Authors' response on the review of the article

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January 20, 2016

Article: *Simulating the effects of mid- to upper-tropospheric clouds on microwave emissions in EC-Earth using COSP*
Journal: Atmospheric Measurements and Techniques

1 Summary

We would like to express our sincerest gratitude to the reviewers who have taken their time to critique our work. Also, due the reviews coming in just before the holidays and so close to the end of the discussion session, it has taken some time to negotiate an extension of the deadlines. Finally, the broad range of issues brought up by the reviewers took time to properly address. Therefore, we ask for some understanding in the timing of the response.

We believe that some of the reviewers have misunderstood the objective of the paper and therefore drove the discussion of the paper into areas that went some way beyond the scope of the original objective. We realize also that this partly our fault for not making the objective clearer and we will adjust the article accordingly.

The scope of the work was largely shaped by the funding available, ad therefore the time available to simulate more sensors and do more analysis. The foundation of the work surrounds the integration of original 5 simulators (MODIS, CloudSat-CALISO, ISCCP, MISR, and RTTOV) into EC-Earth. This entails creating an interface to the model and translating all the necessary variables from the model to COSP. Moreover, the RTTOV interface provided a capability for clear-sky simulations only. We have expanded the RTTOV interface to make it all-sky capable for MW sensors and we thought it prudent the inform the community of this new all-sky tool by showing a demonstration of COSP via RTTOV-SCATT (the RTTOV MW scattering model) in the hope of stimulating more microwave studies to tackle the problems highlighted in the study.

Funds have recently been granted for further work in this area and as such the completion of this paper will be incorporated into this new project. This gives the authors another month to actually extend the work. We have already expanded the RTTOV-COSP interface to include zenith angles and sub-grid variability via SCOPS.

A common critique of the paper was the need to expand it beyond it's current form. We agree to this and suggest additions that we hope will satisfy the reviewers. We are in the process of conducting the following:

1. Expand the capability of the COSP-RTTOV interface to include the sim-

ulation of sub-grid variability and off-nadir zenith angles (already implemented).

2. Filter the modeled atmosphere to to match the local times of the observations.

3. Show the results from other MHS channels as a further demonstration of the COSP-RTTOV.

4. Examine the sensitivity of model brightness temperatures to assumed cloud and precipitation properties. We will perturb both cloud/precipitation scattering properties and mass.

Please consider our proposals and discussion below. The reviewer's actual comment is given in blue and our response in directly after in black.

2 Reviewer 1

2.1 Comment 1

This study only focuses on 190.31 GHz, but I think it would greatly enhance the study to include more frequencies. The authors mention (11767, line 9) that including more frequencies could help in identifying the source of the biases in the model. Including that analysis would add more depth to the manuscript. Also, the authors focus only on near-nadir looks of MHS. Even if you would only use the one frequency 190.31, wouldnt using other look angles help to identify the sources of bias in the model since youll be able to get information about the atmospheric profile? I think it would be very interesting to see how the biases change as a function of look angle.

The consensus is that the article should be expanded to add simulations from more channels as the results at other frequencies could give some insight the biases seen in the MHS results. The authors agree and have discussed the possible sensors/channels that might add some clarity. We feel the ideal thing to do is show results from channels that have differing sensitivity to both clouds and precipitation. TRMM Microwave Imager overlaps NOAA-18 MHS in time, i.e., both datasets cover the year 2006. TMI channels that measure precipitation are at 10.7, 19.4, 21.3, 37, and 85.5 GHz. These lower frequency channels are less sensitive to cloud droplets than the MHS 190.311 GHz channel.

We have compiled the NOAA18 MHS observations up to $\pm 5°$ off-nadir in order to increase the sample size. We will include previous studies that support taking the mean of these smaller, off-nadir angles as the differences are quite small. Therefore it is not necessary to simulate MHS at these smaller angles as this will only confirm what we already know.

