

# ***Interactive comment on “Surface melting over the Greenland ice sheet from enhanced resolution passive microwave brightness temperatures (1979–2019)” by Paolo Colosio et al.***

## **Anonymous Referee #2**

Received and published: 30 November 2020

### General Comments

The authors present analysis using a new, higher resolution passive microwave dataset for determining surface melt across the Greenland Ice Sheet. The authors make a strong case for why such a dataset is important for monitoring the ice sheet and demonstrate that the higher resolution data allows us to study surface melt in greater detail, altering the magnitude of some of the temporal trends and providing sufficient resolution for more thorough spatial analyses. The work is novel, presenting a new dataset and analyzing it with an existing algorithm to study trends in surface melt extent and timing.

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The methods implemented are appropriate and sufficiently explained in most cases. In my specific comments, I have a few points that I would like to see addressed in terms of articulating implications of some of the issues the authors note with the data (i.e. differences in the four PMW sensors used, issue of poor matching between MEMLS and MAR5cm before 1992, MEMLS algorithm issues after main melt season). I do not consider any of these to be major issues; I would just like to see some clarification and explanation of the potential effects of this issues on the results. Additionally, an overall comparison of how this PMW melt detection compares to other PMW melt products in terms of commission and omission errors should be included in order to put this work into context.

The results are significant, demonstrating that trends in surface melting are sensitive to the scale at which they are studied. The trends identified are important in our assessment of surface processes that affect mass balance and sea level rise. The surface melt product is an important dataset that can be used in future work as described in the conclusions.

The manuscript is overall well-written and flows logically, with only minor issues that will be easy to fix.

Please find my specific comments and technical corrections below.

### Specific Comments

line 171: Please explain what sigma is. It “varies in space and time” based on what – is it the standard deviation?

line 190: I appreciate that many melt threshold/algorithms are implemented (and that they are compared to both in-situ data and the MAR output). Please explain why you selected the threshold/algorithms as you did given that you also presented at least 2 others.

line 247: With respect to the differences in acquisition time, is there a consistent

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lead/lag between timing of SMMR and SSM/I-F08? If so, how might the directionality of the lead/lag impact the analysis?

line 260: Which correction did you apply to the SMMR data (the first method with weighted values or the second method using all values and the least square fitting) and why?

line 260: What are the implications (if any) of not correcting the datasets? For instance, the average differences from F08-F11 and F13-F17 are positive, while the difference for F11-F13 is negative in the evening and positive (close to zero) in the morning. If agreement is worse when corrected, I agree that it makes sense not to implement the linear corrections, but it would be important to address what the potential effect of this is.

line 306: Is there an emissivity threshold being considered here to indicate if melting is or is not occurring? If so, please add that.

line 307: I think it was meant to say lower than in Summit Camp case?

line 315: (AWS Comparison Section) Were there any temporal trends the commission/omission errors of the melt algorithms as compared to the AWS data?

lines 325-327: I think the numbers for LWC1m and LWC5cm were swapped here?

lines 346-346: You bring up a very interesting point here. Because the brightness temperature after the largest part of the melt season has ended up lower than the Jan/Feb average, then the MEMLS algorithm would be less able to detect subsequent melt events. Is this a consistent pattern that is observed across sites/years? This could lead to a change in the frequency of omission errors of the MEMLS algorithm pre and post main melt season. Please discuss potential implications of this issue.

line 355: How do the commission and omission errors for these algorithms compare to other PMW melt detection products.

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lines 369-371: Please explain your decision to compare 245K and MAR1m and also compare MEMLS and MAR5cm. I believe it is because the expected differences in sensitivity of each of the different methods of detecting melt, but just want to be sure that is why this decision was made.

lines 375-380: It seems that using the SMMR data (from 1979-1987) is part of the issue here. Is that correct? Is it partly because of the difference in time of day? Or difference in sensor technology used?

line 382: What are the implications of the melt extent being underestimated?

lines 396-397: Is there precedent for using this definition of MOD and MED?

line 406: Here and elsewhere you refer to the trends in the coarser-resolution data. Please consider including this analysis in supplemental material.

line 436: Is there any explanation for the areas in the map with anomalous trends? (figure f, negative trend in Northern Greenland, figure d, positive trend in some regions in central Greenland)

lines 436-438: How are pixels that do not consistently (every year) experience melt handled?

lines 440-446: This content reads more like methods. Consider relocating the description of the methods of the semi-variogram analysis.

line 452-453: The comment about extending this analysis seems out of place.

line 457: Consider showing the figures that accompany the data for Table 7. Perhaps in the supplement at least?

line 458: These are semi-variograms for melt duration in each month. Is this the number of days of the month that melt occurs for a given pixel? Do the days need to be consecutive? I think more detail about the melt duration variable should be provided here.

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lines 464-468: Is there a way to compute uncertainty associated with these distances?

lines 475-476: What does the larger nugget value for MAR as compared to the PMW data tell us?

lines 499-500: The sentence about the threshold for melting seems out of place in the conclusions.

line 500-501: The data do not seem to support “good matching” in most of the years from 1979-2019. The data do seem to support good matching from 1992-2019. Please add this caveat to the statement.

#### Technical Corrections

line 11: modulation “of” ice dynamics

line 13: “in view of” should perhaps be replaced with “due to”?

line 17: km instead of Km

line 19: capable “of detecting”

line 25: the word “interest” seems out of place. Delete or replace, perhaps with “usefulness”?

line 26: monitor should be “monitoring”

line 74: here and elsewhere you use Tbs, when I think leaving it singular as Tb is more clear

line 188: Should this be Tc or Tb? If it is meant to say T $\hat{a}$ nc, please define this term.

line 190: “as sensitivity to Zwally. . .” not sure what is meant here. Typo?

line 251: R2 needs to be a superscript 2

line 255: specify that you referring to data in Table 4 here

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line 267: move “daily averaged from AWS” to directly after air/surface temperature to improve sentence clarity

line 281: correct 919% to 9.19%

line 305: fix subscript on  $T_b$

line 336: Is this average surface air temperature? Please specify.

line 342: detected by the threshold algorithms in AWS temperature? By all three?

line 344: should be “corresponds”

line 355: consider describing it not as an overall error but as what it is a mean of errors calculated using different techniques.

line 364: perhaps rephrase as “Here, we remind the reader that..”

lines 399-400: typo? Partial repeating of a line

lines 405-406: typo in years indicated here?

line 418: Fix figure numbers

line 468: some missing words, should read “the value of the range results is lower in the case of..”

line 473: “till” should be “until”

line 503: typo of word largely

line 525: Perhaps add “We have” to “assessed the capability. ...”

line 729: (Figure 1) consider including scale bar for figures c and d

line 739: (Table 1) table caption perhaps should say “of the selected Greenland Climate Network (GC-Net) sites”

line 752: (Table 4) This table shows regression analysis for more comparisons than

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just SMMR and SSM/I-F08. Please update caption to reflect this.

line 760: (Figure 4) Please ensure that y-axis are the same for all three panels of figure 4a

line 771: (Figure 6) Please consider adding labels to each map for ease of interpreting the figure

line 780: (Figure 7) Please indicate what the vertical teal lines represent

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-250>, 2020.

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