Atmos. Chem. Phys. Discuss., 9, C11616–C11623, 2010
www.atmos-chem-phys-discuss.net/9/C11616/2010/

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

M. Wang and J. E. Penner

minghuai.wang@pnl.gov

Received and published: 17 March 2010

We are grateful for the evaluation of Dr. Kärcher, 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.

B. Kärcher (Editor)

bernd.kaercher@dlr.de

I appreciate the effort by Drs. Wang and Penner (WP09) to increase the realism of GCM simulations of high cloudiness by introducing a physically-based approach to represent cirrus in the NCAR CAM. As discussed by Kärcher and Burkhardt (2008, Sect.5.1) (KB08), a number of possible cloud scheme methodologies may be applied

to this problem. WP09 opted to choose the approach offered by KB08, an initial step towards this goal. In certain aspects, however, the authors deviate from KB08.

The authors should comment on the following issues raised in this discussion thread.

1. Cloud fraction and model implementation

Regarding page 16611, lines 9-12, there has never been an inconcistency between diagnosing cloud fraction and predicting ice supersaturation in the ECMWF model. The model employed a prognostic cloud fraction before ice supersaturation was introduced.

The statement "For simplicity, the cloud fraction predicted here is not advected." (p.16625, l.17) imposes a physical inconcistency between simulated cloud fraction and advected moisture and condensate ïňĄelds and creates numerical artifacts that need to be identiïňĄed and removed. In the KB08 parameterization framework, cirrus cloud fraction should be advected.

More discussion of these issues has been added in section 5.3. Also see our answers to the reviewer 2. Our simplification can potentially impose a physical inconsistency between simulated cloud fraction, and advected moisture and condensate fields, which may overestimate sublimation and affect relative humidity and ice crystal number concentrations in the upper troposphere. But since condensate fields are sublimated when they advect into the clear sky part of an adjacent grid, and since our cloud fraction responds in a physical way to the increase or decrease in moisture due to its advection or sublimation, no numerical artifacts exist. Mass conservation is followed throughout the scheme.

Regarding p.16623, l.6-25: referring to eq.(10), please comment on why eqs.(22a,b) and (23) from KB08 have apparently not been adopted in this study.

We did use all these three equations in our study. Eq. (10) in our paper is just Eq. (22a) in KB08, and Eq. (22b) in KB08 is described in line 11-12 on p 116623 in the ACPD manuscript. As for Eq. (23) in KB08, it is described in line 4-5 on p.

116623 in the ACPD manuscript.

Regarding the discussion of simulated and observed cloud fraction (p.16630, l.2-14). ISCCP sensors detect cloud only above an approximate optical depth threshold âĹij0.2– 0.3. I wonder whether this is taken into account in producing the model cloud fraction shown in Fig.2 (bottom left). If not, to which degree does the choice of assumed optical depth thresholds affect this comparison?

Cloud fraction in Figure 2 includes all clouds, and it does not exclude those clouds with optical depth less than 0.3. Figure 2 shows the zonal-latitude distribution of simulated 3D clouds, and is not intended to be compared with 2D cloud fraction from ISCCP. Global total cloud fraction and high level cloud fraction are compared with ISCCP and HISR (the later is added in the revision), and more discussion is added in section 3 about the limits of observed cloud fractions. The total cloud fraction in the HOM case is 66% which is comparable with that observed from ISCCP and MODIS (65-67%), but is lower than that from HIRS (75%). The high level cloud fraction is 35%, which is comparable with that observed by HIRS (33%), and is larger than that from ISCCP (21%). HIRS measures more optically thin clouds (with a optical depth detection limit of around 0.1) than that from ISCCP (with a optical depth detection limit of around 0.3), and is more representative for high level clouds (Wylie and Menzel, 1999).