2.2 Comment 2

I was a bit confused by Fig. 1 when I first read through the manuscript since you had just finished explaining the various filters you were going to use on the data show in Eq. 2 and Table 1, but then Fig. 1 looks like it uses all data without any filters. Perhaps make it more obvious in the text (11763, 1st paragraph) that youre first going to show all data with no filters. However, in the conclusions (11766, line 12) you say clear-sky calculations are omitted from the study. Does this mean you did not include clear-sky conditions in Fig. 1? This was not obvious in the text.

We have shown in figure 1 the full simulation of RTTOV using the EC-Earth atmosphere. However, this figure contains areas where the uncertainties are very large, especially when showing the difference. This is why figure 2 excluded these areas. We are prepared to remove these areas of large uncertainties already in figure 1 and adjust the text to ensure better clarity. Either way, this issue will be addressed.

2.3 Comment 3

11764, paragraph 2. This is a confusing paragraph to follow. Is there some way to graphically show what you are doing?

We will include a figure to aid in understanding the text.

2.4 Technical comments

11754, line 16. I believe "underestimation" should be "overestimation" 11758, line 8. Take out all the ands and make a list: "cloud ice, cloud water, precipitating ice (snow), and rain"

These will be addressed before re-submission.

3 Reviewer 2

3.1 Comment 1

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 isnt 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.

We understand the reviewer's frustration and disappointment. Please see Section 2.1 and the summary for our plan to address this issue.

3.2 Comment 2

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.

We agree that a study of the microphysics is warranted but not in this study. Any robust study of the 3D spatial distribution of atmospheric hydrometeors, the different ice habits, the various assumptions used in both the observations, climate model, and RTTOV, represent a large amount of work that cannot be solved with passive MW sensors alone. We are willing to demonstrate the sensitivity of simulated profiles to various hydrometeor distribution as well as microphyical assumptions. This combined with citation from previous work in these areas should satisfy the demand here.

3.3 Comment 3

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 havent actually done anything in the paper that is useful and new regarding the microphysics besides flipping the switch to "on" within RTTOV.

The interface to RTTOV in COSP has been extended and upgraded to simulate all-sky conditions in all channels on passive MW sensors. The full list of supported sensors can be found in the RTTOV documentation. However we did not explicitly mention this in the article. We chose to show only the MHS channel and only channel 5 as a demonstration of the new COSP capability. We will add the TMI sensor and modify the text to better explain the new COSP-RTTOV interface and its capability as well as better motivate our choice of sensors and channels to use in the demonstration/article.

A large part of this work went into integrating RTTOV-SCATT into COSP and extending the COSP inputs to account for clouds and precipitation. Prior to our study all that was available the modeling community was clear-sky simulations. Changing the microphysics used by RTTOV is something we are going to examine and add to the paper. Similarly, the use of 1 m/s fall speed was chosen based on previous work which will be cited and added to the paper. To our knowledge, no model has fully addressed the fall speed aspect of precipitation, if they are at all treat precipitation in a consistent manner, i.e., diagnostically verses prognostically, as well as treating convection (updrafts and downdrafts) explicitly. In order to do this we would need another type of model entirely.

3.4 Comment 4

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.

This is a good point: we will do this.

3.5 Comment 5

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?

While these issues are good points, an investigation into these issues goes well beyond the scope of any single article. For instance, there are many studies just on the fall speed alone. The design of the climate model and COSP does not currently allow for the separation and individual treatment of convective clusters verses isolated convection nor for changing the fall speed parameter to a more integrated, mass-based parametrization. Many of these aspects fall within the realm of the model and not COSP. Changes on the level the reviewer is suggesting would be something the require an broad-scale evaluation of the model and published in multiple papers. The fall speed in the model is far from perfect, but in order simulate real world conditions with updraft and the the like would require a cloud resolving model inside our GCM. This configuration called a Multi-Modeling Framework is currently not possible for EC-Earth.

