Response to reviewer comments

We thank all reviewers for their helpful comments. Please find below our responses in blue.

Response to Reviewer #3

Summary: In this paper, the authors undertake an analysis of mid-winter snow melt events across land areas of the pan-Arctic domain above 50°N using microwave remote sensing. An algorithm is developed to infer liquid water in snowpacks using variations in surface brightness temperatures from SSM/I and SSMIS over 1988-2013. Mid-winter melt events are relatively rare with ≤7 occurrences (days) each year across most areas under study, with higher frequencies in temperate regions. The spatial patterns in winter snow melt events inferred from air temperature obtained from reanalysis products concur with those detected by the microwave remote sensing data. Further analyses reveal few statistically significant trends in winter melt events with the notable exception of northern Europe.

This is an interesting paper with novel results and it should be suitable for publication in *The Cryosphere* following some moderate revisions. My report provides guidance on how the paper should be revised prior to publication:

We thank the reviewer for the positive feedback.

General Comments:

1) In-text references do not follow the format used by *The Cryosphere*, i.e. round rather than square brackets should be used for references.

Square brackets are allowed according to instructions on TC website: http://www.the-cryosphere.net/for_authors/manuscript_preparation.html

2) Has validation of the proposed algorithm been performed in regions other than Canada and Finland, such as Russia and Alaska?

Yes – The algorithm was developed/validated with observations at the WMO weather stations across the pan-Arctic as shown in Figure 5b. Note the validation results using the weather station data are presented in the Data and Method Section (Lines 160-169). However, the in situ field measurements (snow survey and surface-based radiometer data) were only collected by the authors in Canada.

3) At times snow melt events occur just below the surface of the snowpack – is the proposed methodology able to detect such events?

This is probably not common during the winter. The melt detection algorithm is based on the sensitivity of microwave signal to the appearance of liquid water in the snowpack (surface or subsurface) - there is a sharp decrease in T_BD from dry to wet snow transition. Thus it should be able to detect subsurface melt events as well. However, detection of sub-surface melting is similar to a mixed-pixel effect (presumably dry/frozen surface and wet melted sub-surface), and thus would be hard to quantify at the satellite scale. Figure 4 provides some evidence that the F/T signal from uneven surface and sub-surface re-freeze likely becomes muted relative to the initial onset of melt. See the Results section on lines 281-283. We have also added the following sentence in the Discussion and Conclusions Section (Lines 378-379).

"The algorithm should also be able to detect subsurface melt events although this aspect was not evaluated in this paper."

4) The results presented in this paper focus on terrestrial snowpacks – can the methodology also be applied to snow on sea ice?

Good question. Similar channel difference approaches have also been used for snowmelt onset detection over the Arctic sea ice [e.g., Drobot and Anderson, 2001]. However, the emissivities of first-year sea ice are different than that of multiyear sea ice, and the emissivities over multiyear sea ice can have a large range due to the varied histories of the ice floes. These complicate the detection of snowmelt over sea ice, so we do not recommend the use of the algorithm developed in this study for melt detection over sea ice. A multiple indicators approach was developed in Markus et al [2009] for melt/refreeze detection over the Arctic sea ice. We have added the above to the Discussion and Conclusion section (Lines 379-386).

Drobot, S. D., and Anderson, M. R.: An improved method for determining snowmelt onset dates over Arctic sea ice using scanning multichannel microwave radiometer and Special Sensor Microwave/ Imager data, J. Geophys. Res., 106, 24,033 – 24,049, doi:10.1029/2000JD000171, 2001.

5) How reliable is the algorithm when applied to complex terrain such as the western Cordillera of North America?

Good point. The algorithm is based on the large difference of T_BD for dry snow versus wet snow (~30K), however, the range of T_BD can be much smaller (~10K) in areas with deep snow and complex terrain [Tong et al., 2010]. In-addition, changes in elevation and terrain aspect can have profound influence on air temperatures at the local scale, resulting in dramatic temperature differences over very short distances. Therefore the use of coarse resolution passive microwave satellites to detect melt events in complex terrain is not recommended. The performance of the algorithm in these areas may have a relatively large uncertainty that needs to be further evaluated. This can be an area of future work. We have added this in Section 4.

