Atmos. Chem. Phys. Discuss., 10, C2348–C2352, 2010
www.atmos-chem-phys-discuss.net/10/C2348/2010/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 3 May 2010

The paper describes the development of a new stratiform cloud scheme in the GFDL AM3 GCM based on the Tiedtke cloud scheme and the two-moment bulk microphysics module of Morrison and Gettelman. The paper is very detailed enabling an evaluation of the results and a comparison with other cloud schemes. Unfortunately, due to the number of changes introduced in the model, it is difficult to pinpoint reasons for the differences in the simulations of the old and new model except when sensitivity runs are performed (WBF run). Major concerns I have only regarding the width of the w-PDF and the fact that droplet activation/ice nucleation happens only in the cloud free area, the latter maximizing the effect of heterogeneous ice nucleation. Nevertheless the model development and the validation with observational data are interesting and

the paper is well written. Therefore I recommend publication after some revisions.

Major comments:

Sigma_w is related empirically to the mixing coefficient for heat K_T. It is important to go into more detail at this point describing which processes are covered by this parameterization and it is certainly not enough to cite a paper that is still in preparation. I assume that w-variability due to temperature diffusion and convection are included. But is w-variability due to gravity waves included as well? This parameterization is central to the whole cloud parameterization and therefore needs to be transparent. It is furthermore important to give more information on the choice of sigma_min. In the ice nucleation section you argue that sigma_min should be 0.25 ms-1, a value that you get from observations. Why should observations give you an estimate of the minimum width of the w-PDF? It may rather be an average value. Choosing such a high sigma_min value I wonder how often sigma_w is equals sigma_min. A statistic could be useful in order to convince a reader that the exact choice of sigma_min is inconsequential. The fact that supersaturation statistics improve when sigma_w is increased would makes me wonder if a process in your sima_w parameterization is missing. I don't think increasing sigma_min is a good way out. It would be interesting to see a geographical distribution of sigma_w in order to get a feeling for the variable.

It is not clear to me whether cloud coverage, aerosol activation and homogeneous/heterogeneous freezing are forced by the same PDF_w with the same sigma_w. The text at the respective places sounds each time slightly different suggesting that for those processes different sigma_w are used which would make the parameterizations inconsistent.

Page 6381, line 10-11: '. . .. activation occurs only in newly formed cloudy fractions . . ..': Wouldn't this lead occasionally to very unrealistic microphysical properties. If at one time step only very few aerosols are activated due to a low sigma_w and at the next time step sigma_w is large then N would be strongly underestimated. The argument in lines14-16 holds only when N is large. In order to test if this argument holds the time scales of reduction of supersaturation due to condensation and that of increase of supersaturation due to w need to be compared.

Page 6389: As argued for aerosol activation, nucleating new ice particles only in the cloud free area, can lead to unrealistic microphysical properties of the clouds and maximize the influence of heterogeneous freezing. Imagine that at one time step only few crystals are formed via heterogeneous (or homogeneous) nucleation, those few particles can suppress any further ice nucleation in the model even if sigma_w is large. Therefore they would have a large impact that would not be found in nature. Can in this scheme heterogeneous and homogeneous freezing occur at the same time step?

Minor comments:

Page 6378 lines 23-25: do you mean to say that this is theoretically possible or that this has been shown to be the case. If you are talking about the latter, it would be good to cite the appropriate papers if you are talking about the former than please add an 'in principle' or the like.

Page 6386, line 4: It is true that Tompkins et al. set K=0.8. But since K is resolution dependent it is not obvious if this is the right choice for your model. Since I can't find any mention of the model's resolution I am unable to judge if the setting is reasonable.

Neither are heterogeneous freezing thresholds of aerosols known nor how many of the aerosol particles would actually act as efficient ice nuclei. Citing a paper that assumes a freezing threshold of 1.2 suggests that the freezing threshold is actually relatively well known. I think it is important to emphasize that those values are extremely uncertain and are likely to scatter over a large range of supersaturations depending on the aerosols, their aging process and on whether they are preprocessed. Assuming a particular threshold can only be regarded as a sensitivity study. Please also comment if you aerosol module is actually capturing these processes.

Page 6386 lines 9-11: I assume that RH_c is set to 1.2 only for heterogeneous ice nuclei

Page 6388 lines 26-27: shouldn't the critical number concentration above which only heterogeneous nucleation takes place depend on sigma_w?

Page 6389: A formula for N*_i,nuc should be given (Equation 15 is the ice nucleation equivalent of equation 3 but the ice nucleation equivalent of equation 1 is missing).

Page 6389 line 22: How is IN removal treated?

Page 6390 lines 7-8: Why do you multiply with 0.3 if about half of all dust is composed of efficient immersion nuclei?

How is r_eff_ice calculated? If equation 7 is used, what is k2?

Page 6397 lines 23 -24: The MOZAIC observations are for cloudy and cloud free air. The data can not be evaluated for only cloud free areas without further assumptions. Furthermore, the MOZAIC PDF should not be regarded a climatology, it certainly is biased to the European-North American area and by aircraft sampling issues.

Page 6397-8: Why isn't the spatial distribution of supersaturation compared with observations? It would be interesting to know if the spatial patterns of supersaturation fit with observations. Are differences New – Obs reminiscent of OLR differences in fig. 12?

Why do you only talk about the differences in the tropics when discussing figure 8? In the extratropics maxima of ice water content reach much lower levels in the model than in observations as well.

The budget plots are very small. A significant number of those lines seem not to be discussed at all. A reduction of the number of lines in figures 10 and 11 might make the main results more easily visible.

Page 6403 lines 14-18: What 'intricate interplay' are you talking about? Please explain or remove sentence.

At several places in the text you are calling your scheme a ‘modified Tompkins scheme’. This is confusing since you are not using the ‘Tompkins cloud scheme’ (Tompkins 2002) but the Tompkins et al. 2007 modification of the Tiedtke cloud scheme. Please change your terminology as ‘the Tompkins scheme’ is an established term.

In a few places in the paper (page 6384 lines 4-6; page 6385 lines 8-9; page 6393 line 12-18) it is mentioned that parameters have been changed compared to some previous setting but no comment has been added to the reasons of those changes, which settings are more realistic, the significance of the changes and even partly why those parameters are needed at all. I very much appreciate that the authors try to list all the steps in this major model development but it is very distracting. A few more explanations should be given and the authors might want to try putting this detail into a table.

Some symbols are not listed in Appendix B e.g. f_adi, K_H, the index ls is not explained

Fig. A1 needs to be described in more detail. In the current form it is not understandable.

Table 2: Would it be possible to include N in the table. Even though there are no observational estimates it would be certainly interesting.

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