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

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Dear reviewer,

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

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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 (âĹij 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 threshold based 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 a 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 discussed here 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 lets 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 time-series 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 âĹij 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 âĹij 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 during the day. 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 âĹij 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 âĹij 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”

Technical corrections:

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 âĹij 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 âĹij 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 âĹij 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

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