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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

Anonymous Referee #2

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The authors present a melt detection technique using a wavelet analysis of enhanced resolution (2.25km) QuikSCAT scatterometer data over Antarctica, focusing on the Antarctic Peninsula and Dronning Maude Land. The authors present this as a novel method and compare results from this approach to a QuikSCAT-based approach using a fixed threshold (3dB below winter mean). The authors further compared to fixed and dynamic threshold algorithms using SSM/I data, but acknowledged the difficulty in this comparison because of the much coarser resolution of the passive microwave data. Further, they compared their findings to temperature data at AWS stations and to the Southern Annular Mode. They found that the wavelet and fixed threshold approaches with the scatterometer data yield similar results, but there are notable differences be-

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cause the wavelet transform is designed to detect the continuous period of the melt season, while the fixed threshold also detects transient events. They also found that the fixed threshold approach better agrees with the AWS stations at all but one site, again largely because of the detection of transient events. The paper is generally well written and presents an interesting new methodology. I am not an expert on the methodology they employed, but it appears to be well explained and appropriate in this context. The topic is relevant to the readership of The Cryosphere.

Below are several points that the authors may wish to consider when revising the manuscript. 1. 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. 2. 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 $W_{\sigma 0}$ (p. 2546, l. 8) and changing the minimum scale of WTMMML (p. 2546, l. 18). I do not 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. 3. 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 $>0^{\circ}\text{C}$ temperatures as a melt criterion for the AWS data. 4. 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? 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),

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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). 5. I recommend that the authors avoid the use of the word “accuracy” in this context (p. 2654, l. 18). 6. 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. 7. 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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