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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 #1

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 simulations 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.

We agree with the reviewer that our effort is only a step forward rather than the end point itself. The modeling of aerosol-ice-cloud interactions in global climate models is still in its infancy and large uncertainties remain. In fact, one of the coauthors (M. Wang) will be supported to continue this cirrus modeling work in the latest version of NCAR CAM at Pacific Northwest National Laboratory. An additional section (5) has been added to discuss some issues associated with our treatment raised by reviewers.

The text is overly long and needs serious editing.

We edited the text following the suggestion by reviewers.

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.

We agree with the reviewer that a decrease in vertical velocity at low temperatures is questionable; or even incorrect. We also agree that we need a more convincing model for the joint sub-grid scale distribution of temperature and vertical velocity. But to develop this kind of sophisticated model to treat the sub-grid scale distribution of temperature and vertical velocity is beyond the scope of this manuscript, and will be explored in a future study. More discussion of mesoscale temperature perturbations is added in section 5.1, and an additional sensitivity test is used to explore how a different mesoscale temperature perturbation model from Gary (2006; 2008) will affect our results, and the issues are mentioned in the abstract.

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).

Cloud fraction from HIRS has been added in Table 2, and more discussion is added in section 3. Now the text about simulated cloud fractions reads: "The total cloud fraction is 66% which is comparable with that 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)."

For the in-situ measurement of ice crystal number concentrations, we add the instrument limit on ice crystal size. For the INCA campaign, the PMS FSSP-300 optical particle counter and the PMS 2D-C probe measured particles with a size range of 3 μm to 800 μm in diameter. For data used from Kramer et al. (2009), an FSSP 100 or 300 is used to measure ice crystal number concentration. The FSSP 100 and FSSP 300 sample particle in the size range of 1.5-15 and 2-20 μm in diameter, respectively. As shown in Kramer et al. (2009), at least 80%, but typically 90% of total ice crystal number concentration is within the FSSP size range. This is added to the manuscript in section 3.

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.

For uncertainties in satellite observations of ice clouds, Waliser et al. (2009) had

an excellent review, and we refer readers to that paper for more details. In our model, snow is diagnosed from cloud fields, but is not carried forward in time, so that it is assumed to fall out instantaneously. Therefore, it is not possible to include snow fields in the comparison to observations. As discussed in Waliser et al. (2009), MLS tends to saturate for cloud systems that have significant amounts of large frozen hydrometers and thus tends to only reflect distributions that are more characteristic of cloud ice alone and is, therefore, appropriate to be compared with the cloud ice simulated in the model. As for in-situ comparisons, since only hydrometer number concentration is compared, and the number concentration of snow is typically much smaller than that of cloud ice crystals, not including snow crystal number concentration has little effect on the comparison. This is now mentioned in section 3.

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.

Sorry for the confusion. What we meant in the manuscript was the second argument (smaller ice crystals lead to longer cloud lifetimes that leads to more evaporation and more moisure is transported to the lower atmosphere), and we thank the reviewer for providing an alternative explanation. After further analyzing our results, we believe that changes in convection are the reason for changes in lower clouds. This is supported by the decreasing convective precipitation rate with increasing ice crystal number concentrations (Table 3). Our results are also consistent with Jakob (2002). Now the text reads: "The changes in ice crystal number concentration and size also affect liquid clouds. Simulated liquid water path and low level cloud fraction are always positively correlated with column-integrated ice crystal number concentration in all our simulations (Table 2). For example, when ice crystal number concentration increases from the HMHT_0.1IN case to the HMHT_1IN case, the liquid water path increases by 9% from 75.3 g/m^2 to 82.0 g/m^2. Our results here are consistent with those of Wu (2002), Grabowski (2000), Jakob (2002) and Sanderson et al. (2008). Wu (2002) and Grabowski (2000) found that a model with low ice crystal fall velocities would produce a more cloudy and moist lower troposphere with less precipitation. The increasing low level clouds and decreasing precipitation in our simulations are associated with decreasing convective activity, as is evident from the decreased convective precipitation rate (Table 3). For example, when ice crystal number concentrations increase from HMHT_0.IN to HMHT_1IN, the convective precipitation rate decreases from 1.96 mm/day in HMHT_0.1IN to 1.82 mm/day in HMTH_1IN. This is consistent with the findings of Jakob (2002) found. Jakob (2002) showed that a smaller settling velocity leads to less convective precipitation and more liquid clouds. Less convective activity is likely caused by the increased heating in the upper troposphere and decreased surface insolation from increased ice crystal number concentrations and decreased size."

The writing of the paper also needs substantial improvement. The paper is overly long.

A lot of editing has been done in the revision.

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.

