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Interactive comment on "Evaluating the effects of microphysical complexity in idealised simulations of trade wind cumulus using the Factorial Method" *by* C. Dearden et al.

Anonymous Referee #2

Received and published: 6 December 2010

General comments.

This manuscript presents results of a study looking at the impact of different microphysics schemes of varying complexity in simulating trade wind cumulus in an idealized 1D framework. The authors use the Factorial Method to isolate and examine different factors in explaining the sensitivity of precipitation to aerosols. Overall, they find that the 2-moment scheme produces results closest to the bin scheme for low updraft speeds, but all schemes produce similar results for higher updraft speeds. I found the paper to be interesting and well-written. Overall, I recommend minor revisions before publication. These issues mostly center around improved clarification of the presentation,

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including the descriptions of the microphysical schemes and the Factorial Method. I also feel that more discussion is warranted on a few other topics, including realism of the 1D driver model setup, treatment of sedimentation, and computational cost of the various schemes. Specific major points are given below, followed by several additional minor comments.

1. I believe that more description is needed for two important components of this study:

1) I think the description of the microphysics schemes was rather confusing in terms of some key points and needs to be clarified and improved. Specifically, I think more description of the droplet activation scheme used in 2-m is needed. Is droplet activation only allowed at cloud base, or throughout the cloud? Is in-cloud activation allowed based on diagnosed supersaturation? Does this treatment differ between the Twomey and A-R activation schemes? It is important that these points are clarified, since they are referred to later in the paper when discussing the results. For example, on p. 23516 it is stated that "This problem is alleviated in the 2-m A-R scheme due to the ability to diagnose the in-cloud supersaturation,..." which leads one to believe that in-cloud supersaturation was allowed with the A-R approach but not the Twomey approach. A similar statement is made by the authors on p. 23514, lines 20-23. However, this is not actually described anywhere in the scheme descriptions. Also note that diagnosing in-cloud supersaturation and allowing in-cloud activation is not a feature of the A-R approach per se (i.e., as described in Abdul-Razzak and Ghan 1998), as implied by the authors here, but rather a feature of the Morrison scheme; the in-cloud activation parameterization in this scheme could have been used with the Twomey approach or any other diagnostic treatment of droplet activation.

I also think a key point in interpreting the model results is the fact that in the 1-m scheme, droplet concentration is assumed equal the CCN/aerosol concentration specified in 2-M for the A-R approach, and the CCN concentration at 1% supersaturation for the Twomey approach (but which is generally not all activated because of the relatively low vertical velocities and hence supersataurations < 1%). Thus, by design the pre-

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dicted droplet concentration in 2-M for both A-R and Twomey is lower than the specified droplet concentration in 1-m, which likely explains the tendency for higher precipitation in 1-m. This is not an intrinsic feature of 1-m versus the 2-m scheme as implied by the authors, but rather a result of the experimental set up and in particular the choice of droplet concentration specified in 1-m (for example, droplet concentration in 1-m could have been set to a lower value more representative of the predicted values in 2-m). This issue needs to be clarified and explained in detail in the text, and related back to the main points in terms of the impact on precipitation. This is especially important since a conclusion of the paper, as stated on p. 23517, is the tendency for more complex schemes to ptoduce higher precipitation rates.

2) I found the description of the Factorial Method to be rather confusing. Section 3.2.1 on the general example of the 2^3 design and the related Table 1 seems rather unnecessary, and how it applies to the actual method employed here is not clear. For example, the labeling system using lower case letters (described on p. 23509) is described in this section and in Table 1, but then not used again in the text. Furthermore, in the general example a total of eight simulations are described (using low and high values for each factor), but then the actual approach employed in this study used either 36 (bin) or 135 (bulk) simulations because varying degrees of each factor were used, rather than just "low" and "high". Also, on p. 23513 it is stated that "Calculation of the relative contributions for each factor and their interactions follows the methodology explained in Sect. 3.2", however, a description of how the relative contributions are calculated is not actually given in this section or anywhere else in the paper for that matter. Thus, I would suggest removing the general example in section 3.2.1 and Table 1, and instead give a brief description of how the relative contributions of the various factors are calculated. The authors can refer to Dearden (2009) and references therein for a more detailed description of the method for this calculation, but at least a brief description is needed here.

2. I am a bit concerned about the impact of different numerical treatments of sedimen-

tation in the bulk and bin schemes leading to differences in the results. It is stated on p. 23506 that for the bin scheme, sedimentation is calculated using the 4th-order Bott scheme, while for the bulk scheme it is handled "implicitly" in the bulk microphysics scheme. In the bulk Morrison scheme sedimentation is actually calculated by an explicit first-order upwind method, so I think the term "implicitly" as used by the authors is misleading. More importantly, using a 4th order versus first order upwind scheme could produce potentially important differences in results. Furthermore, if the sedimentation in the bin scheme is combined with vertical advection due to air motion in a single calculation (i.e., if particle fallspeed is combined with vertical air motion to derive a single advective velocity used to calculate vertical transport), this can have a large impact versus using a time split approach where transport is first calculated due to air motion, and subsequently calculated due to sedimentation (or vice versa). Given that precipitation and fallout of rain appears to be an important difference between the bin and bulk model simulations, this issue should be further explored. Ideally, the authors should test the impact of using the different numerical treatments to unambiguously address this point, although I realize it may be substantial work to recode the sedimentation for the bulk and/or bin schemes.

