Atmos. Chem. Phys. Discuss., 5, S4009–S4011, 2005
www.atmos-chem-phys.org/acpd/5/S4009/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A modelling study of the impact of cirrus clouds on the moisture budget of the upper troposphere" *by* S. Fueglistaler and M. B. Baker

Anonymous Referee #2

Received and published: 18 November 2005

This analytical study investigates the water budget determined by ice growth and fallout in a fixed volume of air. Simplified models are useful because they reveal controlling factors and key dependences between variables in many cases. In this general view I support the efforts of the authors concerning upper tropospheric dehydration.

The paper contains a detailed explanation of the model equations and solution, which is mathematically sound. I suggest to publish the paper in ACP, but only after the authors addressed the issues listed below and have clearly spelled out the limitations of their approach.

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In particular I am worried about three issues. First, temperature amplitudes originating from small or mesoscale fluctuations driven by turbulence or waves are not constant, but depend on the oscillation period. Typically, low frequency cooling/warming waves are asscoiated with large, and high frequency waves are associated with small amplitudes. The model does not capture this dependence and I speculate whether this could introduce a systematic bias.

Second, a key assumption is that ice is in equilibrium as soon as it is formed. This implies that the timescales of relaxation towards ice saturation are fast compared to both the temperature perturbation timescale and the sedimentation timescale. However, cloud simulations show that supersaturation relaxation times range between minutes to few hours, depending on temperature and number and size of cirrus particles, i.e., comparable to the other timescales. Rather long supersaturation relaxation timescales are also the cause of (at least in part) frequently observed in-cloud supersaturations. I argue that this introduces another timescale in the model, rendering its predictions using the single parameter P less universal.

Third, the sedimentation timescale is based on a monodisperse ice crystal size distribution. In reality, ice crystal size distributions are far from unimodal or monodisperse, and sedimentation is a continuous process that integrates loss of ice mass over the entire size spectrum. The least the authors could try is to parametrize the terminal fall speed as a function of the ice water content with underlying assumptions about the form of the size spectrum. Such parameterizations are available in the literature and often used in global models to mimick sedimentation.

The fact that crystal spectra are polydisperse together with the fact that sedimentation is intimately coupled to growth (as recognized in the model) might imply that individual clouds may well lead to a substantial dehydration below the saturation mixing ratios. At least some of the available water measurements carried out at high (northern) latitude do indicate that such cold clouds act as efficient 'dehydrators'. The possible existence of ice nuclei in low concentrations make things worse in this regard: they grow and fall

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ahead of the later freezing particles and might remove a substantial amount of water, although the total crystal number (and thus size) is dominated by the latter. Such a ‘bimodality’ is likewise not considered in the model, and I doubt whether this aspect of the action of ice nuclei is covered by the scaling arguments in section 4.3.

In sum, I suggest that the authors (i) consider the above concerns critically in their revision and (ii) very clearly spell out that their conclusions may not be robust and should be checked by more detailed cloud simulations.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 9769, 2005.