Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-300

Toshi Matsui (Referee)

Referee comment on "Numerical model generation of test frames for pre-launch studies of EarthCARE's retrieval algorithms and data management system" by Zhipeng Qu et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-300-RC1, 2023

Summary: This manuscript describes generation and validation of numerical atmosphere-surface test frames for upcoming EarthCARE satellites. Authors conducted multiple multi-scale modeling to generate high-resolution scenes of virtual atmosphere and surface across different regions. The manuscript, then, qualitatively validated generated virtual fields against the satellite observations. This is a very ambitious paper that intends to validate this many different scenes all together in a single manuscript. This is why the validation method lacks detailed quantitative analysis and non-evident arguments in ice microphysics. Other problems include lack of citations related to previous studies and lack of aerosol validation. Thus, this paper needs "major revision" to resolve aforementioned problems before publishing in AMT. Please read the major and minor comments below.

Major Comments:

1) Lack of citations and discussion in the previous studies.

This numerical satellite mission frame has emerged from the last decades. We have developed a benchmark of numerical test frames before the launch of Global Precipitation Measurements (GPM) Core satellite.

Matsui, T. T. Iguchi, X. Li, M. Han, W.-K. Tao, W. Petersen, T. L'Ecuyer, R. Meneghini, W.

Olson, C. D. Kummerow, A. Y. Hou, M. R. Schwaller, E. F. Stocker, J. Kwiatkowski (2013), GPM satellite simulator over ground validation sites, Bull. Amer. Meteor. Soc., 94, 1653–1660. doi: http://dx.doi.org/10.1175/BAMS-D-12-00160.1

For this, we have validated numerical simulation against the various measurements from the GPM field campaigns.

Iguchi T., T. Matsui, J. J. Shi, W.-K. Tao, A. P. Khain, A. Hou, R. Cifelli, A. Heymsfield, and A. Tokay (2012), Numerical analysis using WRF-SBM for the cloud microphysical structures in the C3VP field campaign: Impacts of supercooled droplets and resultant riming on snow microphysics, Journal of Geophysical Research, 117, D23206, doi:10.1029/2012JD018101.

Iguchi, T., T. Matsui, A. Tokay, P. Kollias, and W.-K. Tao (2012), Two distinct modes in one-day rainfall event during MC3E field campaign: Analyses of disdrometer observations and WRF-SBM simulation. Geophysical Research Letters, 39, L24805, doi:10.1029/2012GL053329.

Iguchi, T., T. Matsui, W. Tao, A. Khain, V. Phillips, C. Kidd, T. L'Ecuyer, S. Braun, and A. Hou, 2014: WRF-SBM simulations of melting layer structure in mixed-phase precipitation events observed during LPVEx. J. Appl. Meteor. Climatol. 53, 2710-2731, doi:10.1175/JAMC-D-13-0334.1.

There should be a lot more papers related to these topics. Please search manuscripts and discuss similarities and differences between your work and other previous work in the introduction section.

2) Lack of quantitative validation.

This paper intends to validate many different scenes that contain many different types of clouds and microphysics. All validation here is qualitative “eyeballs” validation. Thus, the results are stated “very good (line 209). At least, I suggest creating histograms or PDFs for each plot (Figs. 8, 9, 11, 12, 14) for more quantitative discussion in addition to existing qualitative validation. You can see Fig 1d in Matsui et al. 2014 for example.

Matsui, T., J. Santanello, J. J. Shi, W.-K. Tao, D. Wu, C. Peters-Lidard, E. Kemp, M. Chin,

D. Starr, M. Sekiguchi, and F. Aires, (2014): Introducing multisensor satellite radiance-based evaluation for regional Earth System modeling, Journal of Geophysical Research, 119, 8450–8475, doi:10.1002/2013JD021424.

