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Atmospheric Measurement Techniques Discussions

Interactive comment on "Inter-calibrating SMMR brightness temperatures over continental surfaces" *by* Samuel Favrichon et al.

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The authors are grateful for the review by Pr. Linwood Jones of the paper "Inter-calibrating SMMR brightness temperatures over continental surfaces". The comments were specific and definitely helped the authors improve the paper for future readers.

1 Response to the review

This paper presents the results of an inter-satellite radiometric calibration for SSMR over land using the GPM instrument. The authors' approach is novel in that they perform the inter-calibration of two satellite radiometers without near-simultaneous collo-

cated observations. This method is justified because the SMMR was the first conical scanning radiometer in space, and therefore did not have the usual over-lapping period for comparisons with other space-borne instruments. Given this situation, I feel that the authors have made a reasonable case for their statistical method developed to compare the current GMI with the previous SSMR. However, I have some comments, which I feel would strengthen their case if included in their paper.

1) Concerns the three available SMMR Tb datasets that exist. In section 2.1 the CM SAF FCDR was described but not the others? Were they available? It would have been better to compare the three different data sets in their statistical analysis or at the very least to discuss why they were not considered.

Response : We only described the dataset released by the CM SAF as it is the one we used. The reason for that choice is twofold : First the CM SAF released the original data as well as their inter-calibration layer, making it easy to use an uncalibrated dataset. The unprocessed dataset would have been the same from any of the other providers. We still compared the inter-calibration layer from CM SAF as it was available with adequate documentation. Furthermore, the contact with CM SAF researcher was easy and they helped us through various steps in using their dataset.

2) Concerns the oceans and/or Antarctic sea ice data sets. I suspect that a similar statistical comparison could have been made (as presented for land). I recognize that this expands the scope of the analysis, but it also makes the paper stronger. I suspect that similar results would have been found, which would provide confidence to the conclusions.

R The study of sea-ice datasets is indeed a very important topic, and it could be the subject of a specific study. However, the need for inter-calibration for the retrieval of sea ice parameters is more limited, as most sea ice concentration algorithms include inter-calibration processes, with the use of what is called dynamic tie-points. Within the algorithms, changes in the instrument responses is already taken into account

(e.g., Tonboe et al., 2016). The ocean comparison was rapidly tested and the results showed that the methodologies from the different centers yield similar results.

3) Concerns the selection of the two 2-month periods, namely: Jan/Feb and Jul/Aug. Some discussion was provided in section 2.2, but I recommend more information be provided to inform the reader specifically why these were selected (as opposed to monthly comparisons)?

R The two periods selected (6 months apart) offer a large sample of possible Earth states, that are needed for a robust statistical comparison. A month by month comparison could have also been done, but it would most likely only add complexity to the paper without improving the proposed inter-calibration. Section 2.2 was updated to improve readers understanding of the underlying choice in the methodology.

4) The SMMR biases, relative to GMI (SSMI), are presented in Fig-1, -2 -4, but I recommend that they also are included in a Table of results. **R** The Figures 1,2 and 4 only show SMMR biases on specific locations. A global evaluation of the bias is indirectly included in Table 3, with the temperature dependant correction.

2 Specific corrections

The following corrections are suggested:

P-2 Line-3 following Seasat insert “Nimbus-G” **R** Added to the corrected paper

P-2 same pp WindSat and TMI were not mentioned in the list of radiometers? Since only SSMI and GMI were involved in the direct comparisons, the others could have been omitted? **R** Indeed the other radiometers are not used and have been removed from the paper.

P-2 Line-12 . . . Fennig et al. 2019) than that . . . **R** Corrected in the paper

P-2 Line-25 Insert “However”, this method is very **R** Sentence changed.

P-4 Line-5 . . . CM SAF FCDR insert “SMMR” . . . **R** Corrected in the paper.

The authors appreciate the thorough review by Pr. Linwood Jones that offered precise comments to improve possible unclear areas in the paper. We hope that this answer will remove any doubts concerning the methodology and choices made in the study.

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