RESPONSE TO REVIEWER 1

Dear reviewer,

Please find below our reply. We thank you for your comment and precious suggestions.

P. Colosio, M.Tedesco, X. Fettweis and R. Ranzi

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 #1

Received and published: 25 November 2020

General comments:

This study analyzes the newly developed NASA MEaSUREs calibrated enhanced resolution (∼ 3.125 km) passive microwave dataset (37 GHz horizontally polarized channel) (Brodzik et al., 2016, cited in this paper) to examine whether the dataset can be used for studies on the Greenland ice sheet (GrIS) surface melt. The dataset was developed by using the data from the following satellite microwave radiometers: The Nimbus-7 Scanning Multichannel Microwave Radiometer (SMMR) and Defense Meteorological Satellite Program (DMSP) Special Sensor Microwave/Imager (SSM/I). Because the frequency of the GrIS surface melt has been increasing recently due to the ongoing rapid warming, the GrIS surface melt commands considerable attention. Therefore, the topic explored by the authors fits very well with the scope of this journal.

In this paper, the authors compare five post-processing methods applied to the new dataset: the M+ΔT methods with changing ΔT values of 30, 35, and 40 K, the 245 K fixed threshold method, and the MEMLS (Microwave Emission Model of Layered Snowpack) method. All these five methods can be categorized into the thresholdbased method. The first four methods are very simple, whereas the MEMLS method is relatively physically based but its threshold value does not change dynamically. These methods give threshold values of passive microwave brightness temperatures to detect the surface melt. In case a (measured and) post-processed value from a satellite becomes higher than a threshold value, the occurrence of the surface melt can be estimated. Based on the comparisons of the melt detection results with in-situ meteorological/snow data from automated weather stations on the GrIS and the regional climate model MAR, the authors conclude that the MEMLS method shows the best performance in terms of capturing the GrIS surface melt. Finally, the authors present inter-annual variations of the GrIS surface melt area extent obtained from this study.

My honest impression is that this paper contains so many information that many readers will find it difficult to follow the discussion. Tedesco et al. (2013, cited in this paper) already have demonstrated the effectiveness of the MEMLS method over the GrIS, so that, I think results from the M+ΔT methods and the constant 245 K method can be removed. It is because they are very simple compared to the MEMLS method. I do not find interests showing these results in this paper.

R: We thank the reviewer for this useful comment and we acknowledge that Tedesco et al. 2013 performed an analysis of the algorithms. Yet, we would like to keep the description of those results for two reasons: 1) we think that adding another paper to the comparison of the different algorithms increases the confidence in our results. It is best practice in science to test the robustness of the results from previous studies and this is one of the reasons to perform and show the comparison; 2) the results are here discussed with respect to the enhanced product. Given that the previous work was performed on the 25 km and the methods used to create the gridded values are different (not only in terms of spatial resolution but in terms of how Tbs are computed and extrapolated form the observations), we think that it is important to show that the results from the previous study still hold. Again, we thank the reviewer for this useful suggestion.

The authors also compare their results with the outputs from the MAR model. I completely agree with the point that the MAR model is very sophisticated; however, the model output is not the reality. Therefore, I cannot understand why the authors want to compare them in this study, although I have confirmed from Figure 7 again that the MAR model performs very well over the GrIS.

R: In this paper we compare our results with the outputs from the MAR model. In this regard, we do not use the modelled data as a validation but as an assessment, introducing a further comparison to evaluate the different algorithms. In particular, using a third melt classification dataset let us better understand where the PMW and AWS melt detection techniques are in agreement. Moreover, MAR realistic assimilation and modelling of observed data is available on a gridded geographic support which enables an effective comparison with microwave data.

In the global scientific community studying the GrIS surface melt, the dataset by Mote (2007), which utilizes data from the 18 and 19.35 GHz horizontally polarized channels in the same sensors/satellites as those used in this study, has long been utilized widely. As far as I know, the dataset employs a dynamically changing threshold method to detect the GrIS surface melt. Because the horizontal resolution of the dataset by Mote (2007) is 25 km, it seems to me that the new dataset has a big advantage. Therefore, the authors should compare their MEMLS-method-based results with the dataset by Mote (2007). Without this, readers cannot know advantages/significances of the new dataset presented in this study.

R: We thank the reviewer for suggesting this further comparison. We agree with the reviewer that a comparison with a coarser resolution melt product will give strength to the results. In the revised manuscript we report the comparison with the 25 km dataset. We add the major results of the comparison (in terms of commission/omission error, melt extent estimation and melting trends) in the main paper and additional figures and tables in the supplementary material. For 25 km resolution data: https://doi.org/10.5067/MEASURES/CRYOSPHERE/nsidc-0533.001.

