

## ***Interactive comment on “Two-moment bulk stratiform cloud microphysics in the GFDL AM3 GCM: description, evaluation, and sensitivity tests” by M. Salzmann et al.***

**M. Salzmann et al.**

salzmann@princeton.edu

Received and published: 15 June 2010

Reply to review by Anonymous Referee #2

We thank the Anonymous Referee for highlighting several important issues.

### **Major comments:**

The implementation of the turbulence parameterization has previously been described in GAMDT04. The mixing coefficient for heat includes effects of boundary layer turbulence which is parameterized after Lock et al. (2000). Boundary layer turbulence is either thermally (buoyancy) or mechanically driven (wind shear). In the present imple-

C3994

mentation of the Lock et al. (2000) scheme buoyancy is caused by surface heating and cloud-top radiative cooling, but buoyancy reversal by evaporative cooling of entrained air which is also included in the original Lock et al. (2000) scheme is omitted (see GAMDT04 for details). In the free troposphere, the same local mixing parameterization is used as in GAMDT04, which does not explicitly take into account effects of deep convection.

Variability of  $w$  due to sub-grid scale gravity waves is not explicitly taken into account at present although it might play an important role. Neither the effect of gravity waves excited by orography nor of gravity waves excited by deep convection on  $K_T$  is parameterized. Instead, the effect of sub-grid scale gravity waves is included implicitly in the specification of the lower bound of  $\sigma_w$  for cirrus clouds. We do realize that this is not optimal, but the inclusion of a parameterization of the effects of sub-grid scale gravity waves would have been outside the scope of the project. We have added the following Sentence in Sect. 2.2.3 of the revised manuscript:

“This relatively large lower bound on  $\sigma_{\min}$  takes into account that the effects of sub-grid scale gravity waves are not parameterized explicitly in the present version of the model.”

The relationship between  $\sigma_w$  and  $K_T$  follows a suggestion by Ghan et al. (1997), although Ghan et al. argue that it is preferable to diagnose  $\sigma_w$  directly from the turbulent kinetic energy (TKE). TKE is, however, not predicted in AM3. Ghan et al. also place a lower bound on  $\sigma_w$  of  $0.1 \text{ m s}^{-1}$ , arguing that this is appropriate since cloud-top radiative cooling is poorly resolved above the planetary boundary layer unless the resolution is far finer than is computationally feasible in global models. Furthermore,  $\sigma_w$  is expected to be regime dependent, which complicates the choice of a universal lower bound. Several other studies have used various bounds. Chuang et al. (1997) uses a normal distribution for the  $w$ -PDF with a constant standard deviation of  $0.5 \text{ m s}^{-1}$ , and in addition performs sensitivity tests with standard deviations of  $0.25$  and  $0.75 \text{ m s}^{-1}$ . Storelvmo et al. (2006) follow a similar approach to Ghan et al. (1997) and use a

C3995

normal distribution with a minimum standard deviation of  $0.3 \text{ m s}^{-1}$ . This is the same minimum standard deviation as has been used for droplet activation in the NEW run.

Although the effect of cloud top radiative cooling is parameterized in the Lock et al. PBL scheme, in practice, for  $\sigma_{min} = 0.7 \text{ m s}^{-1}$  the lower bound  $\sigma_{min}$  is effective in about 98% of all cases, i.e. the parameterization behaves essentially as if the variance were fixed. The choice of  $\sigma_{min}$  does have an effect on the radiation balance. Decreasing  $\sigma_{min}$  to from 0.7 to  $0.3 \text{ m s}^{-1}$  increases netradTOA by more than  $2 \text{ W m}^{-2}$  (last sentence of Sect. 2.2.1 of the revised manuscript). This issue is also being discussed in some detail in the forthcoming Golaz et al. manuscript. Our finding regarding the higher droplet number in the BASE run due to more super-cooled droplets is, however, not affected by our choices of  $\sigma_{min}$ .

In Sect. 5, we have mentioned an ongoing effort to include a new cloud cover scheme into the AM3 GCM which uses a joint PDF of sub-grid vertical velocity, temperature, and total water mixing ratio. This should eventually replace the present treatment.

