Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-295-AC2, 2017

Geoscientific Model Development Discussions
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Interactive comment on "A multi-diagnostic approach to cloud evaluation" *by* Keith D. Williams and Alejandro Bodas-Salcedo

Keith D. Williams and Alejandro Bodas-Salcedo

keith.williams@metoffice.gov.uk

Received and published: 30 March 2017

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Author’s response to referee 2 on “A multi-diagnostic approach to cloud evaluation”

K. D. Williams and A. Bodas-Salcedo

March 30, 2017

1 Major comments from referee 2

1.1 Referee Comments

The paper mainly serves as an example of how model evaluation against available satellite and ground-based observations of cloud properties might be performed, including the use of techniques to account for uncertainties or biases in satellite retrievals (using simulators), techniques to isolate specific cloud or dynamical regimes (using compositing), and techniques to isolate the climatological or systematic biases in the model from short-timescale processes (using hindcasts). While this is a useful contribution, the paper leaves much to be desired in terms of physical interpretation, attribution, and discussion of identified biases, and instead focuses primarily on listing the identified biases.

Additionally, there is little (if any) discussion of uncertainties in the observational products used, or of the uncertainties in the comparisons between the model fields and those observations. In light of these shortcomings, I would recommend major revisions to the paper, in particular to dive somewhat deeper into identifying physical processes responsible for the identified biases in the model in terms of the model formulation

1.1.1 Author's response

We have included further detail regarding changes made to the parametrizations between GA6 and GA7 and have provided further evidence for attributing identified changes in cloud errors to particular model improvements. It should also be noted that it is intended that the present paper will be part of a GMD special issue which will also include the GA7 model description paper, so a complete description of the model will be readily available. We wish to avoid including speculation in the paper, however to the extent possible, in the revised manuscript we have attempted to link the errors to known model issues.

Observational uncertainties are complex. They depend on the details of the scene being observed (e.g. cloud size, height), illumination conditions, etc. Therefore, a full description of observational uncertainty is not possible within the scope of this paper. We have opted for bringing in information on observational uncertainty when appropriate within the discussion of the results (see response to specific comments).

1.1.2 Manuscript changes

Section 2a has been considerably expanded with a more thorough description of the relevant parametrization changes. Within section 3, where possible, the text attributing changes in errors to particular parametrization changes has been expanded (e.g. around the warm rain microphysics discussion) to discuss how the parametrization differences lead to the improvement and the physical processes operating (see answers to specific comments). We have also added text to draw together the results from different diagnostic techniques to provide greater process understanding of the errors (e.g. around the mid-latitude cyclone RSW error).

The results of two new simulations have been added to Figure 2 in order to clearly attribute the differences seen to particular parametrization changes.

In the revised manuscript, we provide greater discussion of the uncertainties in the observational products where they are relevant to the paper (e.g. the differences between GOCCP and the CALIPSO cloud retrieval used by RL-GEOPROF). The revised manuscript also includes estimates of significance associated with sampling error to the figures.

2 Specific comments from referee 2

2.1 Comment

140: A definition of low, mid, and high cloud categories should be provided here (i.e., what are the altitude bounds for each category?). A short description of how these histograms are produced would also be useful to the reader here, in addition to providing the reference provided (i.e., cloud occurrence in each category is defined as that which exceeds a minimum backscatter ratio of ??).

2.1.1 Response & manuscript change

Definitions have been added to the manuscript as low:$>$680hPa, mid:440hPa–680hPa, high:$<$440hPa along with a description of the histogram as the referee suggests.

2.2 Comment

153: A brief explanation of the approach for each simulator would be helpful here (i.e., the ISCCP simulator emulates the way the retrieval infers cloud top pressure by estimating brightness temperature...).

2.2.1 Response & manuscript change

A brief description of each simulator has been added as the referee suggests.

2.3 Comment

156-165: The addition of this diagnostic the combines the CALIPSO and CloudSat hydrometeor occurrence is fantastic, but this description and discussion of the implementation is not nearly sufficient. A much more thorough description of the algorithm should be provided. The rationale for the choice of thresholds used seems somewhat incomplete as well, and it would be nice to see the comparison between GOCCP and RL-GEOPROF referred to on line 159. On line 160 it is suggested that the cloud detection algorithms differ between that used in COSP and that in RL-GEOPROF, but the nature of this difference is not explicitly stated and probably should be. Overall, some discussion of the uncertainties and sensitivities to the formulation of this new diagnostic should probably be provided to justify its use in the model evaluation. This could potentially be a significant contribution of this paper.

2.3.1 Response & manuscript change

As the referee requests, this paragraph has been completely re-written and expanded to provide a more detailed description of the diagnostic along with justification of the choices made.

2.4 Comment

212-215: This is a nice result, and it would be worth expanding on the cause for the difference in cirrus between GA6 and GA7. In particular, some justification for the claim that the largest difference is due to the reduction in the rate of cirrus spreading could be shown, such as a figure showing the cirrus amount in GA7 with and without the adjusted cirrus spreading parameterization. I do not think the formulation of the cirrus spreading parameterization, or the changes made to improve the simulation, have been documented well enough in the manuscript. This result showing the decrease in cirrus and better agreement with both CALIPSO and CloudSat is a nice validation of the improvement in the simulation due to these changes, and would go nicely with a more thorough explanation of what is going on here.