I also suggest utilizing more detailed satellite simulators to generate observation-equivalent scenes. For example, RRTMG is broad-band RT. The COSP simulator does not account for details in size and phase of MY2. Detailed satellite simulator must closely follow assumptions in size distributions, phase, and shapes in cloud microphysics. You can read section 2 of above paper for more principles and radiance-based model evaluation.

3) Non-evident argument of ice microphysics bias and improvement.

Section 7 argument suddenly starts with "Basically, GEM predicts too many overly small ice crystals….". Unfortunately, I don't see any such evidence in this manuscript or previous manuscript using MY2 microphysics. What is this argument based upon? You must show evidence using either observations or previous manuscript using MY2 in different cloud types. Then, discussion goes to "these alterations were found to …. Improvement of GEMS's simulated cloud properties…." Again, based on what?? No evidence. Essentially Figure 17 compares Reff-T distributions before and after modification.

There are potential pathways to provide this evidence.

- Validate against Reff products of MODIS/VIIRS satellites. Although these products have their own assumptions, it is better than nothing.
- Simulate CloudSat reflectivity before and after the change in ice size. In this case, you must use a detailed radar simulator that accounts for size, phase, and non-sphericity of ice crystals.

Reff is the inverse function of lambda in generalized gamma distributions, thus, change in Reff can significantly impact CloudSat radar reflectivity. You can construct contoured frequency of altitude diagrams (CFADs) or similar statistical composites. See examples Fig 10 & 11 of Shi et al. 2010 for example.

Shi, J. J., W.-K. Tao, T. Matsui, A. Hou, S. Lang, C. Peters-Lidard, G. Jackson, R. Cifelli, S. Rutledge, and W. Petersen (2010), Microphysical Properties of the January 20-22 2007 Snow Events over Canada: Comparison with in-situ and Satellite Observations. Journal of Applied Meteorology and Climatology. 49(11), 2246–2266.

4) Lack of aerosol validation.

Although aerosols are part of the numerical testing frame, only cloud properties are validated. Aerosols are supposed to be one of the major components of EarthCARE satellites, right? Why not validate aerosols? I understand that it is incorporated from the CAMS field, and applied ECSIM scattering properties. You can simulate these quantities and present statistical quantities. Are these realistic against existing Lidar measurements, like CALIOP/CATS sensors? See example of how to validate aerosol total backscattering in Figs 16 of Choi et al. 2020.

Choi, Y., S.-H. Chen, C.-C. Huang, K. Earl, C.-Y. Chen, C. Schwartz, and T. Matsui (2020), Evaluating the impact of assimilating aerosol optical depth observations on dust forecasts over North Africa and the East Atlantic using different data assimilation methods, Journal of Advances in Modeling Earth Systems, 12, e2019MS001890. https://doi.org/10.1029/2019MS001890

Minor Comments:

Title: Title including “Data Management”, but I don’t see particular discussion in DM. Either omit DM from title, or add more plentiful discussion of DM.

Line 13: Write acronym NWP in abstract.

Line 39: “ensemble” does not sound right. It probably means “diverse surface-atmosphere scene”

Line 112-115: Is PBL scheme and shallow convection scheme applied to which domains (coarse grid only)?

Elsewhere: “inner-domains” should be “inner-most domains”.

Line 127: “Saved variables are listed in the Appendix” but I guess it’s also available in Table 5??

Line 138-142: Equations of spectral and white albedo can be omitted since they’re not very important for this paper. Just a reference is enough or say “white albedo is used for diffuse radiation”.

Line 149: Please put a citation for “another emissivity database”, although I understood it is more discussed in the companion paper.

Line 172: Omit sentence “While annoying to look at,”

Line 187 & Figure 10: “making a comparison to GEM useless” This statement is too strong.

Line 195: 1D RT is not the major reason to create homogeneous brightness temperature. Mis-representation of cloud structures due to missing mesoscale forcing should be the main reason.

Line 209: “very good” is too qualitative and vague a statement (not scientific statement).

Line 230: “by algorithm development groups (….” should be “in the previous section.”