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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Physical controls on orographic cirrus inhomogeneity" *by* J. E. Kay et al.

Anonymous Referee #1

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Review of 'Physical controls on orographic cirrus inhomogeneity' by Kay et al.

This paper describes he evolution of a cirrus layer, in a Lagrangian sense, through the coupling of a parcel model that is capable of representing microphysical processes in detail, with the dynamical output from a mesoscale model.

I recommend that the paper should be accepted with revisions.

Main points:

1) I found the approach interesting, but the poor agreement between the model and observations leads me to suggest that the authors should consider restructuring their paper. Because the forecast was not too good, I felt that the comparison between the model and observations detracted from the main message of the paper. I would be

more tempted to state (with brief reasons e.g. advection, moisture) that the forecast was not good in this case and then proceed to present the results as representative of cirrus but not necessarily realistic in this case. Then, the authors could concentrate on attributing variations in Nice and sigma to specific processes or forcings. Additionally, it would be interesting to see what the effect of temporal evolution on the P(sigma) was as parcels moved away from the mountains.

2) A main concern is that I felt that not enough of the methodology and details were described. In particular, details were lacking with regard to the coupling of the parcel model to mm5. For example, how was the moisture initialized in the parcel model? Does the parcel model evolution of humidity match the mm5 or was the parcel model moisture periodically nudged to match that of mm5?

3) The authors indicate that the treatment of the fallout region is simplified. It is implicitly assumed that a 1000 m thick region below the production region is at ice saturation and no further evolution of the size distribution occurs. Firstly, i would argue that a thickness of 3000m for the fallout region appears more realistic from inspection of fig.1b. I suggest that a simple way to build in a realistic thickness and variability for the fallout region would be to use the mean and standard deviation of the thickness indicated in fig 1b to define dz at each timestep using a montecarlo approach. This exercise could be carried out as a sensitivity analysis. Secondly, the mm5 vertical velocities indicate that the fallout region will be subject to vertical velocities similar to those experienced in the formation layer and will therefore be subject to conditions of ice super- or subsaturation. The only way I would suggest this could be dealt with is to couple another parcel model below the one present in the formation zone, to evolve the particles that fallout. I would suggest that the microphysical processes acting in the fallout region (diffusional growth, aggregation) would tend to amplify the heterogeneity generated in the formation layer. It would be good to see an improved treatment of the fallout region, but failing that, the likely effects of the microphysical processes in the fallout layer needs more discussion.

ACPD

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Minor points:

p4890 line 25. It can be seen that sigma is more dependent upon Reff than Nice.

I suggest moving the last para on p4891 ahead of this statement, so that the reader can see how w controls Nice which to some extent controls Reff.

section 4.1 You quite rightly comment on the shortcomings associated with assuming a constant DeltaZ, but would it be possible to use the mm5 output to diagnose a thickness? A relative humidity threshold could be used to determine how deep a Ci cloud would grow (admittedly in 500m intervals). Or the statistical approach suggested above could be used.

section 5.1 Do the comments about the effects of adding IN include the both the addition of 0.03 cm-3 and introducing the Meyers et al. scheme?

section 5.3 I think you should briefly describe how the Reisner scheme predicts Nice.

section 5.3 It could be argued that eq 1 is directly (if ice particle mass is proportional to size squared) proportional to ice water path. It would be nice to see P(IWP) as well as P(sigma) and P(Nice).

fig13 and associated discussion. Although the grid box mean RH is below ice saturation there is an implicit pdf of humidity, if the mm5 has a cloud fraction parameterization. Therefore, some parts of the grid box will be significantly more moist.

What were typical values for Reff? Perhaps there should be some discussion about how Reff might be expected to evolve in the fallout section and how this would effect the estimates of sigma.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4889, 2007.