In the NEW run, aerosol activation and ice nucleation are driven by the same w-PDF, but with  $\sigma_{min}=0.25 \text{ m s}^{-1}$  for ice nucleation and  $\sigma_{min}=0.3 \text{ m s}^{-1}$  for liquid droplet activation. At present, cloud cover is determined independently of  $\sigma_w$ , but the mean of the distribution is identical to the vertical velocity that drives large-scale condensation in the cloud cover scheme. Furthermore, at present,  $\text{RH}_e$  is used to decide whether homogeneous nucleation can take place (threshold given in Eq. 13). Using the same  $\sigma_{min}$  for droplet activation and ice nucleation would have a small effect on the radiation balance and would not change the microphysics plots presented in the manuscript significantly.

p. 6381, line 10–1 and page 6389, regarding potentially unrealistic microphysical properties: A consequence of specifying a minimum standard deviation of the vertical velocity distribution  $\sigma_{min}$  is that unrealistic microphysical properties in the case of extremely low  $\sigma_w$  are avoided. The assumption of activation taking place only in the newly formed

C3996

cloudy fraction of the grid box (Ming et al., 2007) does, however, lead to much more frequent occurrences of very low cloud droplet numbers compared to allowing activation also in pre-existing clouds (Golaz et al., manuscript in preparation). By assuming that droplet activation occurs only in newly formed cloud fractions, we neglect secondary activation of interstitial aerosols in existing clouds (Ming et al., 2007). This choice can be justified by parcel model simulations showing that condensation of water vapor onto existing droplets is effective at suppressing supersaturation, often outweighing a potential increase in supersaturation caused by more vigorous updraft. Nonetheless, we acknowledge that secondary activation is possible when an air parcel undergoes large acceleration above cloud base. Unfortunately, the cloud scale dynamics which ultimately determine local supersaturation in many cases can not be reproduced in present-day coarse grid GCMs. Golaz et al. (manuscript in preparation) presents a review of various approaches that have been used in previous studies to parameterize the sub-grid variability of  $w$ .

Since an equation analogous to Eq. 1 is solved for ice nucleation (Eq. 16 of the revised manuscript), homogeneous and heterogeneous nucleation can in principle occur within in the same grid box, although in the subsequent microphysics calculations, these regions are not treated separately.

#### Minor Comments:

1. p. 6378, l. 23–25: Several studies have suggested that heterogeneous nucleation could result in smaller ice crystal numbers compared to homogeneous nucleation (e.g. DeMott et al. 1997, Kärcher et al., 2007, Murray et al., 2010). Although this appears to be plausible and could help to explain observations of low ice crystal numbers in tropical ice clouds, we now chose a more cautious formulation. The sentence in question now reads:

Thus, the presence of a relatively small number of IN at low temperatures could, in principle, lead to a reduction in ice crystal number compared to a homogeneous nucleation

C3997

scenario (e.g. DeMott et al., 1997, Kärcher et al., 2007).

2. p. 6386, l. 4: The horizontal resolution is  $220\text{ km} \times 220\text{ km}$  in the BASE and the NEW run (line 2, p. 6380). This is close to the  $210\text{ km} \times 210\text{ km}$  in Tompkins et al., 2007.

3. We added the following sentence to Sect. 2.3:

"There is, however, large uncertainty related to this choice and nucleation thresholds are likely to scatter over a large range of supersaturations depending on the composition and mixing state/chemical age of the dust aerosol. At present, hydrophobic and hydrophilic (coated) dust are combined into a single prognostic variable."

4. p. 6386, l. 9–11: This is correct. Homogeneous nucleation is taken into account only below 238.15 K.

5. p. 6388, lines 26-27: The critical soot number in the Liu and Penner (2005), Liu et al. (2007) parameterization does depend on  $w$  and thus also on  $\sigma_w$ . In Sect. 2.2.3 of the revised manuscript we have changed

" ... and Eq. (10) of Liu et al. (2007) specifies the critical IN concentration above which only heterogeneous nucleation is allowed to take place (see Liu et al., 2007, for details)."

to:

" ... and Eq. (10) of Liu et al. (2007) specifies the critical IN concentration above which only heterogeneous nucleation is allowed to take place as function of vertical velocity and temperature (see Liu et al., 2007, for details)."