3. A key goal of this study is to evaluate schemes of varying complexity to determine how well simpler schemes are able to reproduce results of the benchmark bin scheme. The motivation is that more complex schemes, bin schemes in particular, are computationally costly. This I'd like to see a bit more discussion about the computational cost of the various schemes. In my experience, most of the increase in cost associated with increasingly complex schemes is due to the larger number of prognostic variables, and specifically to the advection/diffusion of these variables. Therefore it's not clear that going from the 2-m Twomey to 2-m A-R scheme would result in much of an increase in computational cost, while going from 1-m to 2-m would increase cost due to the extra variable for droplet concentration in 2-m, and going from 2-m to the bin scheme would result in a much more substantial increase in cost. It might be possible to locally diagnose locally whether the 1-m or 2-m approach is used based on local conditions

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(updraft velocity), as suggested by the authors on p. 23517, but the key to minimizing the cost is to reduce the number of variables that are advected/diffused, and advection and diffusion are by definition non-local processes. I also think it should be possible to develop an intermediate scheme between 1-m and 2-m, where the droplet concentration is (locally or nonlocally) diagnosed from CCN, updraft velocity, etc. and allowed to vary in time and space (such an approach has been used for GCM microphysics parameterizations in the past). This would allow for representation of droplet activation and coupling with aerosols, but reduce the cost by limiting the number of prognostic variables since the droplet concentration would be purely diagnostic. Overall, discussion of these issues as they pertain to the issue of scheme complexity versus cost is warranted.

4. The authors do acknowledge the importance of microphysical-dynamical feedbacks and entrainment, for example, in the last paragraph on p. 23518. However, I'd like to see this emphasized a bit more in the paper, perhaps by expanding this discussion, citing relevant work on this topic, and bringing this up earlier in the paper.

Additional comments.

1. p. 23499, last 2 lines. I would argue that mesoscale models with "convection-permitting" resolutions (as they have traditionally been defined in the literature) are not adequate to resolve warm shallow convective clouds; LES with grid spacings of order 100 m or less are needed.

2. p. 23501, last 2 lines. While ACPIM allows for a prognostic treatment of aerosol, was prognostic aerosol used for the simulations here? This is not explained but should be clarified. If it was, how might this impact results compared to the bulk schemes which did not employ prognostic aerosols?

3. p. 23503, second paragraph. I don't believe that the justification for using saturation adjustment in bulk schemes is that it is "valid" for timesteps longer than 1 sec, as the authors imply here. I think a better justification for using saturation adjustment is that

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the mass of vapor above saturation is typically much smaller than the mass of cloud water. For example, one could use very small time step (i.e., « 1 sec), and the cloud water mass is still not likely to be much different using saturation adjustment versus an explicit calculation of condensation/supersaturation (as in the bin scheme), except right near cloud base where the mass of vapor above supersaturation is large relative to the cloud water mass.

4. p. 23504, In Eq. (1), N(D) is actually the differential number concentration. However, in equation (2) and the statement following (2), N is the total number concentration (i.e., Eq. (1) integrated over dD). The authors should distinguish between the differential concentration and the total (i.e., integrated) number concentration.

5. p. 23505. The bulk scheme does not use prognostic aerosol, but again, does the bin scheme employ prognostic aerosol? See minor comment #2 above.

6. p. 23504-23505. Is activation in the 2-m Twomey and A-R schemes only at cloud base, or is in-cloud activation allowed? If so, how is it calculated (neither Twomey nor A-R deal with in-cloud activation per se). See major comment # 1.1 above.

7. p. 23505, line 24. "…can be compared safely." I think "safely" seems like an odd word choice here. I might suggest something like "rigorously" instead.

8. p. 23506. The potential temperature and pressure fields are held fixed so that "the pure microphysical behavior of each scheme could be compared fairly, in the absence of feedbacks". However, with the way the text is worded it seems like the vapor field is allowed to be influenced by microphysics (i.e., through condensation/evaporation). If so, this should be clarified. Furthermore, if the vapor field is allowed to be modified by the microphysics this would seem inconsistent with not allowing latent heating to influence the temperature.

9. p. 23507. It might be helpful to the reader to briefly state the reason that liquid water content decreases with height using the warm 1 case.

10. p. 23507. It would be helpful to clarify that the modified sounding of Shipway and Hill (2010) was not used by the authors for the present study. I think the confusion I had over this is because when discussing the modification of this profile by Shipway and Hill, the authors use present instead of past tense ("is modified", "which leads to..", and "This produces reduced...") when discussing the Shipway and Hill paper.

