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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Improving aerosol activation in the double-moment Unified Model with CLARIFY measurements" *by* Hamish Gordon et al.

Anonymous Referee #2

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This study introduced the first configuration of the UK Met Office Unified Model in which both cloud and aerosol particles have 'double-moment' representations and evaluated the model at two different resolutions – 7 km and 0.5 km. The authors also made changes to aerosol activation to improve the representation of this process. The changes in calculating activation based on existing drops and in accounting for the effect of unresolved vertical velocities are trivial, in my opinion. The work is valuable for the UK Met Office Unified Model, and testing climate physics parameterizations at high-resolution (CRM) is also a meaningful try. But I did not see good model results The detailed model evaluation with aircraft measurements is also valuable. The current version needs major revisions to be accepted as a publication in ACP. Here are

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the major concerns.

(1) The writing of the manuscript is sloppy. Many statements are not in the format of scientific writing. Also, there are many confusing statements and confusing use of the terms. Simulations are not clearly described. There are inconsistent and undefined names used in in figures legends, contradicted statements, lack of clear description of data sampling/processing approach to compare with the model simulation. A lot of my detailed comments below are about these problems. I do not think I captured all of the problems. In my opinion, the manuscript was in some draft mode and not ready for submission.

(2) The organization of manuscript needs improvement. For example, Sections 6 and 7 are purely the description of model developments and they are quite long. They should be moved to the Section 3 or 4 to have model developments described together.

(3) It is an overstatement that the effect of subgrid vertical velocities on activation is accounted. Basically what the authors did was to lower the threshold of grid mean updraft speeds used for activation. The statement makes people think that they connect the activation with the subgrid vertical velocity spectrum calculated from turbulence to get this done. Using a grid mean value of updraft speed, does not justify to say “subgrid vertical velocity”. This has to be clarified throughout the paper. Otherwise it would be misleading.

(4) The model configuration and model simulations are not clearly described. Suggest to use a table to clearly show the configurations of major simulations. Some confusions arise from the misuse of abbreviations, for example, in some places, UKCA and CASIM are used for microphysics schemes, while they also used for different aerosol activation schemes. In the first sentence of Section 9, one of them is referred to as an aerosol scheme?

(5) I do not think 500m resolution is fine enough to simulate stratocumulus clouds explicitly. The turbulence is very difficult to simulate at this resolution because it is

partially resolved and a portion of it needs to be parameterized but model is difficult to know how much. This might be a reason for the poor simulation of PBL height. I would suggest run a test with resolution smaller than 250 m with a smaller domain to see how the simulated cloud and updraft can be improved, particularly the inversion height. Currently, the simulations that authors presented did not do well in simulating the clouds and had a huge problem with the Aitken mode of aerosols as well. This might not justify an acceptance of the paper due to these problems. If you do not want to further look at the aerosol problem, at least try to provide a good simulation of the clouds or find the major reasons leading to the large model-observation discrepancy.

Specific comments: 1. Need to describe explicitly the model configuration/resolution instead of using something like RA1, GA7.1 .

2. P1 Line 18-21, text is contradicted with the text on p9 Line 12. If you are testing the aerosol and chemistry component of the model at a higher resolution than has been attempted before, why do you need to specific a kappa value for activation? Then at P9 Line 25-27, it is said volume-weighted hygroscopicity is passed to CASIM for activation. Very confusing.

3. P8-9, it is not clear why ARG is implemented differently between the 7 km and 500 m resolutions.

4. P.9 Line 5-10, First, is ARG applicable to 500-m resolution since it was developed based on cloud parcel model with timesteps for global climate models? Figure 3 showed that cloud droplet numbers from 500 m resolution are worse compared with 7-km resolution, indicating the scheme might not work well for very high-resolution. Second, based on your description here about accounting for subgrid velocity effect on activation, you are not using any subgrid velocity. If you only use a grid mean value of updraft speed, which means you are only account for the impact of resolved updraft, not subgrid vertical velocity. This has to be clarified throughout the manuscript because the writing gave me impression that a particular method is employed to account for the

subgrid velocity effect on activation in this study until I read here.

5. P9 Line 34-35, it is confusing to say “CASIM microphysics code has the capability to simulate aerosol microphysical process” just because they simulate in-cloud removal and aerosol resuspension. Those processes are called aerosol-cloud interaction processes, not aerosol microphysical processes (instead they are cloud microphysical processes).

6. Figure 3 showed that the model did not capture the observed cloud well. At least try to figure out potential reasons for the large discrepancy between the model and observation.

7. P10 Line 4-7, I am confused. Here you said In the UKCA code, aerosol resuspension is not accounted, but I think UKCA is said as just an implementation method for ARG activation scheme. If it is a different microphysics scheme, why you need a different microphysics scheme from CASIM? Also, didn’t you use coupled GLOMAP-CASIM for this study? Why “a coupled GLOMAP-CASIM double-moment model that includes aerosol microphysical and chemical processing is deferred to future work”?

8. Suggest use a table to clearly describe the configurations of aerosol and microphysical processes of the simulations for each domain.

9. Figure 4, What is ACC and AIT? They are not defined. From Figure 4, the model did not capture the inversion well. Is it the problem of resolution?

10. P12 Line 5-7, confusing. What boundary layer height are you talking about?

11. Sections 5.3 and 5.4, How did you sample the data from the simulations to compare with the aircraft observation? Need some description either in the figure captions or in the text.

12. Section 5.5, should we expect 500m resolution is fine enough to simulate stratocumulus clouds? See my major comments #4.

13. P15 Line 9-11, do you still have cloud fraction for 500-m resolution? If so, how is the cloud fraction determined and is the way to determine cloud fraction is the same between the 7 and 0.5 km resolution?

14. Figure 8, which simulation does “Model” denote? What are UKCA and UKCA 7 here? I could not find definition of these terms anywhere. In the Table 1, there are simulation names of CASIM, UKCA 500m, UKCA 7km, UKCA global. None of them are consistent with the names used in Figure 8.

15. The poor simulations of cloud droplet distribution might indicate that ARG scheme might not be applicable to those resolutions, as I commented above.

16. Figure 9, which simulation did you compare here? Such information should be clearly described in the figure caption.

17. P20, 11-14, which activation scheme are you talking about here? There are two activation schemes - UKCA and CASIM as discussed above.

18. The organization of Section 6-8 is strange since Sections 6 and 7 are purely the description of model developments. They should be moved to the Section 3 or 4 to have model developments described together.

19. Section 9, in-cloud activation is thought as secondary nucleation, which should increase droplet number. Here it decreases CDNC. I think here it means different from what people usually think. It is just a way to treat activation with accounting for existing droplets? Please clarify. Is it added to the primary nucleation (cloud-base nucleation)? If so, how can it reduce CDNC?

20. Section9, first sentence: it is a confusing sentence. Which one is two-moment aerosol scheme and which is the two-moment cloud microphysics? Based on what I read, UKCA and CASIM were also used for naming different activation schemes.

21. P26, Line 15-16, I do not think it is scientific writing by saying “model emits smoke. . .”

22. P28, Line 1, First I do not agree that the model performs well based in the results shown. Second, even if it indeed performs well, please state perform well in what quantity and what aspect. It is not a scientific writing to only state “the model performs well”.

23. P28, 7, I have no idea what you mean about “cloud deck is just under 200km across”.

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