Since the parameterization is readily available in the published literature and since including the complete set of equations would have made our already lengthy manuscript even lengthier we did not repeat this equation as well as several other equations given in Liu and Penner (2005) and in Liu et al. (2007).

6. p. 6389: The analogue of Equation 1 for ice has been included in the revised

C3998

manuscript (Eq. 16 of the revised manuscript).

7. In AM3, scavenging is treated independently from activation/nucleation and identical scavenging coefficients are used for ice and liquid clouds. While such a treatment is not uncommon in GCMs, it will certainly be improved in a future version of the model. To clarify the current approach in Sect. 2.1 of the revised manuscript, the sentence

"The removal of aerosols and trace gases by precipitation is parameterized as a first-order loss process and depends on prescribed species-dependent in-cloud tracer fractions."

has been changed to:

"The removal of aerosols and trace gases by precipitation in the liquid and ice phase is parameterized as a first-order loss process and depends on prescribed species-dependent in-cloud tracer fractions."

Furthermore, the bracket "(identical for ice and liquid clouds)" has been added to the first sentence of the second paragraph in Sect. 2.4 of the revised manuscript.

8. p. 6390, l. 7–8: We assume that the mineral composition of half of all dust aerosols favors ice nucleation. In addition, we make an assumption regarding the effect of surface coating.

9. Effective radii of droplets and cloud ice in the NEW run are obtained by dividing the third by the second moment of the size distribution given by Eq. (5) (see p. 6383, l. 19–21), i.e. by using the term on the r.h.s. of Eq. 7 which does not contain  $k_2$ . Using Eq. 6,  $k_2$  can be computed analytically for the size distribution given by Eq. 5.  $k_2$  has been introduced merely for the sake of comparison to  $k_1$  in Eq. 4. This comparison suggests that the differences between the solid lines in Fig. 4b and d are not caused by the different methods used to diagnose  $r_{eff}$ .

10. Although during MOZAIC not only cloud free air was sampled, one can argue that the exponential parts of the humidity statistics are characteristic of cloud free air

C3999

(Spichtinger et al., 2004). It therefore seems justified to compare the fits by Gierens et al. (1999) to simulation results for cloud free air, as has also been done in a significant number of previous studies. A similar set of plots to Fig. 5 suggests that only taking into account northern hemispheric mid-latitude (30–70N) grid points would not make a large difference for Fig. 5 in spite of the fact that observed super-saturations do depend on altitude and latitude. Note also, that the MOZAIC fit is very similar to the fit based on AIRS data which is also shown in Fig. 5.

11. Observed spatial patterns of super-saturation vary strongly with altitude (e.g. Gettelman et al., 2006) so that a detailed analysis becomes rather complex and would significantly increase the already large number of plots in the manuscript. A rough preliminary inspection of the spatial patterns of supersaturation suggests that they are probably in line with observations, at least qualitatively. Regarding the tropical OLR bias pattern, a similar pattern is found in the BASE and the NEW run. This indicates that the newly added treatment of supersaturation is most likely not the main cause of this bias. The increased OLR bias in the southern Ocean also appears to be unrelated to differences in ice super-saturation.

12. It is not clear how reliable the CloudSat data is near the surface, where surface clutter effectively reduces the radar sensitivity (Marchand et al., 2008).

13. The y-axis label "pressure (hPa)" has been removed from Figs. 10b and b, and the size of the individual plots in Figs. 10 and 11 has been increased in the revised manuscript. The large number of lines arises from the goal of presenting a closed budget.

14. p. 6403, l. 14–18: The sentence has been altered as follows:

"Fig. 10a and b also illustrate the interplay of the cloud cover and the microphysics scheme as suggested by the global balance of terms. This indicates the need for studies which focus on studying these parameterizations in combination with each other in addition to studies in which they are assessed in isolation from each other."

C4000

15. In Sect. 2.2.1 of the revised manuscript where the Tompkins' (2007) scheme is first mentioned, we now refer to the scheme as the "Tiedke (1993) based Tompkins (2007)" scheme. In the remainder of the text we have changed Tompkins' scheme to Tompkins' (2007) scheme, in order to distinguish it from the Tompkins' (2002) PDF based scheme.

