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Interactive comment on "A wavelet melt detection algorithm applied to enhanced resolution scatterometer data over Antarctica (2000–2009)" *by* N. Steiner and M. Tedesco

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The Response to Anonymous Referee #2 (Ref2)

Referee Suggestions 1 (RS1)

The discussion of the SAM seems out of place and does not really contribute to the findings. If SAM were linked directly to melt duration or onset, it might be more relevant to the discussion, but that appears quite difficult to accomplish. I recommend that section 4.3 be removed from the manuscript.

Authors Response 1 (AR1)

We removed this section, as suggested.

RS2

The authors state that they use no a-prior information, such as statistically based thresholds (p. 2641, l.20). However, they do use thresholds within the wavelet methodology, such as reducing falsely classified events by creating a minimum threshold for Wsigma0(p. 2546, l. 8) and changing the minimum scale of WTMML (p. 2546, l. 18). I do no object to their choices, nor do I believe these choices diminish their results. However, these choices may affect the results in the same way that a temporal filter applied to a fixed threshold value affects the results. They may wish to acknowledge the affect of these choices and/or conduct a sensitivity analysis.

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AR2

The authors fully agree with Ref2 in that in selection of a minimum scale of WTMML is extremely important to multi-scale analysis as the determined nature of transitions will be relative to the scale over which they are observed. However, this choice will have direct influence on classification of the temporal characteristics of the measurement record (i.e. frequency), rather than magnitude. Do to the complexity in determining the expected temporal sensitivity of the CWT theoretically, we observe it through comparison with the FT3 method and find that this choice will generally limit the minimum detectable melting period to 6 days as indicated in Figure 6.

The choice of a minimum wavelet coefficient magnitude is generally used to separate changing snow conditions where melting does not occur on a seasonal basis and it was found that melting transitions produced Wf at lease an order of magnitude greater than snow-property related changes. We do not believe that this sensitivity analysis is necessary to include in this effort but may be appropriate in further algorithm development.

We do not use location specific a-priori information to judge the expected magnitude (edge strength) of melting events as in previous dynamic melt-detection approaches, rather the multi-scale characteristics of signal transitions are used to separate melting from non-melting events.

Based on the suggestions from Ref.2 we added more details in the discussion of a-priori data (Section 2.1) and in the discussion of choices of minimum wavelet coefficient (Section 2.2).

RS3

The authors chose a period of 6 hours of above freezing temperatures of AWS stations to indicate melt. Based on AWS data from Greenland, it appears shorter periods of melt can be detected by satellite, but it depends on the overpass time of the satellite. Perhaps the authors could comment whether the results would change if they used a different duration of >0C temperatures as a melt criterion for the AWS data.

AR3

The choice of a 6-hour threshold is primarily based on a multiple of the frequency of temperature measurement from the AWS data-record (3-hourly). The main motivation behind a multiple above zero measurement was to reduce the occurrence of a low-quality measurement triggering a false classification. We agree that a 3-hour above-zero measurement could produce melt, and that this is a possible source of false-classification in our validation data-record. This question would be better addressed with more in-situ measurements, currently unavailable for this time-period of this study. Changes to this effect are added to Section (3.2)

RS4A

The authors state that fluctuations in backscatter resulting from melt-refreeze events are a "weakness" of threshold methods (p. 2650, l. 29). If the fixed threshold value is correctly interpreting the surface as refrozen, is this really a weakness?

AR4A

In reference to (p. 2650, l. 29), we suggest that it is difficult to differentiate melt-refreeze cycles from signal noise when using a fixed threshold that does not separate the frozen and melting state in the presence of signal noise, which may be the case even with an ideal threshold value and is a potential weakness of a fixed threshold algorithm. In some cases these fluctuations in backscatter may be from frequent are truly melt-refreeze events. In this current study we find that we often do not have enough ground validation to differentiate one case from another. We also indicate that at least for the Pegasus South stations where the CWT shows good agreement with the AWS.

The manuscript has been changed so that this statement is communicated more clearly (Section 4.2).

RS4B

Further, why should these transient events be filtered (p. 2653, l. 23) if they are melt events? Later in the manuscript (p. 2652, l. 23), the authors note that much of the difference in the methods in the Antarctic Peninsula is due to these melt-refreeze events that occur before and after the persistent portion of the melt season. The authors indicate that CWT algorithm picks up only sustained events by design (p. 2655, l. 25).

AR4B

We have indicated that short-duration melting in places like the Antarctic Peninsula can be differentiated from the persistent backscatter change using the CWT method. This may be a useful tool for various reasons: persistent melt may be a better indicator when comparing ice-sheet runoff as refreeze cycles are largely frozen in place, persistent melting will have a greater impact on ice-sheet densification as often there are intermittent precipitation events during melt/refreeze cycles, we would like to explore the relationship between persistent melting and changing climate conditions for the above reasons. It is the author's choice in this effort to remove these events from the melting record, however in future work it may be beneficial to include all melting or to study patterns in their occurrence since this is an option in the CWT classification.

RS5

I recommend that the authors avoid the use of the word "accuracy" in this context (p. 2654, l. 18).

AR5

Based on the suggestion from Red2, we substitute the word accuracy with agreement here and for other references. See current manuscript.

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RS6

I was surprised that the MT09 passive microwave method detected so much more melt (p. 2656, l. 15; p. 2661, l.8; Figure 8). The authors state that the reason for the difference in the active and passive microwave results is due to the difference in spatial scale of the passive microwave and enhanced resolution scatterometer data (p. 2657, l. 10). However, it is not clear to me why there is such a large difference in the M+30K and MT09 passive microwave results.

AR6

MT09 is based on a fixed LWC value where M+30K can detect different wet snow conditions with different LWC values, depending on the initial value of the dry snow brightness temperature. This is explained in MT09 and it is the reason for the different sensitivity as well.

RS7

The results from Table 1 indicate that the fixed threshold generally does a better job at capturing the melt duration at most stations. This should be acknowledged in the abstract. This does not detract from the overall findings, as the authors acknowledge (p. 2661, l. 3).

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AR7

Acknowledgement of this is added to the abstract. Please refer to the current manuscript

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Interactive Comment