Atmos. Chem. Phys. Discuss., 10, C12058–C12066, 2011
www.atmos-chem-phys-discuss.net/10/C12058/2011/

Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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.

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Received and published: 6 January 2011

On behalf of all the authors, the first author would like to acknowledge the careful attention afforded by the reviewer to all aspects of the manuscript and to thank them for the detailed comments which serve to strengthen the paper.

Response to general comments

1.i) On the issue of the treatment of droplet activation in the 2-m bulk schemes: The first author concedes that clarification is necessary on this point. The authors propose adding the following paragraph to the end of section 2.2.3 in the manuscript:

"The only difference between the 2-m schemes is in terms of the droplet activation scheme used. For cloud base activation, droplet number in the A-R scheme is determined as a function of both the updraft speed and the aerosol properties. This is different from the Twomey approach, which determines cloud base droplet number based on the updraft speed and the value of the chosen c and k parameters. In both the Twomey and A-R cases, in-cloud activation is also permitted, and is based on a diagnostic calculation of the in-cloud equilibrium supersaturation within the current timestep. This diagnostic relation is a feature of the Morrison scheme, and should a supersaturation be diagnosed, in-cloud activation is allowed to occur via the specified activation scheme."

There is a switch in the version of the Morrison code used, called IBASE, which, when set to a value of 1, specifies the above behaviour. This setting was used for all the results shown in the manuscript for the 2-m schemes. With regards the following statement on page 23516 of the manuscript: "This problem is alleviated in the 2-m A-R scheme due to the ability to diagnose the in-cloud supersaturation..." The authors suggest replacing this sentence with the following: "This problem is alleviated in the 2-m A-R scheme, where knowledge of the aerosol composition and the log-normal size distribution is advantageous in obtaining a better agreement with the bin scheme."

The reviewer is also correct that the design for the 1-m scheme is such that the assumed number of droplets is equal to the assumed CCN concentration, with the implicit assumption that all CCN are activated independently of the updraft speed in question. By design this does produce differences in droplet number compared to the 2-m schemes. To clarify this, the following sentences have been inserted into the revised manuscript:

Page 23504, added the following sentence at line 14: "The experimental set up of the 1-m scheme is such that the assumed droplet number concentration is taken to be equal to the CCN concentration, with the implicit assumption that all available CCN activate to form cloud droplets."

Page 23504, added the following after the sentence that ends on line 17: "The consequence of this is that in the 2-m schemes, not all available CCN are necessarily activated for a given updraft speed, which may lead to lower droplet number concentrations when compared to the 1-m scheme."

Page 23511: Removed sentence on line 26, stating that "the general pattern is for the total precipitation to increase as a function of increasing microphysical complexity..."

Page 23512, line 1: Sentence modified so that it now reads "With regards the 1-m scheme, the assumption of a fixed droplet number means that the total precipitation is essentially insensitive to vertical velocity..."

Page 23514 at line 3; Paragraph modified as follows: "However the schemes disagree on the extent of the relative importance of the CCN and temperature effects. It can be seen in Fig. 7 that the contribution of the CCN effect is largest in the 1-m scheme; this is a consequence of the experimental set up for the 1-m scheme where an assumption is made that the change in droplet number is equal to the change in CCN concentration, independent of the change in vertical velocity. For the 2-m bulk schemes, the contribution of the CCN effect is slightly reduced; this can be explained as follows. For slowly increasing updraught speeds such as the 0.5m/s case, the ability to predict droplet number results in competition for water vapour between growth of existing droplets and activation of new droplets; this is demonstrated by Dearden (2009) using a simple lagrangian parcel model. The presence of CCN that activate at relatively low updraught speeds act as a sink of water vapour through growth by condensation, resulting in fewer droplets activated overall compared to the 1-m scheme where by design the droplet number concentrations are assumed to be slightly higher. This explains why the 1-m scheme produces the largest sensitivity to CCN according to Fig. 7."

Page 23516: Added the following sentence at line 1: "In theory the sensitivity of the 1-m scheme to vertical velocity could be increased by using a diagnostic relationship

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for droplet number concentration instead of the assumption of a fixed value as used in this study.”

ii) On the issue of the Factorial Method description: The example relating to the 2ˆ3 design was included since 3 factors are considered in the manuscript, and so this was considered to be a relevant example. Although a lot more than 8 runs are performed with the bulk and bin schemes, the overall experimental design is still based on the 2ˆ3 example. The reason for the additional runs is to allow different combinations of ’low’ and ’high’ values to be considered. For example, in relation to the w factor (vertical velocity), the effect of going from the ’low’ value of 0.5m/s to the ’high’ value of 2m/s is considered in figure 7, while figure 8 shows the effect of increasing w based on a different ’low’ value (2m/s) up to a new ’high’ value (4m/s). Thus in theory the effect of any combination of low and high values can be explored from the available range of simulations. The results in section 4.4 are from a 2ˆ2 design because in this section changes in the temperature factor were not considered. The authors propose adding the following sentences to the end of section 3.2.1 to briefly discuss how the relative contributions are calculated.

