below, as discussed by Charalampidis et al. (2015). After the extreme 2012 melt season, snow accumulated around KAN_U during the 2013 and 2014 positive mass-budget years, while on 3 May 2015 the half-buried tripod was replaced.

L121: Consider including also Citterio et al. (2015) describing the PROMICE AWS.

L125: Delete “have been shown to”

L127: “from between 1000–200 hPa height, corresponding to the distance between sea level and lower stratosphere at 67 N (e.g. Zängl and Hoinka, 2001). . .”

L129: “Surface*hyphen*atmosphere” since the two are related/interacting.

L130: Replace “features” with “interactions”. Alternatively, replace “Surface–atmosphere interactions” with “Boundary-layer processes”; 500 hPa height (i.e. ∼5000 m above sea level)”

L132: “10-m above surface and 850 hPa height. . .”

L133: “low-level atmospheric flow. . .”

L143: “ice caps, and improving. . .”

L145: “to characterize near-surface. . .”

L151: It should be mentioned that above 0 C air temperatures at on-ice AWSs are considered an indicator of ice-sheet melt.

L152-165: It is not clear how the KAN_B temperature events are defined. I refer to

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L157: “due to the station’s location. . .”

L171: “Based on RCM output, . . .”

L172: Delete “integrated water-vapor transport”, since IVT has already been introduced.

L183: Refrain from mentioning supplementary material as part of the text, and only refer to them in parentheses at the end of sentences. Nevertheless, supplementary material should be referenced outside Results, and if Fig. S1 should be referenced in the first sentence of the Results, perhaps it belongs in the main article.

L189-191: Nice, but perhaps also mention that the difference has narrowed primarily due to the prolongation of the melt season over the course of the thirty years.

L193-195: It is a bit awkward to see all of a sudden a Sisimiut station being mentioned here in the first subsection in Results even though it has not been introduced earlier, and a case being made based on supplementary material. Please, restructure.

L195-199: This belongs in the Discussion.

L201: I am not sure I understand what this first sentence is trying to justify. Also, sounds like it belongs in Methods.

L203: Does the term “composite” essentially refer to the average of all instances? Please, clarify.

L206: Second half of the sentence refers to T above 0 C events? Please, clarify.

L208-209: South-southeasterly winds should have a 157.5 degrees direction, given the orientation of the PROMICE AWSs, so what is now written is incorrect. These winds in autumn are katabatic with a downslope direction (90 degrees) deflected 45 degrees to the right by Coriolis force (i.e. southeasterly direction; cf. Van den Broeke et al., 2009),

plus potential southerly synoptic influence. Nevertheless, this quantification seems to be more prominent above the long-term ELA and KAN_B. Good thing to also mention.

L211-218: This belongs in the Discussion. Nevertheless, I am not sure I agree with this interpretation, and I explain: Strengthening wind speeds during positive air-temperature events at KAN_B do not reflect strengthening of katabatic flow (i.e. cold, dense wind), but rather that additional wind components inducing turbulent mixing might be present. A strengthened katabatic flow would remain cold, and would be more laminar, and hence less turbulent, than usual. Katabatic wind could enhance turbulent mixing during melt season at the lowermost rough-surface parts of the transect, but we are discussing unseasonal melt events, implying that sunlight is reduced, hence surface melt is not sustained in the way it does during the melt season, while surface roughness below ELA might be substantially decreased due to accumulating snow cover, and above ELA slightly increased for the same reason, i.e. sastrugi formation. The observations from most AWSs within the same averaging periods suggest southerly deviation during positive air-temperature events from the cold-event direction, and the way I perceive it, southerly synoptic influence. I note that Figure 2 and Table 2 suggest that melt might occur primarily at the two lowermost on-ice AWSs, while wind speed as well as wind-speed differences between positive and negative cases within the same averaging periods are more pronounced at higher elevations due to comparatively reduced surface roughness (cf. Van den Broeke et al., 2009).

