

Interactive comment on “Simulating the effects of mid- to upper-tropospheric clouds on microwave emissions in EC-Earth using COSP” by M. S. Johnston et al.

Anonymous Referee #2

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Review of ‘Simulating the effects of mid- to upper-tropospheric clouds on microwave emissions in EC-Earth using COSP’, by M.S. Johnston, G. Holl, J. Hocking, S. J. Cooper, and D. Chen, submitted to AMT

This paper summarizes work regarding a new capability of the CFMIP COSP simulator package to deal with cloudy microwave (MW) radiances. Previously, only clear-sky brightness temperatures (BT) are calculated from COSP. This study describes (1) what the authors have done to develop the new capability, and (2) an application of the MW simulator based (in part) on ECMWF output, and is then compared to observed MW BTs based on 190 GHz data from the NOAA-18 Microwave Humidity Sounder (MHS).

C4242

Overall, I was disappointed in this study for a few reasons.

First, with regard to the development of the new COSP simulator capability, only one channel (190 GHz) with scattering is included and described. The authors acknowledge on page 11756, lines 25-27, that this work could be extended to other channels, but it isn’t described at all if this work has been done. Why not make a flexible simulator for all of the MW channels in instruments such as MHS, AMSU-B, and others that would be more useful than a single channel modification to COSP? There is no discussion on the motivation or reasons for restricting this effort to a single channel.

Second, the authors acknowledge on page 11757, lines 7-9, and elsewhere, that the scattering will very strongly depend on both precipitating hydrometeors and suspended cloud hydrometeors, and for ice cloud on the ice particle shape and size distributions. No analysis on the relative importance of cloud versus precipitation is shown in separate calculations, which would be useful. Default microphysics settings in RTTOV are used and no analysis on the sensitivity of the MW BTs are shown with adjustments in these default settings. A sensitivity study of MW BTs to microphysics is warranted.

Third, there are a total of four figures shown which basically present the same data in different ways without much additional insight. In areas of convection in the tropics, large deviations up to 40 K are shown, but the authors basically stop there without any effort in understanding this bias. Is it the scattering? Absorption? Assumptions of microphysical size distributions? Is the source of it from the assumption of a constant 1 m/s fall velocity to make water content profiles from precipitation that are then added to cloud hydrometeors? Are there other factors at play? In the Discussion section, the authors describe how important microphysics are, and cite some other work to support it, but they haven’t actually done anything in the paper that is useful and new regarding the microphysics besides flipping the switch to ‘on’ within RTTOV.

In summary, this work is incomplete and, at the least, requires major revisions and a significant amount of additional work. On the positive side, it is nice to see work

C4243

on cloudy MW radiances and I hope the authors can revise the paper accordingly or resubmit an improved manuscript in the near future.

Additional comments

Page 11757, line 13: there is no attempt made to simulate precipitation and cloud contributions separately. This would be a valuable contribution and may help determine the source of the 40 K bias in convection.

Page 11758, lines 14-17: what is the basis for the assumption of a constant 1 m/s value? There is no citation or description of the reasoning. Surely in convection, especially in the updrafts, the vertical velocity will be an order of magnitude or larger than this value. What about downdrafts? Turbulent flow? What about larger, organized convective systems that fill the MW field of view in observations versus sub-pixel, isolated convection?

Page 11759, lines 19-20: different CFMIP models have different overlap assumptions. Does RTTOV have flexibility to mimic these assumptions made in different climate models?

Page 11760, lines 4-14: the use of default settings is not very insightful. This work warrants a sensitivity study, or the appropriate citation(s) of previous work that convincingly supports the use of the default settings.

Page 11760, line 10: constant density in what?

Page 11762, equation (2): It is odd that only one channel is described in the simulations yet three channels are used to distinguish convection apart from other scenes which exploits spectral signatures in the MW BTs. This doesn't make much sense.

Page 11764, line 9: are the 3080 cases individual pixels in satellite obs? Averaged values within grid boxes in EC-Earth? Aggregated convective 'features' or 'clusters'?

Page 11764, line 12: why not show it? This paper is already sparse on detail and very

C4244

abbreviated

Page 11764, line 167: why not show the standard deviation in BT? That would be very interesting to discuss.

Page 11764, line 24: again, why not show these results?

4 figures: they show basically the same thing but either averaged values or deviations/anomalies between EC-Earth and satellite MHS data. The discussion of the figures lacks any real insight to the biases in BT.

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C4245