The authors should refer to details of how the prognostic cirrus fraction is combined with the diagnostic cloud fraction scheme used for liquid phase clouds. In which circumstances is the prognostic scheme applied (based solely on temperature or extended to the mixed phase cloud regime)? Ice crystals may sediment into lower levels where liquid cloud droplets preexist; how does cloud fraction change as a result of sedimentation both in the upper and lower levels?

The new cirrus cloud scheme described here is only applied to clouds with temperatures colder than -35C. The treatment of ice crystal sedimentation follows

that in the NCAR CAM3 (Bovil et al., 2006). Sedimenting particles evaporate if they fall into the cloud free portion of a layer. A maximum overlap is assumed, so particles only evaporate if the sedimenting particles come from a layer with a larger cloud fraction. Since our model does not treat partial cloud fraction in the vertical direction, sedimentation is assumed to change ice crystal number concentration only but not cloud fraction. This is now mentioned in last two paragraphs in section 2.2.

2. Mesoscale temperature fluctuations

The proposed empirical parameterization of mesoscale temperature fluctuation (MTF) mean amplitudes solely as a function of temperature disregards the physical nature of MTF. MTF are tied to unresolved gravity waves and any refined parameterization should be based on processes controlling their generation and propagation.

We acknowledged the issues associated with the simple treatment of mesoscale temperature perturbations in Section 5.1, and the need to further improve the treatment of these perturbations. A refined parameterizaiton to treat the mesocale temperature perturbation and the resulting cooling rate in a way consistent with the physics in the host GCM model is beyond the scope of this manuscript. However, we added a sensitivity test to see how the mesoscale temperature model from Gary (2006; 2008) affects the model results. The mesoscale temperature perturbation formula in Gary (2006; 2008) is based on the analysis of more than 4000 aircraft flight hours taken by the Microwave Temperature Profiler in the altitude range 7-22 km and with a variety of underlying topography, spanning the latitude range 70°S to 80°N. This formula showed a seasonal, latitude, topographic, and altitude dependence in observed mesoscale temperature perturbation. For example, mesoscale temperature perturbations are greatest over mountainous terrain, are greater at polar latitudes during winter and increase with altitude in a systematic way. The altitude dependence in the Gary formula is consistent with the gravity wave theory (Fritts and Alexander, 2003).

The two cited papers by Gary provide more information than suggested by WP09 (p.16618, l.24-28). The authors seem to argue in favor of aerosol effects in order to compensate for problems arising from the use of their MTF parameterization (p.16619, l.1-22). Indeed, the discussion on p.16635, l.2-13 points to a serious lack of consistency between relative humidity, temperature ïňĆuctuations, concentrations of heterogeneous ice nuclei (IN), and total ice crystal number densities, that needs to be addressed. This lack of self-consistency also overshadows much of the discussions on p.16622, l.6-26 and on p.16646, l.13-19.

The motivation for using this simple MTF parameterization was to get reasonable ice crystal number concentrations in the homogeneous freezing only case, and is not intended to argue in favor of aerosol effects. Our sensitivity tests showed that heterogeneous IN can affect the RHi distribution and the frequency of occurrence of supersaturation, especially at low levels (193 hPa), and improved the comparison with the MLS observations. This conclusion has little to do with our simple MTF parameterization. In the added sensitivity test (HOM_GARY), the frequency of occurrence of supersaturation simulated by using the Gary formula is almost the same as those simulated from our simple MTF treatment (HOM). This suggests that the frequency of occurrence of supersaturation at 139 hPa and 192 hPa in the homogeneous freezing only case is not as sensitive as ice crystal number concentrations to different mesoscale temperature perturbations. So we also removed the discussion in the last paragraph in section 3.

Our model does have a consistent treatment between relative humidity, mesoscale temperature perturbation, heterogeneous IN, and ice crystal number concentrations, but we do have a very simple mesosscale temperature parameterizaiton, as we acknowledged in section 5.1.

