We are grateful to the evaluations of the reviewers, which have allowed us to clarify and improve the manuscript. Below we addressed the reviewer comments, with the reviewer comments in italic and black, and our response in bold and blue.

Anonymous Referee #1

Received and published: 18 February 2011

Wang et al. study aerosol indirect effects in a so-called multi-scale modeling framework (MMF) in which a 2-D cloud resolving model (CRM) is embedded in each vertical column of a GCM and serves as a cloud parameterization. Cloud effects on aerosols are parametrized using the so-called Explicit-Cloud Parameterized-Pollutant (ECPP) hybrid approach for aerosol–cloud interactions (Gustafson et al., 2008). In the ECPP approach horizontal statistics (e.g., cloud mass flux, cloud fraction, and precipitation) from the CRM simulation are used to drive a single-column parameterization of cloud effects on the aerosol and then the aerosol profile is used to simulate aerosol effects on clouds within the CRM. Droplet activation is calculated at each CRM grid cell, using vertical velocities w from the relatively high resolution CRM grid and a parametrization of sub-grid scale variability of w.

There are several potential drawbacks to this approach. In particular, vertical velocities differ greatly between 2-D and 3-D CRMs, it is not clear how well aerosol-cloud dynamics interactions can be represented in 2-D CRMs, and the horizontal resolution of the CRMs is still fairly coarse. Furthermore, the spatio-temporal correlation (e.g. due to vertical transport and scavenging) between aerosol concentration and cloud occurrence can not be well represented in the MMF since large-scale (GCM) horizontal advection always requires horizontal averaging, even if CRMs were placed in North- South as well as East-West-direction. Although Gustafson et al., (2008) suggest that the latter might not be such a big problem after all, in my opinion this point does warrant some further study in the future.

In spite of these potential drawbacks, the paper by Wang et al. represents the best that has thus far been done on a technical level in order to address two important issues with respect to simulating the indirect effects of anthropogenic aerosols related to liquid clouds on the global scale: The MMF approach takes into account the effects of aerosols on deep convection and to some still fairly uncertain extent might in the not so distant future also allow to better take into account feedbacks between stratiform cloud-dynamics and aerosols in a global model. These issues are at present extremely difficult to address in traditional GCMs. Next to this technical achievement, the paper yields a large number of very interesting results. It definitely constitutes a significant contribution to the study of aerosol-cloud interactions.

I strongly recommend to publish this paper in ACP with only minor revisions, and encourage additional future improvements of this still very new tool as suggested by the authors (especially with respect to the representation of boundary layer clouds) as *well as additional future sensitivity studies in order to better sort out which processes, assumptions, and/or possibly also tuning parameters might play a role in determining the sensitivity to anthropogenic aerosols in the present study. I would also like to encourage the authors to briefly mention the most important sensitivities of the new model as far as they are already aware of them. Subtle differences in model formulation can sometimes play a rather large role and it would definitely be good for the reader to get an idea about which might be the most important assumptions and how important these assumptions are.*

One relevant point could perhaps be that the pre-industrial (PI) sulfate source in CAM5 (Table 2) is much smaller than in the MMF model for reasons which are largely unrelated to the difference between the multi-scale framework and a more traditional global model. This difference in PI sulfate source might contribute to the higher sensitivity of the cloud optical properties to anthropogenic emissions in CAM5 even beyond what is suggested in the abstract.

