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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

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We would like to thank both reviewers for their work and constructive criticism. Both reviewers expressed some concern regarding the applicability of results from a simplified model to the atmosphere, and suggested additional sensitivity runs and/or an extended description of limitations. We are fully aware of these problems, but think that the introduction lays out the limitations, as well as the motivation as to why we think simplified models still provide valuable insight into 'real world' processes. We do think that the claims made in this study are discussed in the context of the model limitations, and thus defendable. We have therefore not modified the model underlying the calculations, but adjusted the text according to the reviewers' suggestions.

Detailed response to points raised by reviewers:

Reply to reviewer 1:

1) We do agree that the layer depth ’h’ is a problematic parameter. However, some measure of layer thickness is essential in a model that addresses sedimentation. It is important to note that the governing parameter ‘P’ scales linearly with ‘h’, so that our results may be easily scaled to empirically determined values for the layer thickness. The fall distance after leaving the cloud, as suggested by the reviewer, is not the scope of this paper. Such a scaling parameter may be useful to model the vertical redistribution of water. However, this paper is limited to the question what happens with the water concentration in the layer wherein the particles nucleated. There appears some confusion with respect to ’h’ and ’cloud thickness’ - we have therefore added a few sentences in Section 2 that should help to clarify the issue.

2) While the model has many simplifications,it is one strength of the formulation (and indeed our main interest) that the fall speed is NOT held constant, but is a function of time. Thus the results are an improvement compared to calculations based on the fall speed of the particles ’once they reach their maximum size’. Again, we think that there may be some confusion as to what the goals of this study are: whether the particle fall speed is comparable to the vertical wind field is irrelevant for our purpose, the particulate phase always falls relative to the gas phase, no matter what the motion of the gas phase may be (the latter is of course important in a geometric frame of reference, but not in the Lagrangian employed here).

3) Isobaric temperature perturbations were taken for reasons of simplicity in the analytic formulation of the model. The error introduced by this simplification is very small (a corresponding statement is added in the revised manuscript).

4) The initial aerosol distribution does influence the resulting ice particle number density, however, it is in most cases a second order effect compared to the dependence on cooling rate and temperature at the time of nucleation onset (see also Figure 3 of

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Kaercher and Lohmann, JGR, 2002). We thus ignore this dependency here.

5) Yes, we agree that this definition would make sense as well. However, we would like to retain the definition as is so as not to introduce the possibility of inconsistencies between code, printed model formalism and figures in the revised manuscript.

6) This refers to Figures 4 and 7, which are now properly labelled.

7) See also discussion above - the formulation is such that we can make a statement about the layer wherein the particles nucleate, not how far the particle falls, and not where it deposits its water upon evaporation once it has left the nucleation layer.

8) We have added this to the description of the saturation point temperature (Section 3).

9) We have slightly modified our formulation; see Discussion.

Reply to reviewer 2:

1) We agree that period and amplitude may be correlated in the atmosphere. If one would have a well supported amplitude/frequency relation, one could evaluate the model for prescribed delta_T/tau_T pairs. Such a delta_T/tau_T relation, however, is beyond the scope of the work presented here where we have tried to isolate the problem from additional semi-empirical relations such as the probability distribution of the ratio delta_T/tau_t. Figure 6 in part addresses this issue, as it shows the dehydration efficiency as function of delta_T and tau_T (which is a linear function of P). At first glance the figure suggests that the dependence on delta_T is much larger than on delta_T, however, if delta_T were a very strong function of tau_T, this may be not true. This aspect clearly deserves further attention. However, we think that the conclusions, as formulated in section 6, are not directly affected. Rather, the role of such a relation may be explored in a future publication.

2) We have added a statement in Section 2 that adresses the issue of recently observed supersaturations within cirrus clouds. We therein state that we consider it pre-

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mature to include such an effect on the grounds that at present it is not clear what may be responsible for these supersaturations (hypotheses range from cubic ice to coating with HNO3 to dynamic (non-equilibrium) causes).

3) In a model with explicit microphysics we could indeed more accurately calculate the effective sedimentation rate. However, for our purposes it is not clear that use of ice mass/fall speed parameteriztions would be preferable to our assumption of monodisperse populations. The use of ice mass as a surrogate for fall speed assumes, as the reviewer states, underlying particle size distributions associated with each mass. Thus the ongoing evaporation and growth of the particles, which is an important aspect of our paper, would not be included in a consistent way were we to adopt the parameterizations the reviewer suggests. The only solution to accurately model these processes seem single particle models, with the caveats outlined in the Section 'Introduction'. We do agree with the reviewer that these issues should be pointed out more clearly in the manuscript, and have added a short paragraph in Section 'Limitations'.

Finally, we cannot see how any spontaneous process could lead to dehydration below the saturation mixing ratio corresponding to the minimum temperature, as suggested by the reviewer. We assume there must be a misunderstanding.

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