Also, I would like to suggest that the data and methods section (Sect. 2) is a mix-up of data, methods, results, and discussion, which confuses readers. Figures 2, 3, and 4, as well as Tables 3 and 4 should be presented in the results and discussion section. Please reformulate the section.

R: We agree with the reviewer. We divided Section 3 (Intersensor calibration) in two parts: 1) "methods" in Section 2.5 and 2) "results" in Section 3.1. Now, Figures 3 and 4 as well as Tables 3 and 4 are in Results and Discussion section. We left Figure 2 in PMW data description, as it is used as support to describe the differences in timeseries between the new and the old datasets, providing a first picture of the differences between 25 km and 3.125 km at point scale. Moreover, in accordance with this suggestion, we moved the description of the methodology adopted in the semi-variogram analysis in a dedicated subsection in Data and methods section.

I would like to suggest that the authors should attend to the above-mentioned major issues before considering its publication.

Other specific comments are as follows:

Specific comments:

L. 25: More detailed explanation of "local scale processes" is needed here.

R: In the dedicated subsection in the "Results" section we explain the possible local scale processes affecting the spatial autocorrelation of melting. See "…local processes that drive melting as the melting season progresses (e.g., impact of bare ice exposure, cryoconite holes, new snowfall, etc.) and of a more developed network of surface meltwater, the presence of supraglacial lakes and, in general, the fact that the processes *driving surface meltwater distribution (e.g., albedo, temperature) promote a stronger spatial dependency of meltwater production at smaller spatial scales."*

L. 86 ∼ 90: It is necessary to introduce why such a high-resolution dataset from the Ka band product were not available until recently. What is the key innovation that enabled us to use the Ka band data for the detection of the ice sheet surface melt? It is also important to explain the difference in sensitivities of the K and Ka bands data to the liquid water clouds.

R: For what concerns the novelty of the high-resolution product and its recent availability, we described the improvements introduced in the gridding technique in subsection 2.1. We provide a description of the main steps and techniques adopted in the image reconstruction to reach the resolution of 3.125 km. We refer to this part as "More details are reported in the following sections". We better refer now modifying the statement in "More details are reported in Section2.1". We also added details about the coarse resolution "drop-in-the-bucket" technique, in order to make clearer the difference between the two products. For what concerns the difference between K and Ka bands, we thank the reviewer for pointing this out. The presence of the atmosphere is an important point to be taken into account when working with PMW spaceborne radiometers. The surface emission signal passes through the atmosphere and is affected by its absorption and emission. The atmosphere affects the two frequencies of 19 and 37 GHz in a slightly different way. However, even if a difference exists, it is not that large and reduces as the brightness temperature increases (Tedesco and Wang, 2006). We expand this issue in the revised manuscript adding the following reference.

Reference: Tedesco, M. and Wang, J. R.: Atmospheric correction of AMSR-E brightness temperatures for dry snow cover mapping," in IEEE Geoscience and Remote Sensing Letters, vol. 3, no. 3, pp. 320-324, July 2006, doi: 10.1109/LGRS.2006.871744.

L. 130: "2.2 Greenland air/surface temperature data": It is necessary to explain how the authors obtain surface temperature from the AWS (automated weather station) data. It is because the AWSs do not measure surface temperature directly.

R: Thank you for pointing out this. We corrected in "2.2 Greenland air temperature data". In the data description we actually refer to the data as air temperature (3m above the surface).

L. 132 ∼ 133: Strictly speaking, even if the surface temperature reaches 0 degreeC, it does not ensure that meltwater exits at the surface. How do the authors detect whether meltwater exits at the surface or not from the AWS data?

R: We used the air temperature from the automated weather stations available as a proxy of the presence of surface melting as done in Tedesco (2009). We certainly are aware of the limitations of this approach (that surface melting is not regulated by the temperature only and that the air temperature does not necessarily represent the snow surface temperature), however we classify a day as melting when the air temperature reaches the value of 0°C. Moreover, we performed a sensitivity analysis considering as threshold values for air temperature the values of -1°C and -2°C in order to include possible melt events occurring at sub-zero air temperature conditions. Additionally, we performed the same commission/omission error analysis using the outputs of the regional climate model MAR. The use of MAR simulated LWC gives more robustness to the results obtained with the AWS analysis.