The difference in PI sulfate source is partly caused by the difference in the treatment of SO_2 wet removal (stronger in CAM5), as discussed in Section 3.2. In a sensitivity test (reduced-SO2-wet-removal), we applied the same wet scavenging treatment of SO_2 as that in the MMF to CAM5. Simulated PI sulfate source in CAM5 increases from 15.24 Tg S/yr to 19.37 Tg S/yr. We updated the text and now it reads (Sect. 3.2, P. 13, l. 11-19): "The lower sulfate burden in CAM5 is partly from a larger wet removal rate, as evident from its shorter lifetime, and partly from smaller sulfate production (42.5 Tg S/yr in CAM5 vs. 59.8 Tg S/yr in the MMF). The latter is caused in part by differences in SO_2 wet removal. In the MMF, SO_2 wet removal occurs only by rain (which results in little removal below freezing), and the SO_2 solubility is based its effective Henry's law equilibrium at pH 5. In CAM5, SO_2 wet removal occurs by all precipitation, and the SO_2 uptake follows that of H_2O_2. Both differences produce stronger SO_2 wet removal in CAM5. In a sensitivity test (reduced-SO_2-wet-removal), the same wet scavenging treatment of SO_2 as that in the MMF is applied to CAM5. This increases the PD sulfate burden in CAM5 by 28%.".

Differences in the treatments of SO_2 cloud chemistry and convective transport, which affect the relative strengths of SO_2 loss processes (dry and wet removal versus aqueous- and gas-phase conversion to sulfate), are another reason for the difference in PI sulfate sources between the two models. The lifetime of SO_2 decreases by 14% from PI to PD in the MMF, while it increases by 20% in CAM5. The large increase in SO_2 lifetime from PI to PD in CAM5 can be partly explained by the positive feedback in aerosol indirect effects induced by cloud lifetime effects as we explained in the manuscript. This is now added in Sect. 4.2.2 (P. 20, l. 27-32; P. 21, l. 1-5): "The smaller relative increase in CCN concentrations in the MMF is also partly caused by the smaller relative increase in sulfate sources in the MMF (Table 2). The sulfate source increases by 130% from PI to PD in the MMF, while it increases by 178% in CAM5. This is partly caused by the changes in the lifetime of SO_2, which decreases by 14% from PI to PD in the MMF, while increases by 20% in CAM5. The differences in changes in sulfate source and

SO_2 lifetime from PI to PD result from differences in the treatment of wet removal of SO_2 (less efficient wet removal of SO_2 in the MMF), differences in the magnitude of cloud lifetime effects from aerosols (weaker cloud lifetime effects in the MMF) and differences in the treatment of aqueous chemistry (a fixed pH value of 4.5 in the MMF versus a SO_4-dependent pH value in CAM5) (Wang et al., 2011; Liu et al., 2011, in preparation) .".

We do not expect the difference in sulfate burden to affect our major conclusions in this study, as we discussed in Section 4.2.2 (P. 21, l. 6-12): "The higher sulfate burdens in the MMF versus CAM5 do not appear to contribute significantly to different LWP response to a given CCN perturbation and the stronger indirect effect in CAM5. In the reduced-SO_2-wet-removal sensitivity test (see the discussion in Sect. 3.2 about the sensitivity test and the differences in the treatment of SO_2 wet removal between the MMF and CAM5), reducing SO_2 wet removal has little effect on simulated LWP response to a given CCN perturbation and slightly increases aerosol indirect forcing in CAM5, even though it increases the PD and PI sulfate burdens by 28% and 40%, respectively. ". **(Also see our answers to Question #2, #12 from Review#1 and Question #6 from Reviewer#3).**

Specific comments:

1. p. 3400, l. 9-10: "explicit simulation of aerosol/cloud interactions": Cloud microphysics and the effects of clouds on aerosols (transport, scavenging etc.) are still parameterized. Instead, one could perhaps write "allows for a better representation of aerosol/cloud interactions".

We modified this, and now the text reads (P. 1, l. 18-20): "The extension allows a more physically-based treatment of aerosol/cloud interactions in both stratiform and convective clouds on the global scale in a computationally feasible way.". **Accordingly, we replaced '**explicitly**' with '** more physically-based**' across the manuscript.**

2. p. 3400, l. 12: "The much smaller increase in LWP in the MMF is caused by a much smaller response in LWP to a given perturbation in cloud condensation nuclei (CCN) concentrations from PI to PD": I think that the very different PI sulfate burdens might also play a role.