L214-215: Positive energy flux that contributes to ice melt is most definitely downward, i.e. directed from the atmosphere toward the ice-sheet surface, and not necessarily the result of increased wind, rather the result of increased turbulence; “Sensible heat fluxes” does not sound nice. Consider removing the plural.

L219: The first sentence is not a result. Please, delete or relocate to Introduction.

L222: “at KAN_M and S9 in the upper ablation area.”

L223: Replace “modeled” with “simulated”.

L223-225: Difficult sentence, consider revising. Also, please, relocate in the Discussion.

L228-229: This sentence, as it is, belongs in the Discussion. You can reformulate and keep in the Results as: “We note that there is a height difference between the RCM 10-m above-surface output and the measuring heights of the AWSs, as mentioned in the Data section.”, or something like this.

L233-234: This first sentence is Discussion material. Please, relocate or reformulate, since it sounds like a rather abstract introductory sentence referring to the previously outlined results.

L239: Explain the significance of 540 dam (i.e. 5400 geopotential meter), i.e. it often distinguishes solid and liquid precipitation.

L255: The link between moist air masses and how they may facilitate ablation-area melt is not clear. Please, elaborate preferably in the Introduction.

L261: Include a short comment on strong negative NAO phases, as seen in monthly observations.

L266: “turbulent atmospheric heating”

L267-269: Please, refrain from referring to supplementary material as main part of the text, i.e. delete “As shown in Fig.S6”. Instead, include a citation at the end of the sentence. It should be mentioned that it is a moderate to weak link that seems to be year-dependent. The way I perceive it, this differentiation amongst years suggests synoptic-circulation control that may or may not be present each year, and that is a good comment to include before anything else. (For example, Fig. S6 suggests weak correlation in autumn 2013 between local sea-surface temperature and AWSs, when limited melt occurred even at KAN_U (September 2013; Charalampidis et al., 2015).)

L275: “statistically robust co-variability”: Please, remove “robust” and reformulate.

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L271-279: This part seems somewhat arbitrary, since no evidence of a different wind regime between 2011–2012 and 2013–2015 was presented. Also, I am not sure I agree with the categorization. Please, revise/clarify.

L287: Mention also the distance from Kangerlussuaq.

L290-294: This is highly unclear and speculative. Please, revise.

L295: "appears to be of minor importance. . ."

L304-306: Please, clarify.

L306: "Denmark Strait and Irminger Sea at the East coast of Greenland. . ."

L316: Generally, Conclusions outline key findings in the Results and key Discussion points. The current state appears more like just another part of the Discussion, while the only Concluding bits are between L319-324 and L341-345. Please, consider rewriting the whole section outlining important quantifications from Results. Please remove all citations, as the Conclusions should refer only to the present study, and should be self-standing.

L317-319: Consider simplifying; rephrase "around the limits"

L377: Delete "2018"

Table 1: Include a minus in front of the 1 km of KAN_B, so the reader can immediately see that it is different from the rest of the stations. "50*hyphen*150 m"

Table 2: Were these daily averages? Please, clarify. Note that dashes have been used instead of minuses. Also, make column lines between columns 3-4 and 5-6 thicker to facilitate the eye of the reader.

L540: Replace "30 to 60-day window" with "time window defined by the 60th and 30th day before day of ice advance (DOA)"

Figure 2: Include the legend that is shown in panel a also in b and c. In panel c, in-

clude also N-S-E-W. In panel d, are these points in reference to daily or hourly values? Please, specify.

L551: Define “composites” better.

L552-553: Difficult to fathom. Please, clarify.

L553: “selected”

L569: Spell out IVT and include abbreviation since it is used as such in the figure.

Figure 6: Include legend in both panels. In the caption, spell out all abbreviations and include abbreviation in parentheses.

L754: Define “composites” better.

L577-578: Difficult to fathom. Please, clarify.

Table S1: Dashes as minuses. Please, correct. “2011*hyphen*2015”

Figure S2, S3, S4, S5: Spell out DOA initially, and then use abbreviation.

Figure S4, S5: Define composites better.

Figure S6: Introduce SST properly in caption.

References

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