2.4.1 Response & manuscript change

Figure 2b now has two additional simulations added to it, one of which is GA6 but with the cirrus spreading rate reduced to the GA7 value to demonstrate the impact as the referee suggests. Discussion of this is expended where the figure is referred to in section 3 and the description of the cirrus spreading change in section 2a has been expanded to include the origin of the parametrization, how it is working and the justification for reducing this parameter.

2.5 Comment

221-222: How do we know that the revised numerics are responsible for the improvement in GA7? What specifically changed in the formulation of the model?

2.5.1 Response & manuscript change

Figure 2b now has two additional simulations added to it, one of which is GA6 but using the 6A convection scheme (revised numerics) to demonstrate that this is responsible for the increase in altitude of the cirrus. The description of the 6A convection scheme has been considerably expanded in section 2a with a list of the changes made to the formulation, however the increase is cirrus height is very much an outcome - it's not clear why these changes have this effect (other than the numerics are more accurate).

2.6 Comment

230: How is the "grid-box cloud fraction" being calculated? I am somewhat confused as to how this is produced alongside the profiles of reflectivity shown in the top panel. Is cloud fraction simply being aggregated onto a coarser grid from the reflectivity, calculated as the fraction within the coarser bins above some reflectivity threshold?

2.6.1 Response & manuscript change

Yes, the combined radar–lidar product has considerably higher along track resolution (nominally 1.7km) than the model (80km at the equator), hence regridding the combined radar-lidar data onto the model grid gives an observed cloud fraction to a precision of about 2%. This has been made clear in the revised manuscript.

2.7 Comment

232-236: What does this imply about the model formulation (the cloud parameterizations)?

2.7.1 Response & manuscript change

The following has been added to the manuscript “This is likely due to too little condensate being detrained at these altitudes, with what there is being either the result of convection going slightly deeper on occasional timesteps or, more likely, some of the condensate being advected vertically having been detrained below.”

2.8 Comment

242: Add a note here that the drizzle rates cited are not shown here.

2.8.1 Response & manuscript change

Added in the revised manuscript as the referee suggests.

2.9 Comment

247-250: This is a nice demonstration of the impact of the new microphysics package, but this is lacking a discussion of the mechanisms for the improvement, and should be accompanied by a description of the changes.

2.9.1 Response & manuscript change

The description of the warm rain microphysics scheme has been expanded in section 2a. We have also added the following in section 3 where the attribution of the change to the warm rain microphysics package is discussed “Within this package, the change to use the Khairoutdinov and Kogan (2000) scheme reduces auto-conversion rates by

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a factor of around 100 compared with the scheme used in GA6. These rates would be too low without the Boutle et al. (2014) GCM upscaling, however even after this correction, the auto-conversion rates remain around 10 times small than GA6 which accounts for the removal of the spurious drizzle."

2.10 Comment

258: Could the increase in cirrus here be explained by excessive advection of the cirrus outflow, or again maybe something to do with the cirrus spreading parameterization referred to earlier? What is responsible for the improvement in GA7?

2.10.1 Response & manuscript change

The improvement in GA7 is due to the cirrus spreading change and this has now been added to the manuscript. The upper tropospheric wind errors are not large enough for the bias to be attributable to excessive advection, hence we retain the suggestion in the text as "possibly due to errors in microphysical processes, or macrophysical fields (such as relative humidity being too high)."

2.11 Comment

261-270: This discussion does not contain much substance, and inclusion of the ISCCP comparison seems to almost be an afterthought. This either needs a more complete treatment of the sources of differences, or consider cutting from the manuscript to make room for some of the more fleshed out analysis, such as the discussion of improvements in thin cirrus.

2.11.1 Response & manuscript change

As we describe in the text, accurate simulation of cloud in this region is believed to be particularly important in determining the global cloud feedback under climate change. For this reason, the excellent simulation of stratocumulus amount is worth showing, however it hasn't changed much between the two configurations shown, hence the brevity of the paragraph. We have expanded the discussion around the ISCCP comparison since it highlights one of the key outstanding errors which remain, namely that in many regions low cloud remains too reflective. We have added “Consistent with this, comparison against a number of observational datasets indicates that the cloud effective radius simulated by the model is too low in many regions, including in subtropical stratocumulus, and is indicative of the aerosol cloud indirect effect being too strong.”

2.12 Comment

278-279: This statement could use evidence or a citation to back it up.

2.12.1 Response & manuscript change

This was based on personal experience. Whilst we believe it correct, we do not have a reference and have therefore removed the statement from the revised manuscript.

2.13 Comment

281-286: This could be better tied in with the discussion of cirrus above. In general though the results from this figure are not very compelling and do not seem to add much to the discussion. It is also not clear to me from Figure 5 that cirrus is overestimated in GA6. The most apparent biases in this figure are the altitude bias in the location of the cirrus maximum in GA6, and an overall underestimation of cirrus in GA7.