“Once the main effects and interaction terms have been calculated, the relative contribution of each effect or interaction to the total variance can also be quantified in terms of a percentage of the total sum of squares. The calculation of the sum of squares for a given effect or interaction term is specified in Dearden (2009).”

2.On the issue of differences in the treatment of sedimentation between the bin scheme and the bulk schemes: The revised manuscript will clarify the treatment of sedimentation in the bulk scheme to make it clear that this is handled with a 1st order upwind scheme. Lines 11-13 of page 23506 have been modified thus:

“The ACPIM driver model is configured to combine sedimentation with vertical advection due to air motion in a single calculation, which is handled using the 4th order Bott scheme. In KiD, sedimentation of cloud liquid water and rain is handled within the

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Morrison microphysics scheme itself using a time-split 1st order upwind method."

Although it is non-trivial to re-code the sedimentation in the bin and/or bulk schemes to make them both fully consistent with each other, the authors will attempt to perform some additional tests with the bulk scheme whereby the existing 1st order upwind method is circumvented and replaced with the Bott 4th order scheme. Given the diffusive nature of the 1st order upwind method, it is likely that the diffusion helps to reduce the effect of excessive size-sorting in the bulk schemes, and that a 4th order scheme for sedimentation would make the size-sorting issue more apparent.

3.The discussion on computational cost has been extended in the revised manuscript, based on the findings from repeated 1-m runs where the assumed droplet number concentration was adjusted to match those predicted by the 2-m scheme. The following additions to the manuscript have been made:

Page 23514, line 23: "Some additional simulations with the 1-m scheme were also performed where the assumed droplet concentration at low updraft speeds is set to a value more representative of the predicted values from the 2-m A-R scheme. The results from these tests reveal that tuning the droplet number concentration in the 1-m scheme allows the total precipitation values to converge on those produced by the 2-m A-R scheme, whilst also improving the agreement in terms of the relative contributions. This result suggests that a diagnostic representation of droplet number based on CCN number and updraught velocity would be sufficient to capture aerosol indirect effects for the chosen scenario. This has important implications when considering the balance between model complexity and computational efficiency, as it shows that, in the absence of feedbacks at least, a prognostic variable for droplet number may not be necessary, and that a diagnostic treatment of droplet number would help to minimise the cost of the scheme without compromising the ability of the model to capture the effects of aerosol."

Page 23517, added the following sentence at the end of line 18: "It should be noted

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that it was also possible to tune the 1-m scheme at low updraft speeds to produce essentially the same precipitation amounts and sensitivities as the 2-m scheme, by fixing the assumed droplet number concentration to match those of the predicted concentrations. This suggests that in the absence of feedbacks, a diagnostic relationship for droplet number may perform just as well as a prognostic treatment. This has obvious advantages in terms of computational efficiency given that one less prognostic variable would need to be advected."

Pages 23517-18 (and abstract): The sentences referring to an increase in precipitation with increased microphysical complexity have been removed, as this statement is no longer strictly true when the 1-m runs are repeated with a lower droplet number concentration.

4.The comment regarding feedbacks between microphysics and dynamics has already been highlighted by reviewer 1; please see the author response to general comments of 1st reviewer.

Additional comments

Those additional comments that require a response from the authors are now addressed on a point-by-point basis. The absence of a response to a particular point can be taken to mean that the authors are happy with the suggestions made by the reviewer, and that the appropriate changes will be made in the revised manuscript.

1) Page 23499, added the following sentence at the last line: "In order to adequately resolve and therefore study the effects of aerosol on warm shallow convection, cloud resolving simulations with resolutions of 100m or less are really needed such that the dynamics of the clouds (i.e. updraught strength) are sufficiently captured."

2) Page 23501, last 2 lines: A prognostic treatment of aerosol was used in the ACPIM simulations presented in the manuscript. ACPIM holds prognostic aerosol and mass at each gridpoint. The advantage of doing so may result in a more realistic represen-

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tation of in-cloud activation, since if the total number of aerosol at a given gridpoint is reduced due to activation, a higher supersaturation would be required in subsequent timesteps for the remaining smaller interstitial aerosol to activate. The bulk schemes, even with the diagnosed in-cloud supersaturation, do not possess knowledge of changes in aerosol size, and so the 2-m bulk schemes may overestimate droplet number resulting from in-cloud activation.