L. 185 ∼ 186: "Building on Tedesco (2009), we considered the two LWC values of 0.1% and 0.2 %": Please explain more in detail about this process. It is unclear why 0.1 and 0.2% are chosen here.

L. 193: For MEMLS, why do the authors consider only the case of 0.2% LWC?

R: To respond to the last two comments, the choice of 0.2% of LWC is related to the rationale behind MEMLS algorithm, designed to detect small presence of liquid water (such as 0.2%). This algorithm is supposed to *detect the sporadic melt events. We based our choice selecting the 0.2% according to the results presented by Tedesco (2009), cited in this paper, who tested both 0.2% and 0.1% liquid water content. Accordingly, the value of LWC=0.2% for the MEMLS algorithm better matches the number of melting days detected from other sensors (e.g. QuickSCAT). Contrarily, melting was overestimated by applying the algorithm based on 0.1%. To make the manuscript clearer to the reader, we remove the statements related to the 0.1% LWC as we do not consider it in the following sections.*

L. 193 ~ 196: "As we explain below, this choice was driven by the performance of the different considered algorithms. Moreover, we found that the fixed-threshold algorithm is more sensitive to persistent melting where the MEMLS-based one can detect sporadic melting. This allows us to analyze both melting conditions (sporadic vs. persistent) and analyze them within the long-term, large spatial scales that the PMW data can provide.": I think it is not necessary to state them here. They can be removed.

R: Removed

L. 304: "with the MEMLS being the most sensitive": The authors' intention is unclear. Sensitive to what?

R: Corrected as "with MEMLES providing the lowest threshold"

<u>Technical corrections:</u>

L. 16: "MeASUREs": Its definition should be indicated here.

R: Corrected indicating the definition.

L. 17: "Km" -> "km"

R: Corrected

L. 19: "MEMLS model": Brief explanation of the model or the definition of the abbreviation should be indicated here.

R: Corrected by indicating the abbreviation of the definition

L. 82: Please provide the definition of the abbreviation "rSIR".

R: Corrected

L. 103: "SMMR": Please provide its definition here.

R: Corrected

L. 131: "In order, to" -> "In order to"

R: Corrected

L. 146 ~ 147: "Lateral and lower boundary conditions are prescribed from reanalysis datasets." -> "Lateral and lower boundary conditions of the atmosphere are prescribed from reanalysis datasets."

R: Corrected

L. 153: "meltwater extent" -> "melt extent"

R: Corrected

L. 195: "where" -> "whereas"

R: Corrected

L. 211 ~ 214: Please follow the instruction how to indicate date and time in the text. https://www.the-cryosphere.net/submission.html#math

R: Corrected

L. 576 ~ 579: Brodzik et al. is updated in 2020.

R: Corrected

References:

Mote, T. L.: Greenland surface melt trends 1973–2007: evidence of a large increase in 2007, Geophys. Res. Lett., 34, L22507, https://doi.org/10.1029/2007GL031976, 2007.

R: Inserted in References

RESPONSE TO REVIEWER 2

Dear reviewer,

Thank you for your useful comments and suggestions. See below our reply.

P. Colosio, M. Tedesco, X. Fettweis and R. Ranzi

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.

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?

R: Sigma is the standard deviation of the timeseries of brightness temperature for a specific year and pixel. We added this information in brackets in the revised manuscript.

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.

R: Explained at the end of section 2.4 as "We selected M+ΔT and MEMLS due to their higher accuracy in detecting both sporadic and persistent melting with respect to the other approaches presented above (i.e.

Torinesi et al. (2003), Ashcraft and Long (2006) and MEMLS in case of LWC=0.1%) proved in previous studies (Tedesco, 2009). We selected also the 245K to test a more conservative approach aimed to detect persistent melting only."

line 247: With respect to the differences in acquisition time, is there a consistent lead/lag between timing of SMMR and SSM/I-F08? If so, how might the directionality of the lead/lag impact the analysis?

R: The lead/lag can be obtained by Table 2 where sensors characteristics are detailed. Specifically, in case of SMMR and SSM/I-F08, the lag of SMMR sensor is of about 6 hours (24:00 vs 18:17 for the ascending pass and 12:00 vs 6:10 for the descending pass). This constant lag can lead to errors and biases in particular at the beginning of the melting season when snow undergoes freeze/thaw cycles during the day (e.g. frozen snow, i.e. low Tb, at 6:10 for SSM/I-F08 and liquid water, i.e. high Tb, at 12:00 for SMMR in case of descending pass, the opposite in case of ascending pass). A possible consequence could be an early estimation of MOD from SMMR data (as already pointed out in Tedesco et al., 2009). In the revised manuscript we added "Specifically, we expect larger errors at the beginning of the melting season when snow undergoes thawing/refreezing cycles during the day, potentially having frozen snow (low values of T_b) early in the morning and late at night (SMMR ascending and SSMI/-F08 descending passes) and presence of liquid water (high values of T_b) during the day."

