Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-295-RC1, 2017

Geoscientific Model Development Discussions
Open Access
GMDD

Interactive comment

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

Anonymous Referee #1

Received and published: 5 February 2017

This paper uses a range of diagnostic techniques and observational datasets to provide an evaluation of clouds simulated by two configurations of the Met Office Unified Model. The evaluation is multi-time scale with climatological analysis and case studies for different geographical regions. The simulated cloud properties are compared against a range of different remote-sensing (satellite and ground based) data sets. Compositing techniques are also employed to better isolate the cloud properties.

MAJOR COMMENTS:

1. My primary concern is on the scientific focus of this study. The title of the paper seems to suggest that the aim of this work is to introduce "a multi-diagnostic approach to cloud evaluation". However, the paper has spent a lot of time on the inter-comparison of the two configurations of the UM model. I have no problem with whichever topic the

Printer-friendly version

Discussion paper

study is designed to focus on, as both topics have their own values. However, since the study "tries" to cover two topics at a time, the discussions are somewhat lacking in depth. Therefore, the paper reads more like a report.

If the study is designed to focus on introducing a new multi-diagnostic approach, then a thorough introduction of this approach, including the developments of individual diagnostic methods (including necessary technical details), their merits and limitations, their applications in the literature, as well as a quantitative estimate of the uncertainties of these methods, should be fully discussed. The authors have discussed some of the abovementioned aspects, but only to a very limited extent.

If the study is designed to focus on the evaluation of the simulations, then I have real trouble in understanding what have been done in the new configuration. Section 2a provides a general summary of the changes that have been made, but necessary details such as what processes or parameters have been added or changed in the parameterizations are lacking. Also, there is no dedicated case study to investigate the model performance in depth (except a snapshot in Figure 3 and Figure 8). As such, it is very difficult for a reader to appreciate what differences in the simulations can be considered as a real improvement. This is particularly true when considering the presence of new errors in the new configuration for some cloud properties.

More specific comments on this issue are provide below.

2. My second concern is on the comparison of model simulations against satellite observations (e.g. Figure 7, 9, 10, and similarly supplementary Figure 2-4). Many differences are discussed; however, these is no discussion on their statistical significance. How much of the difference is due to sample errors and how much is due to systematic errors in the model? In my view, a significance test should be applied to the analysis to insure that the differences discussed are meaningful. To do this you can use something simple such as a t-test or more appropriately a Monte Carlo method as applied in Booth et al. (2013).

Overall, I recommend that the paper be revised and a major revision is necessary.

SPECIFIC COMMENTS:

Line 62-63: that's fine, but you also have spent a lot of time on inter-comparison of the two configurations of the model.

Line 66: "high", "mid", and "low" clouds need to be defined.

Line 73: please define "NWP".

Line 97-117: a summary of the changes is good, but what changes have actually been made? What processes or parameters have been added or modified in the parameterizations? For example, what has been changed in the auto-conversion scheme (line 101)? What does the change do in the new aerosol scheme (line 112)? What does the turbulent scheme do to the production of liquid water (line 110)? You have provided the references, but some necessary details would be appreciated by the readers and would help justify your argument of the model improvement.

Line 145 and 147: CloudSat and CALIPSO provide a "curtain view" of the clouds, which are not really 3-D.

Line 180: so how many years are used exactly?

Line 194: but you said "3-D" before.

Line 212: "this corrected in GA7" should be "this is corrected in GA7".

Line 219: it does appear to be the case in GA6 to me. Please clarify.

Line 224: in this case I see the model produces a lot of mid-top clouds (which seem to have moderate optical depth) whereas you argue earlier (line 191) that the model simulates too little of this type of cloud?

Line 230: how "cloud fraction" is defined in the simulation and in the observational data set, respectively? Is a direct comparison meaningful? Please clarify.

Printer-friendly version

Discussion paper

Line 238-239: a lot of these “drizzling” clouds in the simulations have a reflectivity below -20 DBZ, which is very, very weak. It seems odd that these clouds are not picked up by the CALIPSO simulator at all.

Line 257-258 and relevant texts throughout the paper: care should be taken when drawing this conclusion. Previous studies (e.g. Chepfer et al. 2013) have shown that, due to the averaging issue, differences in the zonal cloud fraction retrieved in different CALIPSO products can be quite large (up to a factor of 2 for some regions). It is not unlikely that the GOCCP may have underestimated the cirrus extent.

Line 298: this is a fairly big box. While I understand that this is a standard method used in previous studies, I am not convinced that it is appropriate for high-latitude regions, where cyclones (e.g. polar vortex) are generally smaller in size and the distance between individual cyclones can be a lot smaller (compared to mid-latitude cyclones).

Line 331-322: This is a complicated case, with multiple fronts being diagnosed. Therefore it is hard for me to associate the cloud features discussed in the paper to the synoptic components shown on the MSLP chart. Further information such as latitude and longitude on the discussed cloud fields should help.

Line 332-334: I don’t understand this sentence.

Line 364 and relevant text later in the paper: I disagree. What I have seen is that the large RSW bias is present in some of the cold air side of the cyclone, but almost everywhere in the poleward side of the cyclone! Why? This is, to me, an important issue but no discussion has been made in the paper (or the referenced study). There is a lot of focus on the cold air side of the cyclone, but this is only part of the story revealed by the plot. Also, the bias on the cold air side of the cyclone does not explain the poleward increase of the radiative bias shown in Figure 13.

Line 368: I don’t think Figure 10 is necessary. It does not seem to provide any substantially different information than Figure 9.

Line 369-370: why the cloud amount errors are not large enough to contribute significantly to the SW errors? Please explicate.

Line 373-374: again, the errors seem to be prevalent in the poleward side of the cyclone, too (which is also the case in the referenced study).

Line 411: why this appears to be an issue for the UM? Please explicate.

Line425-426: could some of these excess low clouds actually be precipitation not being detected by the instrument?

Line 447-448: but now there seems to be too much red (for RSW) in the sub-tropics which was non-existent in HadGEM2-A?

Figure 2: (1) you use “equivalent reflectivity factor” in the plot but “reflectivity” in the caption (and the following figures). (2) What do the colour bars show? It should be indicated in the plots. (3) The lowest km should be masked in the Cloudsat plot (as done in your following figures).

Figure 3: What does “CloudSat/CALIPSO” mean in the top left plot while you only show reflectivity?

Figure 5: I don’t think including the very cold SST ranges is necessary as they are quite rare and the plots show very similar features (i.e. the first four plots in the bottom panel).

Figure 8: is 64.32°N (the right end of the cross-session plots) over land? I can see the topography-like feature at the surface in the bottom left plot, but why there are clouds produced underneath the surface in the simulations?

REFERENCES:

Booth, J. F., C. M. Naud, A. D. Del Genio, 2013: Diagnosing warm frontal cloud formation in a GCM: A novel approach using conditional subsetting. Journal of Climate, 26, 5827-5845.

Printer-friendly version

Discussion paper

Chepfer, H., G. Cesana, D. Winker, B. Getzewich, M. Vaughan, and Z. Liu, 2013: Comparison of Two Different Cloud Climatologies Derived from CALIOP-Attenuated Backscattered Measurements (Level 1): The CALIPSO-ST and the CALIPSO-GOCCP. JTECH, 30, 725-744.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-295, 2017.

Printer-friendly version

Discussion paper