The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-285-RC3, 2019

The Cryosphere Discussions
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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 #3

Received and published: 17 February 2019

General comments:

Ballinger, et al propose a hypothesis for the onset of late-season melt events on the Western Greenland ice sheet. Some prior literature has suggested that open-water ice-free conditions in Baffin Bay cause melt incursions onto the ice sheet, but Ballinger, et al use weather stations data, regional climate models and reanalysis products to make a case that incursions of North Atlantic warm air are responsible for these late-season events, regardless of sea-ice conditions.

Overall, the hypothesis is novel, and I applaud the authors for bringing in multiple datasets to support it. As one of the other reviewers also noted, I am, however, very concerned that the short time series (2011-2015) limits the conclusions that can be made with any statistical certainty. From the evidence given, I would say the study

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Discussion paper

is suggestive, but not conclusive in its arguments. In my opinion, the authors have a choice: either use a longer time-series of measurements or model data (for instance, keep the 2011-15 analysis of AWS data but make the regional climate data analysis span from 1979-2015, which is possible given that the two datasets aren't even used in direct comparison to each other), *or* soften the conclusions to make it clear they are suggestive but not entirely conclusive given the short time series. With only 5 years at your disposal, random noise can easily be interpreted as interesting new patterns.

As always, I acknowledge that the authors have spent more time considering this paper than reviewers spend reading it. If authors believe my judgements are unfounded or based on a misunderstanding, or if I just missed something in a comment, they may of course make that case upon resubmission to the reviewers or editor. Overall I do think the study, and the hypothesis put forward, is quite suggestive and worth publishing for that reason, even with the concerns cited, but the concerns should be addressed.

Specific comments:

L31-32: "For the unseasonal melt period preceding the DOA" . . . this make it sounds as if it's referring to a *specific* melt event that hasn't yet been mentioned. Generalizing the sentence more to something like "For periods of unseasonal melt preceding the DOA,. . ." may be clearer to the readar.

L34-35: "the above and below freezing surface air temperature events. . ." same comment as above.

L83-84: "temporally-anomalous GrIS late melt events" : in much of the text, terms such as "unseasonal" and "temporally-anomalous" are used, but never really defined. Does is refer to melt events after a certain date/season? Recent late-season melt events that are more frequent than previous periods? The descriptor is a bit vague here.

L104: (quick format check) Several headings are not "bolded", while the rest are. Quick fixes.

L119: “erroneously low values are filtered out prior to analyses” : Which variables were filtered in this way: temperature, wind speed, others? If you can, be more specific.

L152: “A composite approach is applied to characterize atmospheric conditions. . .” : From what I saw, I couldn’t find an explanation in the text of how these composites were put together, or what exactly the reader is looking at when seeing the composite numbers in the tables and figures. I don’t expect the authors to provide a textbook lesson on composite statistics, but a brief 1-3 sentence explainer for the reader would help reproducibility and clarity of the text. Right now, if I took the same data and tried to reproduce the results I would have no guidance of how to perform the composite analysis other than the authors’ word that they applied a “composite analysis.” More specificity is needed here to make the results reproducible. If the method is identical to one used in other papers, a citation may suffice.

L162-165: The description of the timespans used in the composite approach is good, I would put this at the top of the paragraph before describing why composite approaches were used and what constraints were placed upon them.

L183: “Perspectives on. . . are shown” is a vague term, almost meaningless. If the sentence is meant to point out something specific that Figure S1 shows, describe it explicitly.

L183-199: The first several paragraphs of the Results refer to figures that are almost exclusively in the supplement (Figs S1-S3, with exception of the Fig. 1 map), which makes it awkward for the reader to follow along. These figures appear to be central to the results, not just supplementary. Unless you are limited by the number of figures, consider putting some of the these in the central text rather than forcing the reader to flip back and forth to an entirely separate document just to follow along with your argument.

L192-193: “Relative to the climatology. . . date in 2012-2015 (Table S1).” It is unclear why 2011 is separated from 2012-2015 here, or why this sentence exists at all. Inter-

annual variability has always been present, and no evidence is provided that this juxtaposition of years shows any more significant differences than any other random 5-year period in Greenland's climatological history (perhaps it is, but it isn't demonstrated). If not, just omit it.

L195-199: "Interannual differences. . . found in the east and north (Curry et al., 2014)." These sentences don't show new results, but discuss the context of other literature. Consider moving to Discussion.