We think it is important to discuss the balance between the shortwave effect and longwave effect, because this will help us to understand why the net change in cloud forcing is not sensitive to changes in ice crystal number concentrations. We cited Chen et al. (2000) to show that high cloud changes have a larger impact on longwave cloud forcing than that on shortwave cloud forcing, and we did not argue that the high cloud change cannot impact significantly the shortwave cloud forcing just as much as low clouds can. Still, we shortened the two paragraphs in section 4.1.3 into one paragraph, and deleted the last paragraph in section 4.1.3, and shortened the two paragraphs about radiative fluxes in the summary section into one paragraph.

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.

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

Technical Corrections

Section 2.1. "who" should be inserted between "(DAO)" and "participated".

We clarified the sentence and now it reads:"The mass-only version of the IMPACT aerosol model driven by meteorological fields from the NASA Data Assimilation Office (DAO) was included in the AEROCOM (http://nansen.ipsl.jussieu.fr/AEROCOM/) phase A and B evaluations (Kinne et al., 2006; Textor et al., 2006; Schulz et al., 2006), where it has been extensively compared with in situ and remotely sensed data for different aerosol properties"

Section 2.2. "evaporation" should be "sublimation" in the paragraph that begins "In the new cirrus cloud scheme".

Thanks for pointing this out. This is corrected in the revision and now the sentence reads:"while the specific humidity in the cloudy part of the grid box is used to determine whether vapor deposition or sublimation occurs and is used to determine how much cloud fraction decreases in the case of sublimation."

Section 3. "Appendix 4.B" should be "Appendix B"?

Yes, and we corrected this in the revision.

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.

LWP over ocean from SSM/I is added in Figure 1, and the underestimation in longwave cloud forcing between 30 and 50 degree is noted in the text.

Section 3 Figure 5. Why not add the Kramer et al. data to the figure?

The Kramer et al. data is added into Figure 5.

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?*

We do have the number of ice crystals nucleated from each nucleation method, but in order to demonstrate the relative importance of each nucleation mode, we also need cloud fraction increases from each nucleation method. Unfortunately, we did not separately track cloud fraction increase from each nucleation method. To get cloud fraction increase from each method, we will have to rerun all our simulations and that will be too expensive. In section 4.1.2, we used relative humidity fields to indicate which freezing mode dominates. The relative humidity fields can be used to demonstrate the relative importance of the two freezing modes.

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.

We clarified this. Now the text reads: "One example is shown in Fig. 3. When ice crystal number concentrations decrease from the HOM case to the HMHT_0.01IN case, ice crystal radius increases, which increases the gravitational settling and decreases the ice water simulated over the Arctic in HOM, thereby improving the comparison with MLS observations (compare Fig. 3 c and Fig. 3b)."

Section 4.1.3. Why not show the latitudinal and height distributions of temperature and relative changes in humidity? This could be interesting.

The latitudinal and height distributions of temperature change and relative changes in humidity from the HOM case to the HMHT_0.1IN case are plotted in Figure 11.

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.

We added the relative magnitude of the change in column-integrated ice crystal concentrations in the last paragraph of section 4.2. Now the text reads:"The magnitude of the changes in column-integrated ice crystal number concentration and radiative fluxes from different mesoscale temperature perturbations are comparable to those simulated from different heterogeneous IN concentrations (Sect. 4.1.3). For example, a 25% decrease in the temperature perturbation from the HMHT_0.01IN case to the HMHT_0.75dT case leads to a 44% decrease in column-integrated ice crystal number concentration, a change in the net cloud forcing of -0.22 W/m^2, and a change in the net TOA radiative flux of -0.54 W/m^2, while a factor of 10 increase in the heterogeneous IN concentration from the HMHT_0.01IN case to the HMHT_0.1IN case leads to 47% decrease in column-integrated ice crystal number concentration, a net cloud forcing of 0.32 W/m^2, and the net TOA radiative fluxes of -0.43 W/m^2. This points to the importance of mesoscale dynamics and subgrid scale variations in studying aerosol indirect effects on cirrus clouds (Haag and Kärcher, 2004; Penner et al., 2009)"

Equation B7. What is the symbol "f"? Is it cloud fraction? If so, shouldn't it be "a"?

Yes, it is cloud fraction, and it should be "a". The same is true for Equation B8. Both are corrected in the revision.

Table 4. Why can't you calculate the initial ice crystal concentrations for the experiments other than HOM?

The initial ice crystal concentrations for the experiments other than HOM could be calculated as the average of ice crystal concentrations nucleated from each nucleation mechanism weighted by individual cloud fraction increase due to each nucleation mechanism. Unfortunately, we did not separately track cloud fraction increase from heterogeneous and homogeneous freezing in our output, though we do have nucleated ice crystal concentration from each nucleation mechanism.

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