Atmos. Chem. Phys. Discuss., 7, S1923–S1925, 2007
www.atmos-chem-phys-discuss.net/7/S1923/2007/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 18 May 2007

Review of "Physical controls on orographic cirrus inhomogeneity"

This manuscript describes parcel model simulations of orographic cirrus used to investigate the impact of mesoscale vertical wind variability on cirrus optical depth and ice concentration variability. The manuscript is very well written, well cited, and interesting. I have one major concern about the optical depth variability associated with cloud depth variability versus that associated with ice concentration variability, as discussed below.

Major Comment:

A key point of the paper is that variability in cirrus ice concentration (N_{ice}), driven by variability in mesoscale vertical wind (w), can, in part, explain the observed variability in optical depth (τ). However, the width of the simulated τ distributions (indicated both in Figure 12 and Table 4) are far narrower than the observed τ distribution. Increasing the model resolution from 36 to 4 km only slightly broadens the simulated τ distribution (Figure 12a). The authors acknowledge that the observed optical depth variability may be largely driven by variability in cloud depth, as indicated by the lidar data shown in Figure 1. However, I think the paper would benefit greatly from an estimate of this source of variability. For instance, one could look at the variability of the vertically averaged extinction from individual lidar profiles as a means of examining the observed variability independent of cloud depth.

Minor Comments:

1. It would be worth noting that vertical motions in wave clouds are typically much larger than in other types of cirrus, such as synoptically-forced cirrus. This is an important point in light of the current debate about small ice crystal concentrations in cirrus clouds. The large ice concentrations simulated here (up to 10 cm^{-3}) are reasonable for wave clouds but are probably not typical for other types of cirrus. Even in the context of wave clouds, it is interesting that parts of the clouds have relatively low ice concentrations ($<$ 0.1 cm^{-3}), where as in situ measurements typically suggest large ice concentrations throughout the clouds.

2. Although the parcel model is described in detail in the Kay et al. [2006] paper, I think a bit more detail should be included here. In particular, it would be nice to have some description of how the ice fallout time ($\tau_{fallout}$) is calculated. Presumably, it is sensitive to the assumed parcel and cirrus depths.

3. Page 4896, lines 24-26: The authors state that w and T control the maximum homogeneous nucleation rate and the resulting ice concentration. I do not think this statement is quite correct. J_{hom} is controlled by ice supersaturation and temperature.

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The reason N_{ice} increases with w is because when w is larger, the supersaturation continues to increase even after some ice crystals nucleate and begin to grow. With increasing w, it takes more ice crystals to quench the increasing supersaturation.

4. Section 5.3: To what degree are the simulated cirrus lifetimes controlled by ($\tau_{fallout}$) versus subsidence-driven heating? Following on comment (2) above, the fallout times in a parcel model are somewhat arbitrary, and it would be interesting to know how sensitive the simulated cloud lifetimes are to the assumed fallout time specification.

5. Page 4900, lines 23-26: It is stated that the Meyers et al. [1992] parameterization gives unreasonably large IN concentrations, and the simulations using this parameterization are probably not atmospherically relevant. This is an important point. In fact, I might argue for removing the Meyers et al. simulation just to avoid confusion.

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

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Interactive Comment