Atmos. Chem. Phys. Discuss., 13, C8604–C8607, 2013
www.atmos-chem-phys-discuss.net/13/C8604/2013/

Atmospheric Chemistry and Physics Discussions
Open Access

Interactive comment on "An explicit study of aerosol mass conversion and its parameterization in warm rain formation of cumulus clouds" *by* J. Sun et al.

Anonymous Referee #2

Received and published: 9 December 2013

1 General Comments

This paper determines computationally efficient parameterizations for in-cloud aerosol processing due to collision-coalescence and accretion. The parameterizations are based on the numerical results of explicit modeling of the microphysical process rates and subsequent linear regression analysis. The regression analysis provides a set of power-law relationships (because the regressions were performed in relation to the natural logarithm of the chosen variables). In practice, such relations are useful in advanced numerical modeling studies with large computational domains or over long time

periods. However, there are several important shortcomings with the current study that must be addressed.

2 Major Comments

1. Power-law relationships are common in the field of atmospheric science. However, they are often only useful if a general power-law formulation can be determined. This study provides a set of 64 regression equations representing two processes. Moreover, it is not particularly clear how the authors intend the equations to be used. A quick glance at the set of regression equations is enough evidence that the equations are not general and thus one must determine which equation to use for any particular situation. In other words, these equations are not useful for large domain or long time-scale modeling of aerosol-cloud interactions. The authors need to provide a way to implement the parameterization(s) or provide a much smaller set of parameterized regression equations for the reader to utilize.

   Furthermore, the power-law relations are dependent upon ratios between a given process and a bulk aerosol or cloud property. By using these ratios in the regression equations, the authors assume that the given process and bulk aerosol/cloud property ought to have the the same power except with an opposite sign. What is the rationale for this assumption? Moreover, would it not make more sense to separate these terms and perform the regression for more than one variable? In fact, several variables ought to influence the aerosol processing rates within a cloud.

2. Other than the scatter plots in Figures 7, 9, 11, and 16, there is very little evidence that the parameterizations actually work. In fact, some of these scatter plots would suggest that the parameterization(s) may induce large errors in the aerosol

processing rates in clouds (see Figure 9d). Somewhat related to the first point above, even though a regression equation works for one setup, it may not work for another initialization. Additional simulations to verify the generality of the resulting regression equations should be performed.

Regardless, simulations demonstrating the applicability of a parameterization are necessary. For example, the parameterization(s) could be implemented into a numerical model (e.g., CRM or LES) and used to determine their ability to reflect either more detailed aerosol processing calculations or observations.

3. The model used in this study may not be sufficient for determining the power-law relations. In particular, the use of Bott (2000), in which the aerosol processing is condensed into a single dimensional problem and then relaxed back to two dimensions, may induce errors in the calculation of aerosol processing. Furthermore, an important part of cloud-aerosol interactions is the activation process. There are numerous methods in the literature for determining both the number and mass of activated aerosols. However, this is never discussed in the manuscript.

   The authors provide a section on the sensitivity to the threshold radius for collision-coalescence. I would suggest that the authors use a collision-coalescence parameterization that does not rely on a threshold parameter, especially in light of the large sensitivity to the chosen threshold in the manuscript.

4. Much of the discussion in the text revolves around the E1 case. In this case, there are approximately 1300 cm^{-3} aerosols. Without sufficiently large updrafts and under normal circumstances, I would expect that the collision-coalescence rate would would be very small. However, the authors suggest that the aerosol size distribution is largely changed due to collection processes in Figure 5. I do not see a large change in the size distribution according to this figure.

   Furthermore, the series of "E" cases represent distributions with rather long tails according to Figure 1. Are the results dependent upon the tail? How important are these GCCN to the results presented in the text?

5. A large portion of the analysis surrounds maximum rates. In almost any modeling framework, the maximum values of any field variable or process rate are inherently dependent upon the chosen modeling framework (i.e., dynamical core, grid resolution, time step, etc.). Very little can be learned from only examining maximum rates. I highly recommend that the authors present and analyze other statistical information regarding the processes rates.

6. In general, the text is very difficult to read. I would suggest that the authors' request support from a native english speaker before providing a revised submission.

References

Bott, A.: A flux method for the numerical solution of the stochastic collection equation: Extension to two-dimensional particle distributions, J. Atmos. Sci., 57, 284–294, 2000.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C8604/2013/acpd-13-C8604-2013-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25481, 2013.