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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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General Comments:

The manuscript by Posselt and Lohmann addresses an interesting problem of the parameterization of precipitation in global climate models. The diagnostic precipitation schemes which used in many models do indeed have several disadvantages and therefore an implementation of a fully prognostic scheme would be a step forward. The manuscript is well written and easy to read. It is very much appreciated that the authors are willing to share and discuss these more technical details.

Unfortunately, the suggested implementation of prognostic precipitation uses a very simple approach and is, in my opinion, inadequate in several ways. The main problems are:

- The use of an explicit numerical sedimentation scheme combined with a time-step dependent limitation of the sedimentation velocity. The method itself might be inefficient in this type of model due to the large time step of the dynamical core. The limitation procedure introduces a strong time step, or sub-time step, dependency. The numerical efficiency of the explicit scheme is not discussed and probably poor.
- A very crude piecewise linear approximation of the sedimentation velocity is applied. This simplification is unnecessary.
- A two-moment approach is used without taking into account two different sedimentation velocities for mass and number concentration.
- Some microphysical parameterizations used in the scheme are inadequate or not sufficiently explained.

To discuss or fix these issues, the following modifications of the paper would be necessary:

- Use of a more adequate, semi-lagrangian or implicit, sedimentation scheme and (maybe) a comparison with the explicit approach.
- Use of a correct approximation of the sedimentation velocity.
- Use of separate sedimentation velocities for mass and number concentration. Maybe a comparison with the present approach.
- Use of an adequate parameterization of evaporation of rain drops.

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Unfortunately, this is more than a major revision and therefore the present manuscript should be rejected.

The aim itself, using a prognostic precipitation scheme, is a valuable contribution and I hope the the authors will consider a resubmission of a new paper on this subject.

Some detailed comments:

p. 2, l. 34: The statement that the autoconversion process ’is less important in the atmosphere than accretion of cloud droplets by rain’ is very misleading if not wrong. Without the autoconversion process there would be no rain drops to collect cloud droplets (in a warm cloud).

p. 2, l. 54: Diagnostic precipitation schemes have been used in NWP models for decades. The statement: ’This method [diagnostic precipitation] was put forward by Ghan and Easter (1992)..’ suggests that diagnostic precipitation schemes were not known or used before 1992. This should be rewritten.

p. 3, l. 86: ’Snow is still treated diagnostically’. Why? Due to the longer microphysical timescales and the lower fall speed a prognostic treatment is more important for snow than for rain.

p. 4, l. 105: ’... as well as detailed cloud microphysics’. The word ’*detailed*’ should not be used here, as some readers might associate a bin microphysical scheme with it.

p. 5, l. 133: The assumption of a constant rain drop number concentration during evaporation, i.e. neglecting P_{eva}, is inadequate or simply wrong. See, for example, Khairoutdinov and Kogan (2000). It is quite disturbing that the authors, who obviously know this paper very well, are not willing to discuss the problem of size effects of evaporation in some more detail.

p. 5: How did you parameterize the self collection of rain drops P_{scr}?

p. 6, l. 155: ’For simplicity the same velocity is used for rain mass and number...’.

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Interactive Comment

This an unnecessary oversimplification, which removes one of the main advantages of two-moment schemes. Using different sedimentation velocities for mass and number concentration two-moment schemes are able to describe gravitational sorting, something which is not possible in a one-moment scheme.

p. 6-7: The lengthy description of the parameterization of the sedimentation velocity is not necessary. Claiming the Grabowski (1999) was the first to 'suggest' Eq. (5) is hopefully a joke. Equation (5) is a trivial consequence of Eq. (4).

p. 6, l. 178: 'In models it is more convenient to work with drop mass instead of the droplet diameter' (line 178). This is not true for the treatment of sedimentation that is presented here. For this exercise, it would actually much more convenient to use the diameter D, since Eq. (9) is a function of diameter, not mass.

p. 7, Eq. (10): The approximation Eq. (10) looks like a good idea, but it should maybe be mentioned that the error relative to the Rogers formula is about 50 % for a 100 micron diameter drizzle drop and about a factor of 2 for a 50 micron drizzle drop. A comparison with a more accurate formula would be interesting. Why not simply use your formula to derive a new approximation, e.g. using the formula of Beard (Pruppacher and Klett 1998, p. 417)?

p. 7, Eq. (11): The step from Eq. (10) to Eq. (11) is not clear to me. Eq. (11) is maybe a crude approximation to $\mathrm{v}_m = b_1 + (b_2 - b_1)(1 + 5\, b_3 D_0)^{-4} - b_2(1 + b_3 D_0)^{-4}$ which, in my opinion, is the correct v_m for Eq. (10). Why do you use this piecewise linear approximation?

p. 8, l. 204: Limiting the maximum sedimentation velocity to $\Delta z/\Delta t$ is a very crude approximation. Actually, an implicit numerical scheme is not that hard to implement for sedimentation.

p. 8: You may want to consider a better parameterization of the breakup process.

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