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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cirrus clouds in a global climate model with a statistical cirrus cloud scheme" *by* M. Wang and J. E. Penner

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We are grateful for the evaluation of the reviewer, which has allowed us to improve and clarify the manuscript. Below we address each of the comments. The reviewer comments are in italics and our response is in bold.

Anonymous Referee #2

The manuscript describes the implementation and testing of a new cirrus cloud scheme in a global climate model (NCAR CAM3). The new scheme is based on the work of Karcher and Burkhardt (2008) and makes use of a PDF of subgrid ïňĆuctuations. With the new cloud scheme a series of sensitivity studies is performed with CAM3 and compared to the previous version and some observation. It is argued that the new *scheme leads to an improvement of the model compared to previous versions.*

The topic of the paper is timely and could be an important contribution to the question of aerosol-cloud-climate feedbacks.

The manuscript is well written and the methodology is described in detail. My main criticism is that the authors are not open enough about the many simpliïňĄcations they have to make, namely about their simpliïňĄcations concerning the co-existence of heterogeneous and homogeneous nucleation. I recommend to accept the manuscript after a major revision of the text, and maybe an additional sensitivity experiment to investigate the effect of pre-existing ice on homogeneous nucleation.

We clarified many simplifications in the revision, and section 5 is added to address some simplifications we made. A sensitivity test is added to investigate the effect of pre-existing ice from heterogeneous ice nucleation on homogeneous nucleation in section 5.2.

Major points:

- Any effect of pre-existing ice on homogeneous nucleation is neglected (e.g. page 13, 'heterogeneous IN concentration is assumed to have no effect on homogeneous freezing). This is an oversimpliïňĄcation which is wrong and to some extent unnecessary. At least a zeroth-order effect could be included, e.g., using Eq. (44) of Ren and McKenzie (2005, QJ, 131, pp. 1585-1605). I wonder why the authors did not consider this as an additional sensitivity experiment.

The effect of Heterogeneous IN on ice crystal number concentration is only neglected when the heterogeneous IN concentration is lower than N$_{in_cr}$. When heterogeneous IN concentration is larger than N$_{in_cr}$, heterogeneous freezing will dominate and homogeneous freezing will rarely happen. So the effects of heterogeneous IN are taken into account in this case. As we discussed in the manuscript (the last paragraph in p. 16621 in the ACPD manuscript and sec-

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tion 5.2 in the revision), our approach is reasonable at high heterogeneous IN concentrations (> N_{in_cr}) and low heterogeneous IN concentrations (<N_{in_cr}/10). We noticed that a similar approach is used in Lohmann et al. (2008), where heterogeneous freezing is only allowed in the grid points where IN concentration is larger than 1/L (10/L is used in their sensitivity test). They argued that this approach seems justified as homogeneous freezing and heterogeneous freezing may seldom occur simultaneously (Spichtinger and Gierens, 2009).

We added an additional simulation to explore how results change when the effects of heterogeneous IN on ice crystal number concentration from homogeneous freezing is taken account when $N_{in\_cr}/10 < N_{in} < N_{in\_cr}$ in section 5.2. We used a formula from Liu and Penner (2005), which is similar to that in Ren and McKensize (2005) to treat the effects of heterogeneous IN on nucleated ice crystal number concentrations from homogeneous freezing. Our test showed this effect can be important in some heterogeneous IN scenarios, and that this effect should be included in future studies of the anthropogenic aerosol effects on cirrus clouds.

- Most problematic in this regard is that the authors write the text as if there would be no co-existence of heterogeneous and homogeneous nucleation, and as if heterogeneous nucleation would have no effect on the number of ice crystals formed due to homogeneous nucleation. Often they do not make a clear distinction between their simpliïňĄcations used in the model and the processes as they occur in nature. For example, on page 14 the write that '[..] we use the critical heterogeneous IN concentration [..] to determine whether heterogeneous freezing will occur'. In nature, heterogeneous nucleation will always occur before the homogeneous nucleation is reached. What the author want to tell us is that they neglect heterogenous nucleation. So why not say it that way?