This statement is based on the scatter plots of the relative change in LWP versus the relative change in CCN from PI to PD (Figure 16 a and Figure 16 b). These scatter plots show that the relative change in LWP from PI to PD in the MMF is about three times that in CAM5 for a given relative change in CCN from PI and PD. So it is about LWP response to a GIVEN relative CCN perturbation, and has little to do with PI aerosol levels. However, the PI aerosol levels, or more specifically, the difference in aerosol burden between PI and PD, does play a role in simulating different aerosol indirect forcing in the MMF and CAM5. This is stated in the second half of that sentence: 'to a lesser extent, by a smaller relative increase in CCN concentrations from PI to PD in the MMF**'. Also see our answer to Question #12.**

3. p. 3401, l. 19-20 and also p. 3400, l. 1-3: could you perhaps cite a paper that deals with influences of anthropogenic aerosols on large-scale dynamics in the introduction?
We added Rosenfeld et al. (2006) and Wood et al. (2011) as examples there. And the text reads (P. 2, l. 25-26): '‘Much of this uncertainty arises from the multi-scale nature of the interactions between aerosols, clouds, and dynamics (e.g., Rosenfeld et al., 2006; Wood et al., 2011)’.

4. p. 3403, l. 25: is this the final version of CAM5?
It is based on a developmental version of CAM5 from May 2009. Since then, CAM5 underwent tuning and debugging regarding its turbulence, cumulus clouds, stratiform cloud microphysics and macrophysics parameterization. These changes are not relevant to the MMF work, since in the MMF model, the turbulence and cloud parameterizations in CAM5 are replaced by the embedded CRM in the MMF. Given the constant updates in CAM5 (after its official release in last June, 50 additional new tags with a variety of updates and bug fixes have been released), it is unrealistic for us to keep updating CAM5 in the MMF model with the latest CAM5 release since updating the CAM5 in the MMF is a big task. We are working with our collaborators at NCAR and Colorado State University to merge our codes with the latest CAM5 as we expect to see a more stable version of CAM5 in next a couple of months. The CAM5 results are based on the final version of CAM5 (tag: cam5_0_00).

5. p. 3404, l. 10: are the original versions of the two Morrison et al. microphysics schemes used in the SAM CRM and in the CAM5 or have there been important parameter changes?
The original versions of the two schemes as documented in the publications were used. For the SAM CRM, the scheme documented in Morrison et al. (2005; 2009) was used. For CAM5, the original scheme of Morrison and Gettelman (2008) with modifications described by Gettelman et al. (2010) was used. Small tuning changes were made for the release version of CAM5, but these parameter changes were minor.

6. p. 3404, l. 26: "The vertical velocity for calculating droplet activation is related to the resolved vertical velocity and the turbulence kinetic energy, with a minimum vertical velocity of 0.1ms−1" -> How exactly is this done (reference?) and do you know how sensitive the main results are to this choice?
This is described in detail in Wang et al. (2011). The vertical velocity for calculating droplet activation is the sum of the resolved vertical velocity on the CRM grid and a subgrid vertical velocity that is diagnosed from the CRM's turbulent kinetic energy. The addition of the sub-grid vertical velocity is important to simulate reasonable cloud droplet number concentrations, and the choice of the minimum vertical velocity is less critical, as the choice of 0.1 m/s or 0.01 m/s does not change results much. The text is updated, and now it reads (P. 5, l. 17-24): "The vertical velocity for calculating droplet activation is the sum of the resolved vertical velocity and a sub-grid vertical velocity that accounts for the unresolved motion and is diagnosed from the turbulence kinetic energy

(Wang et al, 2011). A minimum vertical velocity of 0.1 m s^{-1} is set, following Ghan et al. (1997). Short-term sensitivity tests show that including sub-grid vertical velocity is critical to simulate reasonable cloud droplet number concentrations compared with observations, while the choice of the minimum vertical velocity is less critical".