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?

R: Thank you for noticing this. We specified it as "We applied the correction coefficients obtained with the second method according to the higher relative improvement for the evening pass."

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.

R: Possible implications in not correcting the dataset are related to the relative difference of the measurements from different satellites. This can cause errors in melt detection when considering the fixed threshold case (as 245K) but not in case of MEMLS which is computed considering intrinsic characteristics of the timeseries every year (i.e. winter average brightness temperature). However, the computed average difference of Tb in case of F08-F11, F11-F13 and F13-F17 is at most 0.52K, negligible with respect to the increase of Tb due to LWC.

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

R: Even if a rough threshold could be assigned (e.g. around 0.85), in this case we do not give a threshold. Instead the comparison is between the three computed values of emissivity only. Considering a surely dry condition emissivity (0.74 in Figure 5a) and a surely wet snow condition emissivity (0.9 in Figure 5c), if melting occurred in the period 17 June and 17 July we would expect at least a value between case (a) and (c) (between 0.74 and 0.9). This happens in late July, when the brightness temperature "jump" is strong and evident, the air temperature reaches the melting threshold and, consequently, the emissivity reaches a value even higher than 0.9.

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

R: Exactly, corrected.

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

R: We thank the reviewer for this interesting question. We performed the comparison with AWS and MAR data to assess the different algorithms and select the best one, following Tedesco (2009) cited in this paper. We did not look at the temporal variability and trends of the commission/omission errors. We assumed that, if a trend does exist, it would have affected every algorithm. Thus, for our purpose, we only considered the overall error for every available year.

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

R: Corrected.

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.

R: We found this pattern at Swiss Camp site at multiple years as it is possible to see in Figure 5c where the timeseries of Tb in 2006 is reported. The 2006 example seems to confirm the hypothesis: after the main melting season (mid-august, between day 200 and 300) the Tb drops to values lower than before the melting season. After day 300 another jump of the signal is detected (for 1 day only, by MEMLS and the M+DT) followed by a further decrease of Tb. In this case the melt event is detected. Consequently, the lowered capability of MEMLS to detect melting is not a constant issue and it does not necessarily affect the omission error significantly. Similarly, before the melting season a sporadic melt event is detected (before day 100 for 1 day only, by MEMLS and M+DT), followed by a drop of Tb to values lower than before the melt event. It would be possible to expand this interesting point in another research work, addressing the causes and implications of these early/late sporadic melt events.

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

R: We compared the commission and omission errors presented in the submitted version of the manuscript with the ones obtained for the Thomas Mote 25km 19 GHz PMW dataset suggested by the other reviewer. We include the averaged results in Table 5 in the revised manuscript. This PMW dataset is at the resolution of 25 km and uses the 19 GHz frequency, enabling the comparison with a coarser resolution data and giving us the possibility to show the benefits of the highest resolution. We found that, on average, omission and commission errors are lower in case of the higher resolution dataset. Moreover, the comparison with MAR 6km outputs shows lower NSE values in case of the 25 km 19 GHz PMW dataset.

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.

R: We added an explanation of this choice as "due to the expected differences in sensitivity to detect persistent and sporadic melting between 245K and MEMLS, respectively"

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?

R: It seems that the main issue is related to the different sensor technology. Even if we improved the consistency of the timeseries by calibrating the SMMR data, differences still remain, partly because of the *different acquisition time and frequency and partly because of the specific characteristics of the sensor (e.g. different IFOV, swath width, incidence angle). Added a sentence:*

"(…) possibly due to a persistent bias after the intercalibration of the dataset. (…)"

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

R: Added "A possible consequence of the melt extent being underestimated in the first part of the timeseries is a slightly overestimated long-term trend. To address this possible implication, in the next section we compute long-term trends considering both 1979 – 2019 and 1987 – 2019 reference periods."

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

R: Following Tedesco et al. (2009), cited in this paper, we defined MOD and MED as the first and the last two days in a row when melting occurs. Tedesco et al. (2009) identified the first and last days as MOD and Med using a double condition algorithm. Here chose to consider two consecutive days as we prescribe a single melting condition (Tb>threshold).