6) If only the afternoon overpasses are used to infer snow melt events across the pan- Arctic, how are melt events during other times of the day accounted for?

Good point. We have now included snow melt events from the morning overpasses as well.

7) Probability values should be reported for all correlation coefficients presented in the paper.
Done

8) The findings of recent rising air temperatures during fall (SON) with no trends in winter (DJF) and spring (MAM) across the Northern Hemisphere seem to contradict results from other studies (see Figure 12). These results should be placed into context (time period and area of interest). Why are temperature trends not reported only for the domain of study (i.e. pan-Arctic land areas above 50°N) for comparison with the snow melt analyses? Why are the seasonal air temperature trends not inferred from the Mann-Kendall test instead of linear regressions? Probability values for these trends should also be reported.

To be consistent, we have computed the seasonal air temperature trends using the Mann-Kendall test from CRUTem4 data and included the results in the text. The results are very similar to those from linear regressions. We have provided a trend map for the winter season (Figure 11).

9) Further to this, how reliable are trend analyses for a rather short (25 years, 1988-2013) period of study? Are the reported trends greater than the variability experienced over the period of study, i.e. is the signal greater than the noise in the data?

Good point. We now explicitly acknowledge this in Lines 405-408. The question of signal/noise is taken account of in the test for trend statistical significance.

10) The authors should consider suggestions for future work in the final paragraph of Section 4.

We have added a couple of sentences at the end of the final paragraph for future work.

Specific Comments:

1) P. 1, line 12: Insert “GHz” after “19”.
Done

2) P. 1, line 19: Replace “7” with “seven”.
We have replaced 7 with one week

3) P. 1, line 22: “ERA” and “MERRA” are not defined.
These are very common names, for briefness we do not define them in the abstract.

4) P. 2, line 34: Insert a comma after “events”.
Done

5) P. 5, line 104: Define “EASE”.
Done

6) P. 6, line 126: Insert “GHz” after “19” and insert a space in the second “37 GHz”.
Done

7) P. 7, line 151: Add a comma after “e.g.”
Done

8) P. 8, line 170: Change to “one week”.
Done

9) P. 8, line 195: Insert a comma after “disappearance”.
Done

10) P. 9, lines 197/198: Delete “degree” and define acronyms used here.
Done

11) P. 9, line 203: Why are 30-day moving averages of daily mean air temperatures used here for analysis?

This is to define the start and end of winter period similar as in the satellite approach. We have modified the sentence to clarify this point.

12) P. 10, line 224: Insert a space in “Table 2”.

Done

13) P. 11, line 246: Delete the space in “0°C”.
The space is required by the journal.

14) P. 11, line 248: Should this be “1 cm” instead of “-1 cm”? Replace the contraction “didn’t” with “did not” and delete the space in “0°C”.

The snow temperature is for 1 cm below the surface, so it is -1 cm. We have replaced “didn’t” with “did not”.

15) P. 11, line 250: Delete the space in “0°C”.
See above.

16) P. 12, lines 269/270: More information in the Methods must be provided on the selection of Daring Lake and La Grande IV as areas to test the algorithm to detect snow melt events. Provide for instance the province/territory where these locations are found and a brief description of their environment (vegetation, physiography, etc.) What does “La Grande IV” mean?

The specific locations/provinces of the field sites are provided in Table 2. As indicated in Table 2, the Survey Sites are named after the closest weather station while the actual survey locations are provided in lat/lon. On Line 237 it is noted that the sites are a mix of boreal forest and tundra environments. We chose these locations because of the availability of snowpit survey data with melt/ice crusts recorded in field notes.