7. p. 3405, l. 15: "The ECPP approach uses statistics of cloud distribution, vertical velocity, and cloud microphysical properties resolved by the CRM to drive aerosol and chemical processing by clouds on the GCM grid": what are the statistics of vertical velocity used for? If I understand it right, activation is calculated on the CRM grid and transport requires mass fluxes?

Yes, you are right, the transport is done using mass fluxes, and aerosol activation related to cloud droplets is done on the CRM grid. However, aerosol wet scavenging is done in the ECPP, where the statistics of vertical velocity are used to calculate the fraction of aerosols that is activated and transferred from interstitial aerosols to cloud-borne aerosols. This is discussed in Wang et al. (2011). The difference between aerosol activation on the CRM grids and on the ECPP is discussed in Wang et al. (2011) and the text reads (Wang et al., 2011, P. 143, the 2nd paragraph): "Aerosol activation calculated in ECPP, which affects the aerosols, is distinct from the activation calculated in the CRM microphysics scheme (Sect. 2.2), which affects droplet number. In the CRM microphysics scheme, the vertical velocity at each CRM grid point and the GCM grid-cell mean aerosol concentrations are used for activation, while in ECPP, the subclass vertical velocities and aerosol concentrations are used. Though the treatment of activation in ECPP is somewhat inconsistent with that in the CRM, they are in fact coupled since ECPP uses updraft statistics from the CRM, and the CRM uses aerosol statics from ECPP. Some inconsistency is inevitable due to the 'parameterized' aspects of the Explicit-Cloud-Parameterized-Pollutant approach." **Since the ECPP approach is documented in detail in Wang et al. (2011), and the current manuscript is already quite long, we refer readers to Wang et al. (2011) for those details.**

8. p. 3405, l. 17: "explicitly treat the effects of convective clouds on aerosols in [a] computationally feasible manner" -> see my comment regarding p. 3400, l. 9-10

This statement is modified, and now the text reads (P. 6, l. 8-10): 'allows us to treat the effects of convective clouds on aerosols in **a** more physically-based and computationally feasible manner.'.

9. p. 3406: are the results sensitive to choosing different episodes for the MMF and the CAM simulations, or would they be essentially the same if only the 34 of the CAM simulations were analyzed which were also simulated using MMF? How sensitive are the results to choosing such a short averaging period?

Both the MMF and CAM5 run with fixed SST and sea ice, which make the interannual variability of global means quite small in these simulations. This is especially true for the annual mean results, on which most of our major conclusions are based. For example, the slopes between the relative change in LWP and the relative change in CCN in CAM5 (Fig. 17c) change little when we varied the averaging time from 2 years to 4 years (the slopes are 0.30 for the

four-year average, 0.29 for the three-year average, and 0.30 for the 2-year average). Another example is the changes in shortwave cloud forcing from anthropogenic aerosols. The global annual mean values are -1.79, -1.78, and -1.76 W m^{-2} for 4 years, 3 years, and 2 years averaging, respectively. It would be desirable to have longer integration for the MMF results. However, the MMF is an expensive model (more than 100 times slower than CAM5), and we were constrained by available computing resources from running longer simulations. We do expect to have longer integrations for the MMF model that will be used in future studies (also see our reply to Question #2 from Reviewer #3). The following discussion is added in Section 5 (P. 28, l. 22-32): "We note that a short integration time (34 months) is used for the MMF model in this study. The MMF model is an expensive model, and we are constrained by the limited computing resources to have longer simulations for this study. Since the simulations are driven by fixed climatological sea surface temperature and sea ice, the interannual natural variability of global means is quite small in these simulations and we do not expect our results to be sensitive to the short integration time. This is especially true for the annual mean results, which are the basis for most of the major conclusions presented here. For example, the slopes between the relative change in LWP and the relative change in CCN in CAM5 (Fig. 16b) vary little when we varied the averaging time from 24 months to 48 months (the slopes are 0.306 for the 24-month average, 0.290 for 34-month average, 0.286 for 36-month average, and 0.304 for the 48-month averages).".