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

R: In the revised manuscript we will substitute this analysis with the comparison of the trends computed using the Mote PMW dataset, reporting the analysis in the 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)

R: A possible explanation can be related to the definition of MOD and MED (first two consecutive days when melt occurs and stops). Possibly, by modifying the constrain of two consecutive days (e.g. a single day or even 3 or 4 consecutive days) the anomalous areas would reduce. On the other hand the parameter melt duration MD is more spatially continuous in trend evaluation.

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

R: In case of pixels that do not consistently experience melt, when computing the pixel-scale trends for MOD and MED, we performed the calculations for the available data only. In case of melt duration (number of days detected as melting for each pixel), instead, we consider as MD=0 in case of a pixel presenting zero melting days.

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

R: We moved the description of the semi-variogram analysis in a new sub section "2.6 Spatial autocorrelation: the variogram analysis" where we describe the methodology adopted.

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

R: Removed.

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

R: In the revised manuscript we report the figures asked in the supplement.

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.

R: Described adding the following sentence:

“Here, we compute the melt duration for each month of the melting season at pixel-scale as the number of days of the month (May, June, July or August) detected as melting for the specific pixel.”

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

R: It could be possible to evaluate the variability of these distances by performing a larger analysis for every year of the timeseries (1979-2019). We are considering to expand this aspect in a future research focused on this aspect.

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

R: We think that the difference in nugget value is mainly related to the different spatial resolution of the considered datasets. The nugget effect is affected by the volume of sampling, decreasing in value as the volume increase. The nugget effect can be attributed to measurement errors or spatial sources of variation at distances smaller than the sampling interval or both. Measurement error occurs because of the error inherent in measuring devices. Natural phenomena can vary spatially over a range of scales. It is difficult to say what drives this difference without in-situ data (both melting and passive microwave). We note that this does not impact the results on the scale break properties.

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

R: Removed

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.

R: Corrected as: “We obtained good matching (i.e., NSE>0.4 or, at least, positive) in most of the years from 1992-2019 when comparing MEMLS derived melt extent with MAR liquid water content in the first 5 cm of snowpack. On the other hand, we found bad matching in the period 1979-1992, possibly due to differences in sensor characteristics.”

<u>Technical Corrections</u>

line 11: modulation “of” ice dynamics

R: Done

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

R: Done

line 17: km instead of Km

R: Done

line 19: capable “of detecting”

R: Done

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

R: Deleted

line 26: monitor should be “monitoring”

R: Done

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

R: Thank you, corrected

line 188: Should this be Tc or Tb? If it is meant to say TÂnc, please define this term. ˇ

R: We use Tc to refer to the threshold brightness temperature value. We added the definition where first introduced as "(…)Tc indicates the threshold value (we keep the same notation in the following)"

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

*R: Zwally and Fiegles (1994) proposed the DT=30K. Here we test DT=35 K and DT=40 K to test the sensitivity of the algorithm selected. I correct the statement expanding it as "…*equal to 30K and, to test the sensitivity to Zwally and Fiegles (1994), 35K and 40K (M+30, M+35 and M+40 from here on)…"

line 251: R2 needs to be a superscript 2

R: Done

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

R: We are actually referring to Table 3 here, specified.

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

R: Done

line 281: correct 919% to 9.19%

R: Done

line 305: fix subscript on Tb

R: Done

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

R: Yes it is, specified

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

R: Thank you for noticing this, only for Tair=-1°C and -2°C. We added "(Tair>-1°C)"

line 344: should be "corresponds"

R: Corrected

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

R: Substituted "overall" with "average"

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

R: Done

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

R: Yes, typo. Corrected

lines 405-406: typo in years indicated here?

R: Yes, substituted 2016 with 2019

line 418: Fix figure numbers

R: Corrected to Figure10d

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

R: Corrected as “(…) the value of the range is lower (…)”

line 473: “till” should be “until”

R: Corrected

line 503: typo of word largely

R: Corrected

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

R: I think it is correct as it is.

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

R: It is the same scale bar of Figures a and b.

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

R: Corrected

line 752: (Table 4) This table shows regression analysis for more comparisons than just SMMR and SSM/I-F08. Please update caption to reflect this.

R: Corrected substituting with “between the selected couples of satellites”

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

R: Corrected. I also increased the thickness of the lines and the colors to make the figure more readable.

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

R: We apologize but we do not understand this specific question. However, we reported in the captions all the information needed to interpret the figure.

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

R: Done