16. p. 6384, l. 4–6: The minimum number weighted mean cloud ice particle diameter was decreased since otherwise the tail of the  $D_{vi}$  PDF in Fig. 7d, which is also present in the observations, would be cut off. Other than this, the change does not have a discernible impact on the results presented in the manuscript. The reason for decreasing the temperature for which all water is assumed to be frozen from  $-35$  C to  $-40$  C is that the temperature for spontaneous freezing of small pure water droplets is probably closer to  $-40$  C than to  $-35$  C (e.g. Rodney et al., 1980). The effect on the results presented in the manuscript is, however, extremely small. Page 6385, lines 8–9: this change is discussed in the reply above. Page 6393, line 12-18: The reason for decreasing  $f_{adi}$  is stated in the same sentence. Regarding the erosion coefficients, for which different values are chosen in the 32 level version than in the 48 level version for the sake of radiative balance tuning, we added the following statement: "resulting in a net radiation flux at the top of the atmosphere that is fairly close to the one in the standard 48 level version of the model. Sensitivity studies with an earlier 48 level prototype version of the model and full chemistry have indicated that this does not qualitatively change the microphysics results presented in the manuscript."

17. The list of symbols and acronyms is now mentioned in the last sentence of the introduction (Sect. 1).  $f_{adi}$ ,  $q_v^{\max}$ , and  $T$  have been added to the appendix.  $K_h$  was changed to  $K_T$  in the text.  $D_{vol}$  was changed to  $D_{vi}$ .

18. The caption of Fig. A1 has been extended in the revised manuscript. It now reads:

"Schematic: Partially cloud covered grid box with new cloud formation caused by a change in saturation vapor pressure ( $\Delta q_s$ ) and under certain conditions also critical relative humidity  $RH_c$  (see text). C denotes the cloudy fraction and (1-C) the cloud free

C4001

fraction of the grid box at the beginning of the time step. Specific humidity ( $q$ ) equals saturation specific humidity ( $q_s$ ) in the cloudy part of the grid box and is assumed to be uniformly distributed around the environmental value  $q_e$  in the cloud free part of the grid box (diagonal line). Shaded areas represent increases in condensate mixing ratio due to a change in  $q_s$ . Adapted from Jakob (2000)."

19. We have included the simulated global averages of column integrated in-cloud liquid droplet numbers in Table 2 of the revised manuscript. They are 4.6 for the BASE and  $2.1 \times 10^{10} \text{ m}^{-2}$  for the NEW run.

**References not in the manuscript:**

Chuang, C. C., Penner, J. E., Taylor, K. E., and Grossman, A. S.: An assessment of the radiative effects of anthropogenic sulfate. *J. Geophys. Res.*, 102, 3761–3778, 1997.

Marchand, R, Mace, G. G., Ackerman, T, Stephens, G: Hydrometeor detection using Cloudsat - An earth-orbiting 94-GHz cloud radar, *J. Atmos. Oceanic Technol.*, 25, 519–533, 2008.

Murray, B. J., Wilson, T. W., Dobbie, S. Cui, Z., Al-Jumur, S. M. R. K., Moehler, O., Schnaiter, M., Wagner, R., Benz, S., Niemand, M., Saathoff, H., Ebert, V., Wagner, S., and Kaercher, B.: Heterogeneous nucleation of ice particles on glassy aerosols under cirrus conditions. *Nature Geosci.*, 4, 233–237, 2010.

Rodney J. A., Miller R. C., Kassner Jr., J. L., and Hagen, D. E: A Study of Homogeneous Condensation-Freezing Nucleation of Small Water Droplets in an Expansion Cloud Chamber. *J. Atmos. Sci.*, 37, 2508–2520, 1980.

Spichtinger, P., Gierens, K., Smit, H. G. J., Ovarlez, J., Gayet, J.-F.: On the distribution of relative humidity in cirrus clouds. *Atmos. Chem. Phys.*, 4, 639–647, 2004.

Wang, M., and J E. Penner, 2009: Aerosol indirect forcing in a global model with particle nucleation. *Atmos. Chem. Phys.*, 9, 239-260.

---

C4002

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 6375, 2010.

C4003