2.13.1 Response & manuscript change

The manuscript has been re-worded to link back to the discussion of the tropics as a whole. We now highlight the cirrus height increase and refer to the cirrus amount as a change rather than a universal improvement. This variance in whether the change is an improvement or detrement across the regimes highlights the importance of this figure in providing information over what was in the tropical mean analysis in Figure 2 - a point which has been added to the manuscript.

2.14 Comment

287-290: These conclusions are difficult to draw from Figure 5 as shown due to the scales of the axes used. If boundary layer cloud is the focus of this figure, it would be better to show just the boundary layer for the lower panel (SST composites), and on a cloud fraction scale that allows the reader to actually see the differences between the different curves.

2.14.1 Response & manuscript change

As the referee suggests, the lower panel of figure 5 has been re-drawn to just show the lowest few km and the cloud fraction scale adjusted to make it easier to view the differences.

2.15 Comment

340: I realize this is explained in the cited manuscript, but at least a simple explanation of the equation tested should be given here.

2.15.1 Response & manuscript change

The present manuscript has been revised to indicate that the change made to the equation when testing for anticyclones is identifying a local maxima in surface pressure rather than a local minima.

2.16 Comment

352: "Reasonably good" is awkward language to use here. I would suggest replacing with something like "while the cloud simulation was in reasonable agreement with observations".

2.16.1 Response & manuscript change

Sentence changed in the revised manuscript as reviewer suggests.

2.17 Comment

356: Again "reasonably good" is awkward here.

2.17.1 Response & manuscript change

Sentence changed to “Despite the cloud amount composites showing cloud fraction errors of less than 0.15 (and often less than 0.05) in GA7...”.

2.18 Comment

368-369: Elaborate on how these biases are consistent with the radiation errors.

2.18.1 Response & manuscript change

Description expanded in revised manuscript to highlight that in regions of positive albedo bias in Figure 10, there is a positive RSW bias in Figure 9 and vice-versa. However, the in-cloud albedos in Figure 10 do not depend on the insolation hence for the same cloud albedo error, the RSW error will be larger in the summer than winter.

2.19 Comment

385-389: This is an excellent example of the utility of using multiple observations in the evaluation strategy. This would be a good point to emphasize, and perhaps use as a jumping off point for a more elaborate investigation of the source of these differences (multi-layered cloud vs excess precipitation) than is given in the sentences to follow.

2.19.1 Response & manuscript change

We have highlighted that this is a good example of the utility of using multiple instruments. We have also expanded the discussion explaining why we can’t rule out either the shielded low cloud or precipitation options at this stage (our suspicion is that both may contribute).

2.20 Comment

401: Why is SYNOP data the most reliable here?

2.20.1 Response & manuscript change

Sentence expanded in the revised manuscript to discuss the problems of viewing the lowest levels from space and that an upward pointing ceilometer or human observer is likely to be at their most accurate for low cloud bases.

2.21 Comment

403-405: Need evidence or references to back this up.

2.21.1 Response & manuscript change

Reference to Mittermaier (2012) added.

2.22 Comment

410: How is an okta defined in the context of the model?

2.22.1 Response & manuscript change

This is simply a cloud fraction of 1/8 (0.125). This has been added to the revised manuscript.

2.23 Comment

439: What caused the reduction in the cold bias in GA7?

2.23.1 Response & manuscript change

This was mainly due to the introduction of the 6A convection scheme. This has been added to the manuscript, however it is beyond the scope of this paper to discuss these non-cloud related impacts of the model changes and instead a reference given to Walters et al. (2017) who discuss this further.

2.24 Comment

447-450: I am not sure I entirely agree with these conclusions. The reflected shortwave biases around the subtropical cumulus transitions seem to have reversed in sign between HadGEM2 and GA7, but the magnitudes do not seem to be universally reduced. Perhaps I am looking at the wrong part of the figure though, so maybe a box or symbol on the figure indicating the region where the improvement is evident would be appropriate. The underestimate in reflective shortwave over the Southern Ocean also does not appear to be significantly reduced.

2.24.1 Response & manuscript change

The sentence has be revised to read "The error in the sub-tropical cumulus transition regions of excess RSW has been removed and there is now a smaller negative bias in GA7. The lack of RSW over the Southern Ocean has been reduced by a third and...". We have also reproduced Figure 13 with a revised colour bar to make it easier to quantify the changes e.g. that the negative bias in the transition region in GA7 is smaller in magnitude compared with the positive bias in HadGEM2-A.

2.25 Comment

482-485: This seems to really be a key point of the paper: to demonstrate that the multi-diagnostic approach used reduces the possibility of drawing the wrong conclusions. This is hinted to at points in the paper, but I think this could be drawn together a little better here, perhaps by recounting the points in the preceding analysis that illustrate this (such as the contrast in the comparisons between CloudSat and CALIPSO that demonstrate errors due specifically to thin cirrus, or to excess precipitation as opposed to cloud errors).

2.25.1 Response & manuscript change

The discussion has been expanded here using a number of examples including the ones the referee suggests.