We clarified the effect of heterogeneous IN on ice crystal number concentrations dominated by homogeneous freezing in several places in the manuscript. Now

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the text regarding our treatment in section 2.2 reads: "In the real atmosphere, when the saturation ratio exceeds the freezing threshold saturation ratio, heterogeneous freezing occurs whenever heterogeneous IN are present. But given the large time step used in the NCAR CAM3 (30 min), if we allow heterogeneous freezing to occur whenever heterogeneous IN are present in the model, this will significantly decrease supersaturation levels in the model and result in very few homogeneous freezing events even when the concentration of heterogeneous IN is very low (< 1/L). To avoid this, we only allow heterogeneous freezing to occur when the heterogeneous IN concentration (N_{in}) exceeds a critical IN number concentration (N_{in_cr}) above which the ice crystal number concentration is determined by heterogeneous freezing (Gierens, 2003; Ren and Mackenzi, 2005; Liu and Penner, 2005; Kärcher et al., 2006; Barahona and Nenes, 2009). When the heterogeneous IN concentration is lower than N_{in_cr}, heterogeneous freezing is neglected in the model, and the heterogeneous IN concentration is assumed to have no effect on homogeneous freezing (issues regarding this assumption and the competition between homogeneous freezing and heterogeneous freezing are further discussed in Sect. 5.2).". **More discussion is also added in section 5.2.**

- It is claimed in the manuscript that the authors have implemented a consistent treatment of ice nucleation and cirrus clouds (e.g. page 33). Besides there oversimpliïňĄed treatment of ice nucleation, they do not advect the cloud fraction of cirrus cloud, and advected cloud ice may evaporated instantly when it is advected into a cloud free grid box (page 17). This is obviously a severe inconsistency leading to an overestimation of sublimation of cloud ice.

We added more discussion in section 5.3 to explain this simplification, and we acknowledged that our simplification can potentially impose a physical inconsistency between simulated cloud fraction, and the advected moisture and condensate fields, which may overestimate sublimation and affect relative humidity and ice crystal number concentrations in the upper troposphere. But given the large uncertainties in simulating cirrus clouds in global climate models, this simplifi-

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cation is still acceptable. We also noticed that many global climate models still have a long way to go to include a physically consistent treatment for all clouds.

It would be interesting to quantify the potential effects of our simplification, but this is difficult given the current model configuration. The main difficulty comes from the diagnostic treatment of cloud fraction in the warm and mixed-phase clouds (warmer than -35C). If cloud fraction is advected, the parameterization for warm and mixed-phased cloud formation would need to be changed, which is beyond the scope of the present work. We attempted to advect only the cirrus cloud fraction. But this introduced a major inconsistency in the boundary between mixed-phase clouds and cirrus clouds. When only cirrus cloud fraction is advected, cloud fraction can be added or subtracted from cirrus clouds, but the mixed-phase clouds in the adjacent grids are not subtracted or added because cloud fraction in mixed-phase clouds is diagnosed, mainly based on relative humidity. It is not clear whether the mixed-phase part is implicitly decreased or added to since the warming/cooling and drying/moistening associated with large-scale transport may change relative humidity and result in changes in cloud fraction in the mixed-phase clouds. To make the advection of cloud fraction physically consistent within the cloud scheme, a prognostic cloud fraction scheme across all clouds is needed. But this requires tremendous additional effort and is beyond the scope of this manuscript. A sensitivity test did suggest that advecting cloud fraction can be quite important. But given the inconsistency introduced when only cirrus cloud fraction is advected, we have to be cautious in explaining these results. We will revisit this issue when a prognostic cloud fraction treatment for other clouds is also available in the NCAR CAM.

Minor points:

- page 4: Repeated sentence in paragraphs 2 and 3 of this page. 'In these studies, the individual GCMs ...'. Please rewrite both paragraphs.

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This was a mistake in the version of the manuscript submitted for quick reviews, which was corrected in the version of the manuscript that appeared in ACPD. Sorry for the confusion.

- page 5: Typo 'Moreover,To'

Fixed.

- page 38, line below Eq. (A7): Maybe (A6) instead of (A5), and Eq. (10) instead of Eq. (8)?

Yes, that is correct, and fixed.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16607, 2009.

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