17) P. 13, line 285: Revise to “(Figure 5c). A pixel-wise”…
Done

18) P. 13, line 286: Delete the second “winter”.
Done

19) P. 14, line 301: Insert a comma after “e.g.”.
Done

20) P. 14, lines 316 to 318: Are any of these trends statistically-significant? It is difficult to interpret linear trends when associated probability values are not provided. Figure captions for trend analyses do report a statistical significance of 90% and as such the Methods section must discuss use of this level as definition of statistically-significant trends.
We have added a sentence in the Methods Section to indicate the use of 90% level as definition of statistically-significant trends.

21) P. 14, line 319: Delete “are shown in” and insert brackets in “(Figure 9).”
We have modified the sentence to include information about the significance level.

22) P. 15, line 321: Avoid tentative language such as “tends”.
We have modified the sentence.

23) P. 15, line 323: Delete “period”.
We prefer to keep the “period” because we’re referring to the winter period duration defined in this study, which is different than the commonly used winter season (i.e. DJF).

24) P. 15, line 327: Again avoid the use of tentative language.
Done

25) P. 15, line 334: What is the probability value for the correlation coefficient reported here?
$p < 0.001$, we have added this in the text.

26) P. 15, line 336: Replace “are” with “is”.
Done

27) P. 16, line 348: Revise to “lasts”.
Done

28) P. 16, line 363: Change to “northern”.
Done

29) P. 17, line 370: Replace “which” with “that”.
Done

30) P. 17, line 383: Replace “which tend to” by “that produce”.
Done

31) P. 17, line 386: Delete “which revealed”.
We have modified the sentence.

32) P. 18, line 404: Should this be “pan-Arctic”?
We have removed this sentence.

33) P. 18, line 405: Any thoughts on possible future work that could be added here?
We have added a couple of sentences for future work at the end of the paragraph.

34) P. 18, line 409: Replace “which” by “that”.
Done

35) P. 28, Table 1: How does the change in SSM/I orbital overpass from descending (July 1988 to December 1991) to ascending affect the results presented in this study?

Note F-08 descending (July 1988 to December 1991) is for afternoon overpass, which is different than other satellites. We have modified Table 1 to include both the morning and afternoon overpass.

36) P. 31, Figure 2: Are snow pit data available for this site in Finland, as presented in Figure 3 for Manitoba?
No, we choose this site for its multiple melt/refreeze events.

37) P. 32, Figure 3: If possible, this figure should have the same format (two panels) as shown in Figure 2 for consistency between them. Are Tmin and Tmax not available for this site?

Note this figure shows hourly air temperature, so it is impossible to make it the same as in Figure 2, which shows daily air temperature.

38) P. 33, Figure 4: The caption should specify the location where these time series results apply.
Done

39) P. 34, Figure 5: How do these results compare to those presented by Choi et al. (2010)?

Choi et al. [2010] only presented time series of the average snow season duration over the Northern Hemisphere during 1972-2007, not the spatial distribution. Since both the study area and the time period are different between Choi et al. [2010] and this study, it is impossible to compare the results.

40) P. 35, Figure 6: The color scale should be identified as “Days”.
Done

41) P. 36, Figure 7: Why are results for June not presented here? Please define the color scale here as well.

Good point. Results for June are now included, color scale defined.

42) P. 37, Figure 8: What are the units for the color scale? Why are these results presented and how relevant are they to those on the detection of snow melt events from microwave remote sensing?

This figure was removed from the paper as it was not considered essential and the climatology can be readily generated from existing gridded observational or reanalysis datasets.

43) PP. 38/39, Figures 9 and 10: The text must specify what level of significance trends are reported at. Insert “Days” for the color scales here too.
Done

44) P. 40, Figure 11: What are the probability values for the correlation coefficients presented here?
We have included the significant level in the caption and text.

45) P. 41, Figure 12: This figure could be improved by using a program other than Excel for plotting. The y-axis lacks a title and units.
We have modified this figure (now figure 11).

<u>References:</u>

Choi, G., Robinson, D. A., and Kang, S.: Changing Northern Hemisphere snow seasons, J. Climate, 23, 5305-5310, 2010.