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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling of cirrus clouds – Part 1: Model description and validation" *by* P. Spichtinger and K. M. Gierens

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General comments:

This paper presents the formulation of a cirrus cloud model with a double-moment bulk microphysics scheme; the new idea being the capability to keep track of nucleation by different kinds of aerosol. They also performed test runs and compared the results with other cirrus models of similar parameterizations to valid the model, and some additional sensitivity runs to test the effects of certain variables (such as temperature and vertical velocity fluctuations and horizontal wind shear).

Since cirrus clouds may have great impact on global climate due to radiative forcing and yet we still have no adequate observational data about how cirrus clouds would

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respond to environmental changes, new studies in the detailed microphysics by models such as this one are needed to make better assessments of their impact.

In general the paper is clearly written and some test and sensitivity run results are interesting. Most of the conclusions appear to be reasonable. I do have some suggestions that, I believe, would lead to some improvements of the paper.

Specific Comments:

1. The paper is very long. It includes not only the description of the model formulations but also validation and sensitivity study results (and this is just Part 1!). While the model description part is well done, the discussion part of the results can be more extensive. I suggest that the authors consider modifying it into two shorter papers; the first one is the description of the model and validation and the second one is dedicated to the discussion of sensitivity study results.

2. The validation is done by comparing the present results with other models using similar schemes. This is really just validation in numerics but not in physics. The most ideal validation will be that compared with observational data which are, unfortunately very rare. Under such a circumstance, it is useful to compare the results with models using different schemes to see if the present results are consistent with others (this is what climate modelers have done also). There are a few such cirrus models, for example, Liu et al. (2003a,b).

3. It will be useful to discuss the rationales of using specific parameterizations, for example, Koop et al. (2000) for nucleation versus that by Meyers et al. (1992). It is of great value to cirrus modeling community to see discussions of benefits and disadvantages of different parameterization schemes of ice microphysics.

4. The title of section 3.2.1 is “Homogeneous nucleation”. Would it be better to use “homogeneous freezing” instead and leave homogeneous nucleation to mean nucleation by pure water substance?

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5. Line 5 of sec 3.2.2 : “using the general size-mass relation”. Please clarify or give a reference.

6. P. 619, Line 3: Capacitance factor C. Since you are dealing with columns, why use prolate spheroid approximation and not use the more exact column capacitance (given in Chiruta and Wang, 2005)?

7. P. 624, 4th line after Eq. (45): “Growth of ice crystals does not affect their number concentration…”. This is true only because you have not considered aggregation but only diffusion growth.

8. P. 629. The comparison is made between model results. Is there any possibility of comparing with observation or results of model using different nucleation schemes (which seem to have great impact on the results).

9. All results presented in Sec. 4.2.2 are in layer-average form. The layer-average plots usually do not show problems that may be associated with boundary conditions. You should at least include a few 2-D domain plots that show spatial contour distributions of ice concentration, IWC, etc., and then give some discussions whether the results conform to the assumed boundary conditions. This is probably the most appropriate when you present the results with horizontal wind shear. Can you still maintain cyclic boundary condition with wind shear?

10. Sec. 4.2.4 The above-mentioned 2-D plots are especially relevant to cases with perturbations since cellular structures in the cirrus cloud will appear in such plots (you won’t see that in layer-average plots).

11. Starting from Fig. 16 on, many vertical profiles show double peaks. What are the reasons? Are they due to the initial environmental sounding profile? Liu et al. (2003b) used a different sounding where a single peak of RH is present initially. In their cold stable case, the cloud evolves and duo-peak features show up at later times due to the nucleation at upper level and sedimentation at lower level. The present results look

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different.

References

Chiruta, M., and P. K. Wang, 2005: The capacitance of solid and hollow hexagonal ice columns. Geophys. Res. Lett., VOL. 32, L05803, doi:10.1029/2004GL021771, 2005.

Liu, H. C., P. K. Wang, and R. E. Schlesinger, 2003a: A Numerical Study of Cirrus Clouds. Part I: Model Description. J. Atmos. Sci. , 60, 1075-1084.

Liu, H. C., P. K. Wang, and R. E. Schlesinger, 2003b:A Numerical Study of Cirrus Clouds. Part II: Effects of Ambient Temperature and Stability on Cirrus Evolution. J. Atmos. Sci. , 60, 1097-1119.

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