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Interactive Comment

Interactive comment on "Introduction of prognostic rain in ECHAM5: design and Single Column Model simulations" *by* R. Posselt and U. Lohmann

Anonymous Referee #2

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General Comments

The paper addresses the introduction of a prognostic rain scheme in ECHAM5. Results from simulations with a single column model are shown for two case studies.

A diagnostic treatment is often applied to represent precipitation processes in global atmospheric models. Consequently, interactions of rain drops with cloud water (accretion) and other processes are highly idealized in the models. This may have considerable implications for simulated hydrologic cycles and climate.

The current study is based on a newly developed prognostic treatment of rain. Overall, the approach presented in the paper is reasonable and can be expected to lead to useful improvements of microphysical schemes in global models.

There are two major issues that should be addressed in a revised version of the manuscript. First, some of the approximations that are used are not sufficiently explained and tested. Second, simulation results for different sub-time steps should be explained in more detail since the sensitivity of the results to different sub-time steps appears to be rather non-trivial.

Specific Comments

P. 14679, l. 21: Snow is still treated diagnostically in this study and the focus is on warm rain processes. It seems that a diagnostic approach for precipitation which removes precipitation within a single time step is particularly worrisome for high precipitating clouds. The time it takes for the precipitation to reach the ground is potentially much greater than the model time step. Could the authors clarify what kind of atmospheric conditions may be expected to cause particularly large errors in models if precipitation is treated diagnostically? Do the simulations that were selected for the current study sufficiently address these situations? The authors should consider adding an additional case study to specifically address the effect of the model improvements for situations with high precipitating clouds.

P. 14682, l. 5: A precipitation fraction is used in the model. The authors should explain the maximum overlap assumption for the precipitation fraction in their model. Is this consistent with the cloud overlap assumptions that are used in other parts of the model (e.g. radiation calculations)? Is a maximum overlap reasonable for large vertical distances between cloud layers? Does the rain fully overlap with the cloudy portions of the grid cells?

P. 14682, l. 23: The same sedimentation velocity (v_m) is used for rain mass and number mixing ratios in the model. From the discussions on the following pages it appears that an effective terminal fall velocity based on the total rain mass flux is used. This should be clarified since this assumption can be expected to substantially affect the results. Results for v_m are potentially very different compared to the effective terminal fall velocity for the droplet number flux (v_n), i.e. if Eq. (3) was instead applied to the droplet number size distribution. The authors should explain the motivation for this approach in some detail. In addition, the sensitivity of the simulation results to this approximation should be addressed, e.g. by adding simulation results based on v_n instead of v_m for at least one of the cases.

P. 14686, l. 6: Asymptotic solutions based on Eq. (11) and an approximation based on these solutions are shown in Fig. 1. It is not clear how large the errors are that are caused by the approximation. The authors should also include results based on direct application of Eq. (11) in Eq. (3) in this figure so that the errors of the approximations can be understood.

P. 14687, l. 5: The authors do not allow fall velocities greater than a certain threshold velocity v_{max} in the model. Although this appears to be a reasonable approach, the authors should provide information about how often this threshold is reached in the simulations for the case studies so that the effects of v_{max} can be understood.

P. 14687, section 2.4: If the number of drops larger than 5 mm exceeds 1% of the total rain drop number concentration the rain drop distribution is changed by increasing the total rain drop number in order to mimic the effects of drop break-up processes. What determines this threshold value? Have sensitivity experiments been performed that could be used to determine corresponding uncertainties in model results?

P. 14686, section 3.2: The SCM approach should be explained in more detail. Are advective tendencies and nudging towards thermodynamic profiles and winds used? How strong is the nudging? How are the surface fluxes computed?

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P. 14690, l. 7 and following: Differences between simulated and observed precipitation rates are discussed. Depending on whether nudging towards thermodynamic profiles was used or not, the total amount of surface precipitation in a SCM simulation is generally related to the treatment of water vapour sources and sinks in the domain. For instance, insufficient amounts of simulated precipitation may be caused by unrealistically weak sources of moisture from advection or surface evaporation. An analysis of the moisture budgets could be an useful addition to the discussion. Furthermore, how do results for the precipitation flux and cloud water path from the simulations compare to results from other SCM's or cloud resolving models for the same case? If these results are similar, this might provide further evidence for potential problems with the forcing data set.

P. 14691, l. 13-21: It is not easy to follow the argumentation here. Why should the accretion rate increase if the number of sub-time steps is increased? Increases in the number of sub-time steps should lead to more efficient drop sedimentation because the fall velocities will tend to exceed v_{max} less frequently. Shouldn't this tend to reduce the rain water contents locally which should then lead to reduced accretion rates? Reduced accretion rates should lead to increased cloud water contents and therefore increased autoconversion rates. However, this is opposite to what the simulation results show. What leads to the simulated decrease in the autoconversion rates?

P. 14692, l. 2-5: Some of this discussion is also relevant to the discussion on p. 14690 and should probably be moved there.

P. 14692, section 3.3: Similar to the other case: How strong is the nudging for this simulation and how are surface fluxes represented? What are the impacts of nudging on the precipitation results and sensitivity to the number of sub-time steps?

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Technical Corrections

P. 14677, l. 20: "weigh" instead of "weight".

P. 14677, l. 24: "... larger than 5-19 μm *in radius*."?

P. 14687, Eq. (13): A definition of parameter D_B is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14675, 2007.

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