11. p. 23508, lines 3-4. "The result is such that changes in the dynamics do not affect the cloud height or depth." In the literature, "dynamics" usually refers to the wind field, so I would suggest using "thermodynamics" here instead of "dynamics" to refer to changes induced by modification of temperature (and thus pressure).

12. p. 23508, line 7. I suggest using "grid spacing" instead of "resolution".

13. p. 23510. The closeness of the liquid water path curves for the bulk and bin model simulations without precipitation or sedimentation also confirms the validity of the saturation adjustment approach in the bulk scheme, as opposed to explicit calculation of condensation/evaporation in the bin model.

14. p. 23511, lines 18-19. The shape parameter also directly impacts evaporation rates, so I would remove the word "hence" from this statement.

15. p. 23512. It seems a bit counterintuitive that higher updraft velocity leads to reduced precipitation (one might generally expect the opposite result), but I suspect that this occurs because the timescale over which the updraft is applied changes with updraft strength (as shown in Fig. 1), such that the maximum extent the column is lifted vertically is the same for the tests with varying updraft strength. This should be clarified.

16. p. 23512. "...not all droplets are necessarily activated for a given updraught." I think the authors mean that not all aerosols or CCN are activated as cloud droplets for a given updraught.

17. p. 23512. The total precipitation converges between 1-m and 2-m with increasing

updraft strength, which is presumably due to a convergence of droplet number concentration. This is implicit in the discussion in this paragraph, but it might be helpful to explicitly state this point.

18. p. 23512. The tendency to produce precipitation at the surface earlier in the bulk schemes than the bin model is interesting. Does this appear to be due more to earlier rain formation aloft, or faster sedimentation?

19. p. 23513. The tendency to over or underestimate precipitation in 1-m will be dependent on the droplet concentration that is specified (see major comment #1.1 above), which could also account for differences between the results presented here and those of Shipway and Hill (2010).

20. p. 23513. I understand that nonlinear interaction between the different factors is accounted for in the factorial analysis presented here. However, I noticed that the "interaction" term shown in Figs. 7-9 is always zero or positive. Presumably the different factors could interact in a way that would reduce the influence of each individual factor, in which case the "interaction" term should be negative. If it occurs, how is this situation handled by the Factorial Method as it is applied here?

21. p. 23514-23515. The discussion of the relationship between sedimentation and evaporation of rain is a bit confusing. The bin model produces more rain mass aloft, larger drops, less evaporation, and more precipitation, relative to the bulk models. However, I'm not sure if larger drops are necessarily consistent having a greater rain mass aloft - in my experience the opposite is often true, as a reduction in drop size and hence fallspeed can lead to accumulation of rain mass aloft. Further, greater rain mass aloft in the bin model simulation compared to the bulk model would tend to lead to larger not smaller evaporation rates, but apparently the larger drop size in the bin simulation (and perhaps other factors as well) is enough to compensate for the larger rain mass, thereby leading to a reduced evaporation rate.

22. p. 23515. How does the formulation for rain fallspeed differ between the bulk and

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the bin schemes, and does the modification of the "b" fallspeed parameter bring the fallspeed formulation in the bulk scheme closer to that in the bin scheme?

23. p. 23516. I find it a bit odd that the 2ˆ2 system is defined as having three degrees of freedom, with the third degree of freedom arising from the sum of the nonlinear effects of the two main factors. In my view, the nonlinear interaction between system components is typically not defined as leading to an additional degree of freedom.

24. p. 23517. "... a positive correlation was found between microphysical complexity and the amount of precipitation produced." I don't think the word "correlation" is appropriate here, as this implies some statistical significance when in actuality there are just a few schemes tested. I would say instead something like "an increase in microphysical complexity tended to lead to an increase in the amount of precipitation at the surface." Furthermore, some of this trend may be simply due to how the droplet concentration is specified in 1-m compared to the value predicted in 2-m, see major comment #1.1 above.

25. p. 23518. I agree with the authors' statements concerning the importance of testing different bin schemes if they are to be used as benchmarks for testing bulk schemes. I might also add the importance of constraining bin schemes with field observations and lab experiments.

26. p. 23520. The Milbrandt and McTaggart-Cowan sedimentation study has been written up in a paper that is currently in press in JAS and is available in early on-line release. I would suggest using this reference instead of the conference paper.

27. p. 23526, Table 5. How is integrated rain evaporation defined in this table? Is this a vertically integrated quantity? Furthermore, based on the units given in the table caption (kg m-2), it is not expressed as a rate. Thus, either the units are incorrect or this is a bulk evaporation rate integrated over some time period.

28. p. 23532, Fig. 6 and other figures. I realize this is mostly a production/layout issue,

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but the figures were very small and difficult to read.

Technical comments.

1. p. 23498, abstract. "...warm idealised clouds are explored using..." should be "...warm idealised cloud is explored using..."

2. p. 23520. The Morrison et al. (2005) reference cited in the paper is missing in the references.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23497, 2010.

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