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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

Anonymous Referee #1

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General Comments

The authors present results of global climate model simulations of cirrus clouds in which aerosol effects and sub-grid scale distributions of temperature and in-cloud total water are considered. Novel aspects of the work include coupling homogeneous and heterogeneous nucleation parameterizations with a previously published statistical cloud scheme and simulation of these clouds in a model with both prognostic aerosol and cloud parameterizations. The results indicate a complex dependence of ice cloud microphysics and relative humidity distributions on the competition between heterogeneous and homogeneous nucleation as well as the assumed magnitude of sub-grid scale temperature fluctuations.

This is a highly ambitious work that is not overly successful or convincing. The simu-

lations are highly dependent on numerous uncertain processes notably the simulation of aerosols in the upper troposphere, the ice nucleating properties of these aerosols, the assumed sub-grid scale distributions of temperature, and the sub-grid scale vertical velocity used in the homogeneous nucleation parameterization and its relationship to temperature anomalies. It was not clear to me the value of adding the statistical cloud scheme when significant compromises were made (see next paragraph). While I recognize that a lot of work went into the present manuscript and there are some positive results in their simulations, there are still such large uncertainties in many areas of parameterization that the present manuscript can only be viewed as a step along a long path towards improved model simulations of aerosol-ice-cloud interactions rather than the end point itself.

The text is overly long and needs serious editing.

Specific Comments

A significant inconsistency is that they are unable to simultaneously have realistic sub-grid scale temperature perturbations and ice crystal concentrations in the tropical tropopause layer cirrus. They choose a sub-grid scale vertical velocity of 1.2 cm/sec at a temperature of 193K in order that their parameterization of homogenous nucleation results in a reasonable ice-crystal number concentration. However, their parameterization that relates vertical velocity and temperature anomalies (Equation 4; omega = 8.2 dT) implies a mesoscale temperature anomaly of 0.05K for this vertical velocity and temperature. This small temperature anomaly is at odds with the known characteristics of gravity waves that produce larger temperature anomalies at higher altitudes. At this point, one should question the validity of the relationship between vertical velocity and temperature fluctuations (Equation 4) as well as recognize that the believability of their simulations is compromised. The authors should more prominently acknowledge the large dependence of their simulations on this inconsistency and that what is needed is a more convincing model for the joint sub-grid scale distribution of temperature and vertical velocity. Furthermore, the abstract and conclusions should admit the uncertain nature of their results due to the dependence on this and other uncertain parameterizations.

As for the comparison of the simulations to observations, the results are mixed. The upper-troposphere relative humidity distributions appear favorable although the comparison of ice crystal concentrations to in-situ observations is not so favorable. Some comparisons to observations fail to mention observational uncertainty that can be very high for ice cloud properties. Specific uncertainties they should mention include the difficulty of measuring small ice crystals from in-situ probes and the inability of ISCCP to see most thin cirrus (HIRS might be better). It also appears that the authors do not include the model snow fields in their comparisons to observations (MLS & in-situ) which they should do because the observations do not distinguish between snow and ice.

I found unconvincing their discussion of how ice cloud changes impact low clouds. In the abstract, they claim that increased sublimation of settling ice crystals leads to greater lower level humidity and thus more clouds. Later at the end of section 4.1.2, they claim that smaller ice crystals lead to longer cloud lifetimes that lead to more evaporation and “more moisture is transported to the lower atmosphere”, which then leads to greater low clouds. These explanations do not appear to be consistent as smaller ice crystals would lead to less sublimation of settling ice crystals and less low-level humidity by the first argument, but more low-level humidity by the second argument. More importantly, no evidence is presented that shows the changes in cloud lifetime, cirrus sublimation rates, or the rate of sublimation of settling ice crystals. Another possible mechanism that might explain your results is that more high-level clouds warm the upper troposphere and stabilize the atmosphere to convection. With less convection and less precipitation, more water vapor is accumulated in the lower troposphere which leads to increases in low cloud. Because you do not present analysis of the mechanisms that could affect low clouds, you should acknowledge that your explanations for changes in low clouds are only speculations. The consistency of your results with the

mechanisms in Wu, Grabowski and Sanderson is unclear without further analysis.

The writing of the paper also needs substantial improvement. The paper is overly long. I did not find the discussion of the actual balance of cloud forcing changes between longwave and shortwave effects to be important or useful. The paragraphs that begin (“the simulated net cloud forcing is more complex.”) and (“the moistening effect of ice crystal gravitational settling on the lower atmosphere has been recognized for a long time”) could be deleted with no major impact on the paper. (Here there is a flaw in logic in that the authors assume that the high cloud changes cannot impact significantly the shortwave cloud forcing just as much as low clouds can.) Much of the last two paragraphs of the conclusion section could also be removed. In the introduction, the paragraphs that begin (“Global models have been used recently to study the effect of homogeneous and heterogeneous nucleation on cirrus cloud properties.”) and (“In recent years, global models have been used to study the effect of homogeneous and heterogeneous nucleation on cirrus cloud properties”) are redundant.

Technical Corrections

Section 2.1. “who” should be inserted between “(DAO)” and “participated”. Section 2.2. “evaporation” should be “sublimation” in the paragraph that begins “In the new cirrus cloud scheme”. Section 3. “Appendix 4.B” should be “Appendix B”? Section 3 & Figure 1. Why not show the observed estimates of LWP? The agreement between model and observed LWCF is not that great, particularly between 30 and 50 degrees latitude. Section 3 & Figure 5. Why not add the Kramer et al. data to the figure? Section 4.1.2. In order to demonstrate the relative importance of heterogeneous and homogeneous nucleation could not you compute the number of crystals nucleated through each nucleation method? Section 4.1.2. “compare Figure 3c and Figure 3b”. These figures look nearly identical to me, except in the Arctic. Thus I don’t see the difference you are talking about here. Section 4.1.3. Why not show the latitudinal and height distributions of temperature and relative changes in humidity? This could be interesting. Section 4.2. Last paragraph. Rather than contrasting the impact of changes in assumed temperature fluctuations and ice nuclei on cloud forcing, you should highlight the relative magnitude of changes in ice crystal concentrations and effective radii. That seems more significant to me. Equation B7. What is the symbol “f”? Is it cloud fraction? If so, shouldn’t it be “a”? Table 4. Why can’t you calculate the initial ice crystal concentrations for the experiments other than HOM?

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