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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

B. Kärcher (Editor)

bernd.kaercher@dlr.de

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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 inconsistency between simulated cloud fraction and advected moisture and condensate fields and creates numerical artifacts that need to be identified and removed. In the KB08 parameterization framework, cirrus cloud fraction should be advected.

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.

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 $\sim$0.2–0.3. 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?

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?

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.

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 fluctuations, 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.

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, n_i. In other cases, the vertical velocity dependence of n_i is similar to homogeneous freezing (Kärcher and Lohmann, 2003).

Field data do a good job of constraining the possible number density of IN in the troposphere. Therefore, the scenario $HMHT_1IN$ (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.

The treatment of competition of different aerosol particles during ice formation is parameterized by WP09 with a threshold IN concentration above / below which pure homogeneous / 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 n_i created from fully soluble particles in the presence of IN.

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 define their data points as being "outside of cloud", and how does this choice affect their results (a sensitivity study would be useful)?

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

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