3.6 Comment 6

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

The idea behind COSP is to capture the model parameters before any overlap assumption is applied. This is a fundamental feature of COSP. By applying only one assumption to all models, any error based on the assumption become systematic and allow for a better inter-comparison of different models. Now we have made use of the COSP subgrid variability with RTTOV-SCATT which brings RTTOV-SCATT inline with the other simulators in COSP. RTTOV-SCATT applies only 1 cloud overlap assumption to all simulations alike.

3.7 Comment 7

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.

The main focus of the study is to highlight the new COSP features, i.e., expanding RTTOV to simulate all-sky brightness temperatures. Undoubtedly the new simulations are built on the work done by other studies. We did not feel it necessary to repeat studies of the past and did quote the relevant studies upon which RTTOV's MW scattering algorithm and current default optical properties are based. However, some will do some sensitivity study in this area.

3.8 Comment 8

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 Tbs. This doesnt make much sense.

The constant density will be explained in more detail. We tried to show that the areas of depressed brightness temperatures are in areas of frequent convection, such as the Tropical Warm Pool and the Amazon Basin.

3.9 Comment 9-10

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"?

This will be made better explained in the paper.

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

This figure will be added back into the paper.

4 Reviewer 3

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.

4.1 Comment 1-2

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. 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?

We present RTTOV as one of the simulators available to the modeling and measurement community in a manner that is novel but under development. COSP provides a standardized method of passing GCM data to a variety of observation simulators, one of which is RTTOV. The treatment of sub-grid variability is done in COSP and passed to RTTOV. We will nevertheless consider changing the title in a manner that better reflects the objective of the paper. Likewise we will address the confusion of writing COSP in places where RTTOV is a better term.

4.2 Comment 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. Cant 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?

CloudSat since it's mission start back in 2006 has been used to study cloud ice water. However, as stated in Waliser et al 2009, comparing CloudSat's more complete ice, i.e., range from small cloud ice to large precipitating ice, to the models prognostic and diagnostic treatment of ice, along with a diffuse line of demarcation forms one of the large barriers to constraining the global ice water content in models. Models do not treat their ice in a manner that is consistent across the community and therefore large variations arise when inter-comparing the geophysical parameters. Part of COSP's appeal is the possibility of at the very least standardizing the evaluation of cloud ice where possible. So you are right, there is a trade off. By doing such studies we hope to nudge the modeling community towards standardizing the treatment of ice in their models and providing the microphysics used to generate their ice.

Nevertheless, looking at cloud ice is part of the newly funded study that will take such aspect into account, but not in this paper.

4.3 Comment 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)?

In the study we did use the 3 water vapor channels to identify deep convection in the simulated profiles. However since the the simulated profiles are not as sensitive to the decrease in brightness temperatures normally associated with deep convection, this technique could not be used. The technique was used in the observations to identify areas of deep convection and highlight this shortcoming in the model. We will endeavor to make this clearer in the paper.

4.4 Comment 5

Near line 16, page 11758 - "assuming a constant fall speed of 1 m/s". 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"?

We will motivate the use of a constant fall speed. In the model, large-scale, i.e., non-convective, is merged with the convective component to build a total precipitation variable. In this model, precipitation is treated diagnostically and therefore given as mass fluxes.

4.5 Comment 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?

See previous comment in Sect. 3.3.

4.6 Comment 7

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 deviations 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?

We agree that ensuring the overlap assumption agree across the simulators. To this end we have used the COSP sub-grid variability with the RTTOV-SCATT simulator.

4.7 Comment 8

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.

This is a good point and something we realized afterwards. We will sample the model at the time closest to the observation in an effort to be reduce this effect.

4.8 Comment 9

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.

This is a good idea that we will incorporate into the revised version.

4.9 Comment 10

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).

This would be another study entirely and one that we plan to do in the near future with another simulator.

4.10 Comment 11

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.

We will try and make the biases a bit more understandable.