5) Page 23505: See author response to additional comment #2;

6) Page 23504-23505: See author response to major comment #1.i;

8) Page 23506: The vapour field is indeed allowed to be influenced by the microphysics. On reflection, the sentence quoted by the reviewer on page 23506 has been revised as follows to avoid confusion: “the pure microphysical behaviour of each scheme could be compared fairly, in the absence of thermodynamic feedbacks”. This means that latent heat from the microphysics is not allowed to influence the temperature; this is by design in the KiD framework (see author response to 1st reviewer comment in general comments section).

9) Page 23507: The profile of liquid water content (LWC) achieved with the warm1 profile does reach a peak value just above cloud base. This is a consequence of the fact that the whole column is lifted in response to the vertical velocity field, where at a given timestep the applied vertical velocity is the same at every gridpoint. The relative humidity reaches a peak at around 750m; at grid points above this height, the relative humidity begins to decrease such that the amount of water vapour available for condensation is reduced, leading to a slight reduction in liquid water content with height.

17) Page 23512: The convergence of total precipitation with increasing updraft strength is indeed a result of converging droplet number in the bulk schemes; the manuscript has been updated on page 23514 with the additional sentence: “This is a consequence of the fact that, towards larger vertical velocities, the droplet number concentrations

converge in the bulk schemes, thus producing very similar sensitivities".

18) Page 23512: On the issue of why the bulk scheme tends to produce earlier surface precipitation: This is not an easy question to answer, since the the onset of rain formation aloft is affected by the assumed efficiency of coalescence, and is therefore sensitive to the choice of collection kernel in ACPIM and to the choice of autoconversion scheme in the bulk model. It would appear from the plots of rain mass that the bulk scheme forms rain slightly earlier aloft.

19) Page 23513: See author response to general comment 1.i;

20) Page 23513: By design, the main effects of each factor are chosen such that moving from the low to the high value always results in a reduction in precipitation; in this study at least, all the corresponding interaction terms were also found to result in a reduction of precipitation. Therefore it is safe to express the main effects and their interactions in terms of their percentage contribution to the suppression of surface precipitation.

21) Page 23514-23515: The results from ACPIM do suggest that despite a larger rain mass aloft relative to the bulk schemes, the larger drop size resulting from the enhanced efficiency of coalescence is enough to compensate for the expected increase in evaporation, leading to larger surface precipitation rates overall (with the caveat that the effect of the different treatment of sedimentation may also be contributing to the increase in rainfall).

22) Page 23515: The fallspeeds in ACPIM are based on the discussion of terminal velocities in Pruppacher and Klett, Ch. 10. The terminal velocity of drops is regime dependent, and for drops with diameters greater than 20 microns up to and including 1070 microns, eq 10-146 on page 417 is used. This determines terminal velocity as a function of the dynamic viscosity of air, the Reynolds number (based on diameter), the density of air, and the drop radius. It should be noted that the idea of modifying the 'b' fallspeed parameter in the bulk scheme was not done necessarily to bring the bulk

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fallspeed relation closer to that used in ACPIM, but rather to explore what might happen in the bulk scheme if the rain drop size is increased. Modification of the fallspeed parameter from 0.8 to 0.825 produces an increase in rain mass aloft due to an increase in accretion rates, hence creating larger drops (the increase in accretion is not shown in table 5 but could be added if the reviewer thinks it is necessary). The accumulated evaporation is also found to reduce, leading to an increase in the amount of surface precipitation, which is qualitatively consistent with the ACPIM results.

23) Page 23516: In the Factorial Method, both the main effects and the interaction terms are classed as a source of variation, and each represents a degree of freedom. So in the 2ˆ2 case, there exists the main effects A and B, and the interaction effect, AB, which counts as three degrees of freedom in total. This is consistent with the description of the Factorial Method in Montgomery (2005).

24) Page 23517: The sentence referring to a positive correlation between precipitation and microphysical complexity: This sentence has been removed as the statement is no longer strictly true when the 1-m runs are repeated with a lower droplet number concentration.

27) Page 23526: The 'integrated rain evaporation' in table 5 is the vertical integral of the bulk evaporation rate accumulated over a 2-hour time period; the units of kgm-2 are correct. In the revised manuscript paper it will be defined as accumulated rain evaporation instead.

28) Page 23532: The authors will strive to improve the clarity and readability of the multi-panel figures in the revised manuscript.

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

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