L201-202: "The spatial coherence of observations across the K-transect along with inhomogeneous GrIS Region 3 spatial melt patterns and satellite pixel contamination issues at the tundra-ice interface, lead us to assess the melt events at the station level." It's very unclear what is meant by "inhomogeneous GrIS Region 3 spatial melt patterns", or why that would motivate a K-transect station-level approach. Please clarify. Similarly with "the spatial coherence of observations across the K-transect".

L208-209: ". . .and comparatively becomes slightly more southerly" One issue with the way these composite records are presented is that there are no uncertainties or spreads presented with them at all, other than which ones are/aren't statistically significant. Thus, comparisons between them, such as "slightly more southerly" are impossible to make without knowing whether or not the difference are simply within the noise of the two datasets, or are just part of statistical noise. This is the case with all the comparisons, actually, and it makes the conclusions difficult to defend.

L223: "Modeled wind speeds are more intense during T+ versus T- events" How much more intense? 2 %? 150 %? As noted in the comment above, is the difference greater than statistical noise?

L227: "with low root mean squared errors (not shown)." Unless I'm misunderstanding this statement, RSMEs for two values should just be two numbers. They can be stated explicitly instead of just saying (not shown). If they weren't computed, this statement should be omitted.

L227: A slight positive bias in both models is evident. . .” I assume a slight positive bias in wind speed? Clarify so that the sentence can stand on its own without ambiguity.

L235: The “North Atlantic region” is not explicitly defined here, it is just vaguely referenced even though the authors are clearly looking at particular portions of the map. This ambiguity makes the rest of the entire paragraph extremely difficult to decipher from a reader’s perspective, given that different portions of “the North Atlantic” behave in different manners in Figure 5.

L235-238: “Whereas T- events “left panels” tend to be characterized by northerly winds over the 1000-700 hPa layer, . . .” It is extremely unclear what is being referred to here. Are these northerly winds specifically in Baffin Bay, or over the whole figure, or. . . ?? Also, this sentence is disjointed and somewhat convoluted, its meaning unclear to the reader.

L238: “found over ice sheet” –> "found over the ice sheet"

L238-239: “with the 540 dam contour” The 540 dam contour is very unclear in most panels of figure 5. Consider making it clear to the reader exactly which contour is 540 without them needing to interpolate between other lines, if you’re going to make a point specifically about the 540 line. Make it easy on your readers to see your point. (Also, as another reviewer noted, make contour labels fully visible, not partially cropped by the panel edge.)

L246-255: I found this paragraph particularly good and compelling.

L265: “toward earlier (later) melt (freeze)” I understand this sentence construct, and occasionally it’s useful, but it’s also extremely awkward to read and should only be used when necessary to save space. In this case, “toward earlier melt and later freeze” says the same thing with only one more word, and is much clearer English for the reader to understand.

L265: “This hotspot of melt” It’s unclear if you’re referring to a particular hotspot of melt

referred to earlier (if so, point it out), or making a more general statement here.

L267-269: “Sisimiut SSTs fluctuate... ablation zone at S9.” It’s unclear why this sentence is important. Additionally, it’s unclear that a statistically significant difference can be inferred at all if it only happened in 3 of 5 years, but not the other two. Even with just 5 years of data it’s hard to make firm conclusions about climate patterns... moreso if just picking three years selectively out of those five. This needs to be better justified, or omitted completely.

L271-279: The two points made at the end of this paragraph seem difficult to support conclusively with only 5 years at your disposal, comparing the first two years to the second three. The problem with such a short time series is that signals can be easily interpreted from random noise, making such conclusions problematic at best. This is emblematic of the greatest weakness of the whole paper, making strong conclusions about climate patterns from only five years of data. It is unclear that if you had a 30-year record (long enough to infer at least some of the variability of the patterns you describe), that the same inferences could be made in any significant way.

L289: “near-surface air penetrates at least to” –> “near-surface air often penetrates at least to” (There isn’t any indication that it always does.)

L300-301: “positive (negative) GBI (NAO) values” –> “positive GBI and negative NAO values” (more readable)

L332-334: It is good that the authors recognize that a longer time series would help with these analyses. It is still unclear that all of the conclusions in the paper can be made so confidently with the short time series available, and that some of the results are more just “suggestive” than “conclusive.”

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