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Atmospheric Measurement Techniques Discussions

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 #3

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Simulating the effects of mid- to upper-tropospheric clouds on microwave emissions in EC-Earth using COSP

Johnston, Holl, Hocking, Cooper and Chen

The paper describes the use of the RRTOV simulator to compute the all-sky microwave emission at a particular channel. This is done within the framework of COSP, applied to EC-Earth output and used to evaluate the model against observations from MHS.

While the point of the paper is well defined, clouds are now considered in RRTOV and the brightness temperatures are used to evaluate EC-Earth, what material is new needs to be clearer (what did the authors add) and the analysis needs to be improved

beyond plots of differences in the brightness temperatures. For example, some of the uncertainties due to using only the brightness temperature to evaluate EC-Earth could be explored using other simulator output from COSP and associated observations.

Therefore I recommend major revisions.

Comments:

1. The title is a bit vague as COSP is used to "house" the RRTOV simulator which is used to compute the microwave emissions. It would be clearer to indicate this in the title by changing COSP to RRTOV.

2. Similar to the title, some of the text is made vague when using the term COSP. For example, in the abstract there is this sentence,

"However, COSP is unable to simulate sufficiently low TB in areas of frequent deep convection."

which suggests there is a problem with microwave simulator in COSP. Is this the case or is it the case that EC-Earth does not simulate an atmospheric state that translates to low brightness temperatures?

3. Near line 17, page 11754 - It is hypothesized that errors in the amount of simulated cloud ice water may be an issue. This quantity is provided by other simulators in COSP and datasets, e.g., MODIS and CloudSat. Can't this be evaluated? Would some of these issues be reduced if one worked with geophysical parameters (Holl et al., 2014) rather than directly with brightness temperatures? Or is there a trade off?

These two papers are examples of evaluating simulated cloud ice using A-train data,

Jiang, J., et al, Evaluation of cloud and water vapor simulations in CMIP5 climate models using NASA A-Train J. Geophys. Res., 2012, 117, D14105-

Li, J.-L. F. et al., An observationally based evaluation of cloud ice water in CMIP3 and CMIP5 GCMs and contemporary reanalyses using contemporary satellite data J.

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Geophys. Res., 2012, 117, D16105-

4. Near line 17, 11756 - It is stated that,

"Our choice of this channel is motivated by the veritable dearth of studies that examine simulated clouds and precipitation scattering at this particular frequency."

This does not give a strong reason to the reader why the focus is on this particular channel rather than one or several of the other channels measured by MHS. At the end of Sec. 2.3 it is noted that multiple channels are used to retrieve quantitative information about ice clouds. Why not include these other channels in RRTOV and consider emulating the retrievals by Holl et al., 2014 or others, e.g., the identification of deep convection (Eq. 2)?

5. Near line 16, page 11758 - "assuming a constant fall speed of 1 m sˆ-1". Is this consistent with the model physics and therefore accurately represents the vertical distribution of precipitation mixing ratios? If not where does it come from. What does it mean to "merged with the large scale precipitation"?

6. Section 2.2 - It is not clear here what is your contribution to the enable clouds to be included in RRTOV. Was it basically turning on what already was present in RTTOV but not used within COSP, which is how it currently reads, or did you have to do more?

The comment about cloud, and precipitation, overlap (line 20, page 11759) and Eq. 1 should be expanded. If RTTOV is using its own overlap method from Geer, 2009 it may be inconsistent with the other simulators in COSP. Within COSP there are "subgrid-scale" generators to overlap clouds and precipitation using specified rules that are consistent with the model to which COSP is being applied. If the all-sky RTTOV brightness temperatures are to be used with the other outputs from COSP it should use a consistent treatment of the hydrometers, e.g., ISCCP, MODIS and CloudSat and the host model.

Does the all-sky RTTOV respect this assumed overlap? If not how large are the devia-

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tions due to assuming the Geer, 2009 overlap? Is RTTOV applied to the entire gridbox or applied to "subcolumns" and then averaged like the other simulators in COSP?

7. Section 3.1 - By limiting yourself to radiances within 5 degree of nadir could there be some error introduced due to sampling differences since EC-Earth, I assume, is sampled over all gridpoints and every model timestep. If there is a strong diurnal cycle then perhaps this could contribute to some of the differences.

8. Line 5, page 11763 - Is is not explicitly stated that the results shown in Figure 1 are unfiltered results. That said I would suggest merging Figures 1 and 2 by adding the right column of Figure 1 to Figure 2, i.e., the columns of the new figure from left to right would be MHS observations, the difference and then the effect of filtering. Currently Figures 1 and 2 are partially repeating results and a merged figure would make it easier to see what regions are less certain and filtered out.

9. Line 7, page 11765 - Since you have COSP and access to EC-Earth output why not compare the ice water path and ice water contents to results from MODIS, CloudSat or other observations? While not definitive it would give an indication if the simulated ice clouds have any significant biases. This is suggested in Sec. 4 (line 8, page 11767).

10. Can you attach significance or physical insight to the biases, especially the 3K bias, in brightness temperature? For readers who are not use to working with brightness temperatures, it is not clear if 3K is still a significant bias or not.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 11753, 2015.