5 Reviewers comments

Atmos. Meas. Tech. Discuss., 8, C4436–C4437, 2015
www.atmos-meas-tech-discuss.net/8/C4436/2015/

Atmospheric Measurement Techniques Discussions

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8, C4436–C4437, 2015

Interactive Comment

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 #1

Received and published: 21 December 2015

This manuscript describes a new version of COSP that includes simulation of cloudy scenes in the microwave spectrum, where the previous version only included clear sky scenes. I think this is an important topic since being able to simulate more than just clear sky scenes has a great impact on analysis that can be done, however, I find this manuscript to be lacking in-depth discussion of the topic. Only one microwave frequency is analyzed, and at only one look angle. The authors just speculate on the causes of biases in the results, rather than actually do a study to find out what could be the cause of the biases. A more comprehensive analysis should be done.

This study only focuses on 190.31 GHz, but I think it would greatly enhance the study

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to include more frequencies. The authors mention (11767, line 9) that including more frequencies could help in identifying the source of the biases in the model. Including that analysis would add more depth to the manuscript. Also, the authors focus only on near-nadir looks of MHS. Even if you would only use the one frequency 190.31, wouldn't using other look angles help to identify the sources of bias in the model since you'll be able to get information about the atmospheric profile? I think it would be very interesting to see how the biases change as a function of look angle.

I was a bit confused by Fig. 1 when I first read through the manuscript since you had just finished explaining the various filters you were going to use on the data show in Eq. 2 and Table 1, but then Fig. 1 looks like it uses all data without any filters. Perhaps make it more obvious in the text (11763, 1st paragraph) that you're first going to show all data with no filters. However, in the conclusions (11766, line 12) you say clear-sky calculations are omitted from the study. Does this mean you did not include clear-sky conditions in Fig. 1? This was not obvious in the text.

11764, paragraph 2. This is a confusing paragraph to follow. Is there some way to graphically show what you are doing?

Technical comments:

11754, line 16. I believe "underestimation" should be "overestimation"

11758, line 8. Take out all the 'ands' and make a list: "cloud ice, cloud water, precipitating ice (snow), and rain"

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

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8, C4436–C4437, 2015

Interactive Comment

Atmos. Meas. Tech. Discuss., 8, C4242–C4245, 2015
www.atmos-meas-tech-discuss.net/8/C4242/2015/

Atmospheric Measurement Techniques Discussions
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8, C4242–C4245, 2015

Interactive Comment

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

Received and published: 9 December 2015

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).

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

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8, C4242–C4245, 2015

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

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8, C4242–C4245, 2015

Interactive Comment

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.

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

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8, C4242–C4245, 2015

Interactive Comment

Atmos. Meas. Tech. Discuss., 8, C4468–C4471, 2015
www.atmos-meas-tech-discuss.net/8/C4468/2015/

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

Received and published: 22 December 2015

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

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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.

AMTD

8, C4468–C4471, 2015

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.

Atmos. Meas. Tech. Discuss., 8, C4462–C4465, 2015
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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.

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My comments on this paper will focus on why in the tropics the simulations of cloudy brightness temperatures produce warm biases of up to ~30 K in the ITCZ. The paper briefly speculates that this warm bias could be due to "incorrect ice particle scattering assumptions used in the COSP microwave forward model." They also state on page 11758 lines 7-9 "The precise amount of scattering will depend not only on total ice water path but also on the ice particle shape and size distributions." In my view, the paper does not explore this sentence in sufficient detail. The assumed PSD and shape are important contributions. The reason why the scattering could be wrong is

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not only due to PSD assumptions, and shape, but also the assumed density-size relationship predicted by the adopted model, which being related to shape, is probably the more important consideration at microwave frequencies. If the density-size relationship is in error, then of course the single-scattering properties will also be in error, and these errors can be very significant (see 3rd paragraph below). The importance of the density-size relationship is not discussed and whether or not their choice of relationship is consistent with the most recent observations of cirrus microphysics.

