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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Formulation and test of an ice aggregation scheme for two-moment bulk microphysics schemes" *by* E. Kienast-Sjögren et al.

Anonymous Referee #2

Received and published: 27 November 2012

The paper introduces proposes a parameterization for ice crystal aggregation. The kernel is evaluated and fitted with a polynomial. This is then used in a prognostic equation for ice number concentration. Tests are then carried out in a box model and 2d model.

My main criticism is that I do not see what is completely new here. It is clear that aggregation will reduce ice crystal number. It is also clear that the effect of aggregation will diminish with decreasing temperature, due to the form of the equations employed in representing this process. It does not seem to require a model to demonstrate that.

Main points:

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Discussion Paper

1. The authors need to be clear about what is novel here. For example, why should I cite this over existing microphysics descriptions that include prognostic ice number such as Morrison et al. 2005 (JAS) and references therein, or Mitchell 1991 (JAS) who shows how the integrated kernel changes with changes to the ice parameters?

One way to improve the paper is to compare with observations. Even lacking comparisons with observations, it should be possible to test the proposed scheme against existing freely available microphysics schemes such as those that are distributed with WRF to provide some context for the proposed scheme and make the results more useful for other modellers.

If the authors are able to demonstrate satisfactorily that they have novel results then the work could be published. Otherwise, I feel it should be rejected.

2. A clearer description of what has been done is needed (see comments below 23980,23983,23986)

23978,24: don't quite follow this sentence, here.

23980,11: I assume that this is because eq 2: =0.5*I1-I2. This could be explained a bit more clearly.

23982,5: I do not quite understand the choice of mass distribution used here. Aggregation tends to produce distributions with an exponential tail (in D space). A lognormal may be a good approximation for the particles growing by vapour diffusion, but not so applicable after aggregation has acted for some time.

23982,19, unnecessary colon.

23983,3: further -> other ?

23983,7: why use hexagons. If aggregation is occurring then the most likely type of 'habit' will be an aggregate. Perhaps the intention is only to model the first very few aggregation events?

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23983,14: eq 9: is this the cross sectional area or total surface area? Only the cross sectional area is required - which orientation is assumed?

23983,23, eq 12: Is there a pressure term for the terminal fallspeed?

23986,3: where is the equation for the increment in the modal size of the distribution? I assume that the number concentration is predicted and then the modal mass is diagnosed?

23986,26: newer results from Connolly et al. (2012) for laboratory studies are also available.

23989,7: I do not quite follow this. All we have is that the flux in the top is half the flux from the bottom. One way to do this is to simply halve the number concentration of the size distribution but keep f(m) the same. That would mean that all sizes are reduced in number, not just the large ones.

23993,26: 'not otherwise' i don't follow this sentence.

23994: Tian etal JAS 2010 paper use a lognormal distribution to normalise the size distributions. Westbrook et al. 2004 shows that a function with an exponential tail is a solution of the collection equation. This perhaps supports your choice of lognormal.

fig 8 - no contour line values.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23975, 2012.

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