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Atmospheric Measurement Techniques Discussions

Interactive comment on "Analysis and evaluation of WRF microphysical schemes for deep moist convection over Southeastern South America (SESA) using microwave satellite observations and radiative transfer simulations" *by* Victoria Sol Galligani et al.

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This is an interesting study evaluating the ability of WRF simulations to produce realistic convection simulations, using the model-to-satellite approach, with satellite-observed microwave radiances as the reference. A first highlight of the paper is the novel 'equal mass approach' for converting WRF model hydrometeor particles to their equivalents in the forward radiative transfer. This is a pragmatic and useful way to match up particles that may have different geometric representations in the moist physics and the radia-

tive transfer. A second highlight is the evaluation of the WRF microphysics schemes, indicating substantial differences between WSM6/WDM6 and Thompson microphysics schemes. The manuscript is generally fit for publication, but there a few main points for the authors to consider:

1) In the introduction, or at the end of section 2.2, it would be good to survey all previous validation of the WRF microphysics and to summarise any known issues.

2) A minor but repeated issue (e.g. lines 142-144; lines 468, 478, 495) is the attribution of brightness temperature (TB) depressions at low frequencies (e.g. 85 GHz or less) uniquely to scattering. Over land, at low frequencies, cloud water and particularly rain (and possibly snow too) can generate TB depressions through absorption and emission pushing the weighting function up to colder layers in the atmosphere. It would be good to examine more closely whether it really is scattering causing the TB depressions in all cases. Incorrect modelling of the cloud water or rain could also contribute to mismatches between observations and simulations.

3) A feature of the equal-mass approach is that it changes the relative amount of scattering generated by the Liu particles, in one case making the sector snowflake the most scattering particle (e.g. lines 506-510; 537-541). This is one of the most interesting aspects of the study and it could do with further exploration in the text (and possibly more figures) to explain exactly how this occurs (note the lack of labelling on Fig. 6 has not helped here - minor point 8).

4) Some of figures 16-20 could be considered for reduction, as they mainly repeat and confirm the results of the case study in section 4.

5) Some figures are too small and fuzzy - e.g. Figure 3 longitude and latitude legends; Figs 9, 10, 11, 12.

Minor issues

1) Line 232: "As expected WSM6 and WDM6 schemes model similar ...loadings": for

the benefit of the reader, could the reason be restated here, instead of just saying "as expected"?

2) Line 308-311: "The only computationally realistic approach is to assume a one-shape model". This statement could be challenged: an ensemble of particles could be used without much additional computational effort - for example something like the Baran (2009) ensemble.

3) Lines 338-339, 445-447. In both these areas the question arises "are both snow and graupel simulated using the same Liu particle habit?" The answer is probably yes, but it would be worth (re?)-stating this for the benefit of the reader.

4) Line 524: "The higher the window channel" - higher what? frequency?

5) Line 602-603: "WDM6 leads to excesive scattering at > 19 GHz". This is not obvious to me. At 37 GHz WDM6 is the only model to generate TB depressions as low as observed, albeit over a wider area than observed. At 89 GHz, none of the schemes generates sufficient TB depression.

6) Line 603-604: "Figure 14 shows good agreement" - this could be restated in more depth and a little more critically. For example there is the broader spread of TB depressions generated by THOM, versus perhaps too-narrow areas of TB depression from the other schemes. As in Fig. 13, none of the schemes have deep enough depressions at 89 GHz.

7) Line 709-710: poor wording in this sentence suggests that the Liu habits all have the same bulk scattering properties - please rephrase.

8) Figure 6 has no key for the black dashed line in the left panels, or the significance of solid versus dashed in the right panel.

9) Figure 8 needs a key to the black dashed line.

10) The many figures featuring chi-squared ststs are not all consitent in terms of the y

axis labelling: some use "#()" (not explained) and some "E()" (explained in the text)

Grammar points

1) There is a repeated error in the text that writes "South Easter" instead of the correct "South Eastern"

2) Line 165: "described" should be "describe"

3) In a few places, "sensibility" is used in place of the correct english term "sensitivity".

4) Line 230 "similarly to TMI observations" is hard to understand and the sentence should be rewritten for clarity.

References

Baran, A.J., Connolly, P.J. and Lee, C., 2009. Testing an ensemble model of cirrus ice crystals using midlatitude in situ estimates of ice water content, volume extinction coefficient and the total solar optical depth. Journal of Quantitative Spectroscopy and Radiative Transfer, 110(14), pp.1579-1598.

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