Atmos. Chem. Phys. Discuss., 12, C4599–C4603, 2012
www.atmos-chem-phys-discuss.net/12/C4599/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity studies of dust ice nuclei effect on cirrus clouds with the Community Atmosphere Model CAM5" *by* X. Liu et al.

Anonymous Referee #1

Received and published: 12 July 2012

General Comments:

The present manuscript discusses effects of mineral dust aerosol on cirrus through heterogeneous ice nucleation. Investigations based on global simulations with the CAM5 model are presented. Cirrus clouds are the most frequent cloud type in the upper troposphere. They have large effects on the Earth's radiation budget and are of large relevance for the climate system. The present knowledge on cirrus microphysical properties and the role of aerosols in cirrus formation is still very uncertain. Hence, the manuscript is of high relevance for atmospheric and climate science and is well suited as a contribution to ACP.

As an important result, the study reveals that homogeneous freezing activity is reduced due to competition with heterogeneous ice nucleation on mineral dust aerosol. The effect induces changes in cirrus microphysical properties and global mean cloud forcing. A clear advantage of the study is that uncertainties of these results are estimated by application of two different ice nucleation parameterizations and by comparison with measurements. The manuscript is clearly written and well structured. The applied methods are described thoroughly and, in most cases, the results obtained are discussed carefully.

Unfortunately, the statistical significance of the simulated effects of mineral dust shown in Figure 8 of the manuscript is not proven. In the case of Figure 9, it is not clear whether the differences between the model runs shown are larger than model noise. Since a fully coupled general circulation model is applied, the differences can also be due to feedbacks of the cirrus changes on model dynamics, rather than being directly related to cloud microphysical effects. To some extent, they could just be related to changed 'weather' and could average out if a longer, decadal simulation would be analysed. Even if the differences found by the authors are larger than the interannual variability of the discussed quantities they can still be affected by feedbacks. To cope with this problem, a robust statistical analysis of the aerosol-induced differences (e.g. by means of a student's t-test) is necessary. This needs to be urgently addressed in the paper.

The manuscript aims to discuss climate impacts (title of section 5). However, with fixed sea surface temperatures and a simulation of only five realizations (5-year runs), a climate impact cannot be quantified. The simulations can just be used to discuss perturbations of cloud microphysics and related radiative flux changes, which could trigger possible climate change. This however has to be demonstrated by means of longer-term coupled atmosphere-ocean simulations. I would suggest changing the title of section 5 (e.g. 'Atmospheric effects') and other formulations in the manuscript accordingly.

I recommend publication of the manuscript after the above comments as well as further comments listed below have been addressed by the authors.

Specific comments:

Page 13124, lines 5-6: I do not see that the mentioned parameterizations provide information about the ice crystal size distribution. In my understanding, the parameterizations provide initial ice crystal number concentrations which serve as input for the microphysical cloud scheme (2-moment) of the large-scale model. The formulations should be skipped/corrected.

Section 2.2: The description of the model representation of the Bergeron-Findeisen process is hardly understandable. It should be rendered more precisely or skipped, just referring to Gettleman et al. (2010).

Page 13126, line 17: It should be mentioned here that aggregation provides a sink for ice particle number only and conserves the ice water content, while the other sinks mentioned affect both quantities.

Section 3, paragraph 3: Mineral dust aerosol effects on cirrus are the major subject of this study. Hence, a more detailed description of the model representation of mineral dust cycles (sources, initial size distribution assumptions, sinks) would be appropriate here (or in section 2.1), rather than just referring to other literature.

Page 13134, lines 1-2: This should be re-worded carefully. Homogeneous freezing seems to dominate Ni, but this does not mean that heterogeneous nucleation is unimportant. It could still have important effects during many cloud events, especially at low cooling rates.

Section 4, comparison with MOZAIC data: It should be discussed whether the MOZAIC data could be biased towards cloud-free conditions since pilots might avoid passages through thick cirrus layers. It should also be discussed whether the all-sky model data could be biased towards low supersaturations owing to the humidity relaxation during

ACPD

12, C4599–C4603, 2012

Interactive Comment

the comparatively large time steps of large-scale models.

Page 13136, lines 2-4: ‘These comparisons suggests that homogeneous nucleation may play an important role . . .’. I do not see this from the comparison. The het simulations generate high concentrations (larger than 100/L) more frequently than observed and fit even better to the observations than the hom cases. Only for concentration between 10-100/L the BN-het case shows too low frequencies. Here homogeneous freezing seems to be very important. As a potential reason for the discrepancies between model and observations also possible shortcomings in the representation of cooling rates in the model should be mentioned.

Section 5, title: The title should be changed (e.g. ‘Atmospheric impacts’) since climate change cannot be simulated by means of the simulation set-up chosen (see general comments above). All other formulations about ‘simulated climate effects’ should be rephrased accordingly.

Section 5, Figures 8/9: It needs to be shown whether the differences presented in Figure 8 and discussed in section 5 are statistically significant (e.g. by means of a student t-test performed on the base of the results obtained for the individual model years). In Figure 9, the interannual variability should be indicated in order to enable a fair evaluation of the significance of the differences between the model runs shown. See also general comments above.

Page 13136, lines 25-27: Could the increase in stratospheric water vapour also be due to reduced sedimentation of clouds ice?

Section 5, last paragraph, table 2: It should be discussed whether the differences between the global mean numbers gained from the different model runs are larger than their interannual variation. Otherwise the differences could just be due to ‘model noise’. To proof significance appropriate statistical analysis is necessary here. Differences that are not significant should not be discussed.

ACPD

12, C4599–C4603, 2012

Interactive Comment

Conclusions: It should be assured that all results discussed are statistically significant.

Technical corrections:

Page 13123, line 25: include period/full stop behind approach.

Page 13125, line 24: replace ‘seas’ by ‘sea’.

Page 13126, line 2: explain MG08.

Page 13128, line 26: include ‘as’ before ‘in’.

Page 13129, line 5: include units behind 2.5x1.9.

Page 13131, line 22: replace ‘reduces’ by ‘reduce’.

Page 13133, line 16: include ‘the’ before ‘heterogeneous’.

Page 13134, line 5; and other parts of the manuscript: Replace ‘probability distribution frequency’ by ‘probability distribution’ or ‘frequency distribution’.

Page 13135, line 4: Explain DOE.

Page 13135, line 18: Explain SGP.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13119, 2012.