Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-319-RC1, 2018

Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluation of microwave radiances of GPM/GMI for the all-sky assimilation in RTTOV framework" *by* Rohit Mangla and Indu Jayaluxmi

Anonymous Referee #1

Received and published: 4 December 2018

This paper describes a comparison between GMI microwave observations and radiative transfer simulations performed with the RTTOV-SCATT model and WRF forecasts as well as ERA-Interim and ERA-5 forecasts. Major work would be needed for this work to be considered for publication: the paper is poorly structured (e.g. WRF simulations are mentioned before explaining that WRF is used in the study ; the order of the paragraphs in section 3 does not make sense and it is not justified why ERA-Interim and ERA-5 datasets are used together with WRF without any obvious link between the two). Many statements are not justified by statistics but only by figures on which it is hard to see something. I would suggest to fully rewrite the paper with better selected results (either WRF only or ERA results) and figures supporting the findings.

Printer-friendly version

Discussion paper

Specific comments:

Introduction: Line 61: “Geer and Baordo, (2014) claimed that DDA sector snowflake is approximately fit for all frequencies at a global scale” => “Claimed” is not the appropriate word, the authors of this paper showed that the sector snowflake is a compromise over a large range of frequencies and over the globe with a large dataset.

Line 67: “FG departures” => FG has not been introduced before

Line 78: “Geer, (2013) used the same model at multiple frequencies of TMI and SSMIS channel.” => The same model was not used, the parameters of it have been adapted to each sensor and each frequency.

Line 79: “In microwave spectrum, the symmetric error model is known to perform well for low frequencies” => The error model was used as well for high frequency, please read the papers around MHS all sky assimilation.

Line 82: “At higher microwave frequencies, the backscatter/brightness temperature registered by the sensor is mainly due to scattering from frozen hydrometeors, assuming a spherical shape” => Please do not mix backscatter and brightness temperature, they refer to two different things. The physical phenomena at play at high frequencies have nothing to do with “assuming a spherical shape”, there is a confusion here between the physical phenomenon and the modeling of it. This sentence need to be rewritten.

Line 84: “Long-term monitoring of FG departure was found useful for identifying the instrumental error from ground based microwave observation (De Angelis et al., 2017).” => This sentence does not have much to do with the rest of the text. Please remove it.

Line 89: ”In addition, we also include the analysis of ERA-5 reanalysis datasets (Malardel et 90 al., 2015) to extend the sensitivity to cloud physical processes at a higher resolution.” => The authors mention ERA-5 but do not even explain before with which model they worked over India.

Data and method:

Line 109: “insensitive at 183 GHz.” => This is a wrong statement. 183 GHz observations are sensitive to surface emissivity specification. This is why dynamical emissivity retrieval techniques have been developed for data assimilation.

Line 130: “The surface emissivity over oceans are calucated by the surface parameters” => calculated with

Paragraph 2.2 : the WRF model is mentioned here but is presented later, please restructure this section to present the information in a proper order

Results and Discussion:

Line 159 to 165: These details of the radiative transfer simulations should be given in section 2.2 Line 172: “upto Âă70-80 K” => down to 70-80K

Line 173: “Underestimation was observed using the mie-sphere, sector snowflake and six-bullet rosette shapes. Though the overall pattern and location of convective clouds near the eye of cyclone matched closely with the observations” => These statements are not supported by any statistics at this stage of the text

Line 182: “A negative departure occurs when the RTToV model is unable to produce realistic representations owing to cloud and precipitation.” => This statement is wrong, a negative departure occur at 183 GHz when the simulated brightness temperatures are too warm with respect to the observation.

Line 183 to 187: “Within DDA shapes, the pdf curve is found to follow a symmetric distribution. ...” => The figure shown does not support any of this statement.

Line 188: “observation errros “ => errors

Line 202: “As the quantities of . . . and ... are affected with sampling error (Geer and Bauer, 2011), their average is considered as the average cloud amount ...” => This sentence does not make sense.

Line 208: “The sudden peak at . . . 0.8 ” => These peaks are likely due to a smaller number of samples in these categories. The number of samples used is an important information to provide when trying to understand a result.

Line 217: “The bandwidth of FG departures are very high (Figure 4) and finding a symmetric bias in absolute FG departure is not feasible. “ => This sentence does not make sense

Line 221: “From Figure 6, it can be seen that, the normalized FG departure curves follow symmetric distribution but its peak was too high with smaller errors. ” => From the results of the normalization, it seems the distributions obtained are far from Gaussian. There must be a problem with the error model itself but unfortunately it is not shown in the paper.

3.4 Quality Control (QC) => Again the comments are not supported by statistics and it is hard to see the effect of the QC selected. There are also mistakes in the text, for instance normalized FG departures do not have a physical units like Kelvin (line 236). 3.5 Measure of goodness of fit => It does not make sense to have these results presented here after the error models. 3.6 Sensitive to ERA-5 reanalysis datasets => The results presented in this section are poorly introduced and have little connection with the results of the rest of the paper

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-319, 2018.

Printer-friendly version

Discussion paper