The paper by Geer and Baordo, 2014 is cited, and in that paper, they favour the Liu (2008) sector snowflake model, the single-scattering properties of which were calculated using the DDA code made available by Draine and Flatau (2000). As an aside, in the paper under discussion the DDA code made available by Yurkin et al., (2007) is cited rather than Draine and Flatau (2000). Are the authors absolutely sure that the DDA code of Yurkin was used rather than the former code? This is important as the two codes could produce very different results even for the same crystal model. If the authors have used the Yurkin code, then they should compare the single-scattering solutions obtained from that code to those presented by Liu (2008). Liu (2008) tested his circular cylinder DDA solutions against T-matrix solutions. However, it is unknown as to whether the single-scattering solutions obtained for the snowflake are correct, as it cannot be assumed that the convergence criteria found for circular cylinders will be the same as that for snowflakes using the DDA method. To test the numerical accuracy of the employed electromagnetic method then, in general, the reciprocity theorem should be applied (Schmidt K,Yurkin M A, Kahnert M . A case study on the reciprocity in light scattering computations. Opt Express 2012;20(21):23253–74) as well as comparisons against other electromagnetic methods. To this end, another publicly available electromagnetic code is the boundary element method (BEM), called BEM++, which could, in principle, be applied to snowflakes such as the model of Liu (2008), and the paper describing BEM++ and its application to atmospheric ice can be found at the following link http://www.sciencedirect.com/science/article/pii/S0022407315002769.

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Now back to the density-size relationship. The sector snowflake model of Liu (2008) predicts a density-size relationship of the form ∼Dm-1.566 , where Dm is the maximum dimension of the ice crystal. However, this form of the density-size relationship is not supported by aggregation models and observations, which show that the ice mass of aggregating particles follows ∼Dmˆ2 and so, the density should follow ∼Dmˆ-1.0 (see the following papers, Westbrook CD, Ball RC, Field PR, Heymsfield AJ. 2004. Universality in snowflake aggregation. Geophys. Res. Lett. 31: L15104, DOI:10.1029/2004GL020363, Brown PRA, Francis PN. 1995. Improved measurement of the ice water content in cirrus using a total-water probe. J. Atmos. Oceanic Technol. 12: 410 – 414, Heymsfield AJ, Schmitt C, Bansemer A. 2010. Improved representation of ice-particle masses based on observations in natural clouds. J. Atmos. Sci. 67 : 3303 – 3318, Cotton, R. J., Field, P. R., Ulanowski, Z., Kaye, P. H., Hirst, E., Greenaway, R. S., Crawford, I., Crosier, J. and Dorsey, J. (2013), The effective density of small ice particles obtained from in situ aircraft observations of mid-latitude cirrus. Q.J.R. Meteorol. Soc., 139: 1923–1934. doi: 10.1002/qj.2058).

In the tropics, ice crystals aggregating to maximum dimensions of order 1000s of microns would be expected, and at these sizes the Liu (2008) snowflake model under predicts the density (relative to Cotton et al. 2013 and others) by several factors and consequently, the bulk extinction coefficient of these ice aggregates will also be under estimated. Due to the particles becoming very thin or in the case of hexagons, for example, becoming elongated or tending to large aspect ratios (to conserve observed mass-D relationships), hence their volume extinction coefficients will become small relative to equal Dm hexagons of aspect ratio unity. These elongated or thin ice particles become essentially weakly interacting large particles or "WILPS" for short! As a consequence, WILPS will transmit more upwelling microwave radiation and hence warmer upwelling brightness temperatures will result. Therefore, a further reason for the warm brightness temperature bias in the ITCZ could be due to their model particles becoming WILPS at sizes typically encountered in the tropics. Ice crystal models that follow observed mass-D and density-size relationships are required, if the simulations

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discussed by the authors are to be further improved in the tropics. This also requires PSDs which are representative of the tropics and are sufficiently broad to include the occurrence of very large ice crystals.

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