

Manuscript: AMT-2019-493
Title: Inter-calibrating SMMR Brightness Temperature over Continental Surfaces
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Recommended Disposition: Accept with minor revisions

General Comments:

I initially had some serious reservations regarding the authors attempt to intercalibrate SMMR Tbs using data from GMI almost 30 years later. After reading the manuscript, however, I think that the authors are providing some valuable insight into the SMMR data, although I would like to see them strengthen their conclusions a bit regarding the limitations of the SMMR data. I tend to agree that the SMMR data has value for certain applications, but any attempt to use this data to look at climate trends or more subtle variations is definitely not warranted. I believe the authors conclusions bear this out, but I would like to see a bit more explicit statement to this effect.

Specific Comments:

Page 3, line 17: The statement “Here, we suggest to use GMI as a reference instrument, assuming that the environmental conditions have not changed drastically from the SMMR to the GMI era, to allow the comparison of a large set of observations averaged over time. This strategy does not allow to perform a detailed intercalibration but it makes it possible to correct for major biases that so far hamper the use of SMMR over land for the generation of climate record of geophysical parameters.” This statement regarding the nature of the intercalibration needs to be reiterated in the conclusions. I’m also a bit concerned with the statement suggesting that the intercalibrated data can subsequently be used for climate data records. While it depends greatly on the specific application, certainly using SMMR data to extend the global satellite time series for identifying subtle climate trends is not warranted.

Page 5, Table 2: GMI has operated from March 2019 forward. The data have been reprocessed back to this date to provide a consistent data record.

Page 5, line 13. Again, this hypothesis of limited changes in environmental conditions comes with significant caveats given climate change.

Page 6, line 4: This statement is a bit unclear. Do the authors mean GMI data are collected for 2 months in summer (i.e. July and August) and two months in winter (i.e. January and February)?

Page 6, line 12: Did the authors attempt to look at potential calibration differences between the three years of SMMR data used (i.e. 1981, 1982 and 1987)? Is there any indication of a time-dependent change or calibration drift (or lack thereof)?

Page 7, figure 1: I’m a bit unclear on the location and extent of the regions chosen. The text mentions 25 grid cells. How large is the region covered by 25 grid cells. A figure showing the various regions selected for the results in Figure 1 would be useful.

Page 8, line 1: The relative consistency in the slopes shown in Figure 3 does lead point to the possibility of an error in either the warm load temperature or the antenna pattern correction (i.e. spillover). This might be worth mentioning in the conclusion, although the results by Dai and Che don't appear as consistent between channels.

Page 8, Figure 2: The large difference between the histograms shown over Antarctica (i.e. 5-10K for all except the 37 GHz V-Pol channel) raises some questions. Is this result consistent with the subsequent results shown in Figure 3? The authors mention screening out ocean and sea ice, but isn't the land region covered by snow/ice? What impact might changes in snow/ice emissivity have on the observed Tb? Emissivity of snow/ice is difficult to determine and could potentially be influenced by a number of different factors and thus have little or nothing to do with calibration differences. As a result, unless these differences are consistent with the linear fit derived from Figure 3, it isn't clear to me that these results over Antarctica are useful. Please justify.

Page 9, Table 3: The values of slope and intercept from the linear fit don't provide the reader a clear sense of the magnitude of the calibration differences. I suggest that the authors consider adding the mean intercalibration difference and uncertainty for GMI Tb values of 200 and 300K. This would provide the reader a much better sense of the magnitude of the adjustment as well as the uncertainty for both cold (ocean) and warm (land) scenes.

Page 12, line 13: I would like to see the authors strengthen and better qualify the statement "With these hypothesis, the objective is to correct for the large differences between the sensors". As mentioned regarding Table 3, exactly how large are these differences for typical cold ocean and warm land scenes (i.e. 200 and 300 K)? Ultimately, I believe that highlighting the limitations of the SMMR data is as important if not more so than that value of the corrected data itself. The results in Figure 4 support this in that F08 SSM/I appears to be significantly better calibrated than SMMR. This should be stated in the conclusions.