3. Heterogeneous ice nucleation

WP09 state that the number of ice crystals formed by heterogeneous ice nucleation is

not as sensitive to changes in vertical velocity as those from homogeneous freezing (p.16620, l.13-15). This only holds if total number of available IN limits the number density of ice crystals, ni. In other cases, the vertical velocity dependence of ni is similar to homogeneous freezing (Kärcher and Lohmann, 2003).

Our statement is for the case where heterogeneous freezing dominates because this is applied only to the heterogeneous freezing part of the clear sky part of a grid, when heterogeneous IN concentration exceeds the critical heterogeneous freezing. We clarified this in the revised text, and now it reads: "The ice number from heterogeneous freezing is not multiplied by a factor of 2, since the factor of 2 applied for homogeneous freezing is primarily used to account for the nonlinear dependence of ice crystal number concentrations on vertical velocity and since the number of ice crystals formed in an environment dominated by heterogeneous freezing is not as sensitive to changes in vertical velocity as those in an environment dominated by homogeneous freezing."

Field data do a good job of constraining the possible number density of IN in the troposphere. Therefore, the scenario HMHT1 IN (Sect. 2.3) seems to be unrealistic as it causes a predominance of (midlatitude) heterogeneous ice formation that is not consistent with observations (DeMott et al., 2003; Haag et al., 2003). This also affects previous results discussed in Liu et al. (2009) and Penner et al. (2009), reporting rather large longwave effects due to cirrus changed by IN. The Hendricks et al. (2005) study dealt with the potential effects of IN, but did not derive RF changes caused by IN.

HMHT_1IN is presented here as a sensitivity test to see how heterogeneous IN will affect clouds and radiative fluxes if they represent 100% of the total dust and soot aerosol particles. We did not argue in the text that this is a realistic case.

The treatment of competition of different aerosol particles during ice formation is parameterized by WP09 with a threshold IN concentration above / below which pure ho-

mogeneous / heterogeneous ice formation occurs (p.16621, lines 14-16). This approximate approach does not allow to simulate the basic effect of IN, namely the reduction of ni created from fully soluble particles in the presence of IN.

The effects 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 account under these circumstances. 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).

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. Our test showed this effect can be important in some heterogeneous IN scenarios, and this effect should be included in future studies of anthropogenic aerosol effects on cirrus clouds.

Concerning the discussion of the relative humidity statistics, Haag et al. (2003) distinguish between inside and outside of clouds using large particle number densities from CVI measurements and found a dependence of the relative humidity threshold dividing clear sky and cloudy data points on this distinction. How do WP09 deïňĄne their data points as being "outside of cloud", and how does this choice affect their results (a sensitivity study would be useful)?

We described in detail how we sampled the model data to compare with MOSAIC relative humidity and with that from MLS data in Section 3. For the comparison with MOSAIC data, the PDF of the clear-sky RHi based on Eq. (A2) in the model is used. Since a uniform RHi distribution is assumed in the cloudy sky, we did

not include this part of the RHi in our comparison. As we discussed in the text, this part of RHi is close to 1 because of the large time step used in the model. Including the cloudy sky RHi would bias the RHi distribution toward 100%. For this reason, it is difficult to explore how the simulated clear-sky RHi depends on the threshold ice crystal number concentration used to define the cloudy part of the grid box.

Given the above issues with IN and MTF, I don't think the assertion p.16646, l.16-19 is robust.

As we discussed above, the conclusion has little to do with our simple MTF parameterization. The frequency of occurrence of supersaturation at 139 hPa and 192 hPa is not sensitive to different mesoscale temperature perturbations. This is now discussed on the last paragraph of section 3 and the third paragraph of section 5.1. This conclusion also has little to do with our treatment of the effects of heterogeneous IN. Our treatment takes account of the basic effect of heterogeneous IN on humidity fields, and the fact that heterogeneous freezing will deplete water vapor earlier and lead to less supersaturation in clear sky. This explains why the heterogeneous IN decreases the frequency of occurrence of supersaturation at 192 hPa.

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