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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 #1

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Review of: "Improving aerosol activation in the double-moment Unified Model with CLARIFY measurements"

Authors: Gordon et al. General comment:

The authors present results for a new 2-moment microphysics scheme for activation of aerosols and nucleation of cloud droplets in the UK Met Office Unified Model. They demonstrate marginal improvements via validation of simulated marine stratocumulus clouds over the previous 1-moment scheme that provided ok results for the wrong reasons due to compensating precipitation processes. The presentation and analysis is sufficient, but I find it difficult to accept some of the justifications, simplifications,

and assumptions made in the schemes being discussed. More specific details about this statement are below. Perhaps the authors can address my comments and shed additional light on the motivation for the efforts described herein.

Specific comments:

1.Page 3, Line 27 down to Page 4, Section 2: It's unclear why you would use a 2-moment scheme with aerosols and then use a saturation adjustment for supersaturation and condensation. In an aerosol-limited regime, like typical marine stratocu, what happens if you consume all the aerosols but still have vertical motion and generation of supersaturation? If you are not permitting supersaturation to be carried around, where does the excess vapor go? To the growth of existing droplets? Will those just grow indefinitely via saturation adjustment? It seems like this could truly cause a problem with reliable prediction of latent heat release and droplet growth. I think more explanation or justification is needed beyond a reference to Morrison and Gettleman (2008).

2.Page 6, Line 33: Why do the fine grid regional domains go all the way up to 40km? Seems like you could reduce the number of vertical levels substantially and improve runtime if you topped things out in the lower stratosphere, especially since you're not simulating deep clouds.

3.Page 11, Lines 8-9: Could these high peaks be resulting from saturation adjustment? If you suddenly force new droplets to form and/or vapor growth of existing droplets in order to use up all saturation in one time-step, perhaps this is shocking the system and creating a sudden spike in drops and perhaps also a spike in latent heat release, buoyancy, and W. This is one particular reason to move away from saturation adjustment schemes.

4.Page 12, Lines 9-10: Perhaps the under-predicted RH is causing less simulated cloud fraction shown in figure 3. Maybe you're just not getting enough of an area that can generate clouds due to lack of moisture. Any thoughts?

5.Page 15, Lines 6-7: I would suspect you're not getting enough simulated convection. Your simulated cloud area is a lot less than the satellite viewed cloud cover. Perhaps this again goes back to the simulation being too dry? Your vertical velocity distributions in Figure 7 show that the simulations produce FAR fewer strong updrafts with very few instances of simulated W > 2 m/s. This would also help explain why your cloud cover is much less than observed. This could certainly be a model resolution issue and perhaps these simulations should have been run with DX < 200m in order to get better resolved updrafts.

6.Page 15, Lines 14-15: This again looks like it could be an issue with saturation adjustment in combination with activating aerosols with such a scheme.

7.Page 18, Line 3: The underestimation of large drops could be due to the required fit to a gamma distribution. It could also have something to do with rain drop breakup being too aggressive when large oblate drops attempt to form via collisions.

8.Page 21, Lines 23-24: In light of the comment regarding Phillips et al. (2007), I have to wonder why efforts are being made to improve a scheme with admittedly known limitations rather than adapting the model to something that avoids saturation adjustment and permits prognostic supersaturations. Many models have already moved in this direction.

9.Page 23, Lines 3-4: Here you state that the "ratcheting" effect should cause the 2-moment scheme to produce more droplets than the 1-moment scheme. Many times, 1-moment schemes hold the number concentration fixed, so your statement isn't really broadly true.

10.Page 25, Lines 23-25: If the PDFs are different, this is likely due to your choice of gamma distribution shape parameter. You would get a different solution if you change the breadth of the distribution.

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