10. p 3408 l. 14: "It is also close to that retrieved from Cloud-Sat (around 80 gm−2), and MODIS (60 gm−2), but is much larger than estimates from ISCCP (around 35 gm−2) and NOAA NESDIS (around 10 gm−2) (Fig. 18 in Waliser et al., 2009)" -> also cite original references? CloudSat is 75±30gm−2

Waliser et al. (2009) did a very nice job in overviewing different ice cloud products from satellite observations and associated issues. We think it is appropriate to refer readers to Waliser et al. (2009) for a summary of IWP values derived from different satellite observations, since this will not only point authors to the original references provided in Waliser et al. (2009) but also point to issues in individual satellite products discussed in Waliser et al. (2009). As for the original references, they typically documented the product details, but did not produce the global value of IWP cited here. Nevertheless, we added the original reference for CLOUDSAT (Austin et al., 2009), and now the text reads (P. 8, l. 23-28): 'It is also close to that retrieved from CloudSat (75±30 g m^{-2}, from Austin et al., 2009), and MODIS (60 g m^{-2}), but is much larger than estimates from ISCCP (around 35 g m^{-2}) and NOAA NESDIS (around 10 g m^{-2}) (IWP from MODIS, ISCCP, and NESDIS can be found in Fig. 18 in Waliser et al., 2009). CloudSat retrievals are sensitive to large hydrometeor particles and are considered to be more representative of total frozen water (Austin et al., 2009; Waliser et al., 2009).'. **Accordingly, we updated Table 1 with the numbers and references.**

11. p. 3408, l. 24: "These differences may result from the differences in the microphysics schemes in the CRM components in the two MMF models." -> could it be *due to different densities of graupel and snow resulting in different fall speeds?*

The density of snow and graupel used in the two MMFs are the same. The density of snow is 0.1 g m^{-3} while the density of graupel is 0.4 g m^{-3}. We do note, however, the density of cloud ice in the PNNL MMF is smaller than that in the NASA MMF (0.5 g m^{-3} in the PNNL MMF vs. 0.92 g m^{-3} in the NASA MMF). The smaller density of cloud ice is consistent with the slightly smaller contribution of cloud ice to the total ice water path in the PNNL-MMF. It is still not clear why the PNNL-MMF produces much smaller contribution by graupel. These discussions are added, and now the text reads (P. 9, l. 4-10): 'These differences may result from the differences in the microphysics schemes in the CRM components in the two MMF models. For example, the CRM component in the NASA fvMMF employs a single-moment bulk microphysics scheme (Tao et al., 2003), while the two-moment microphysics scheme is used in the PNNL MMF. Though the density of snow and graupel are the same in both MMFs (snow: 0.1 g m^{-3}; graupel: 0.4 g m^{-3}), the density of cloud ice is smaller in the PNNL MMF (0.5 g m^{-3} vs. 0.92 g m^{-3} in the NASA fvMMF)'. **(Also see our answer to Question #7 from Reviewer #3).**

12. p. 3413, l. 4 ff: do you have an idea on how strongly these different assumptions with respect to SO2 wet removal influence your results regarding the anthropogenic aerosol effect?

The following discussion is added in Sect. 4.2 (P. 21, l. 6-12): "The higher sulfate burdens in the MMF versus CAM5 do not appear to contribute significantly to different LWP response to a given CCN perturbation and the stronger indirect effect in CAM5. In the reduced-SO_2-wet-removal sensitivity test (see the discussion in Sect. 3.2 about the sensitivity test and the differences in the treatment of SO_2 wet removal between the MMF and CAM5), reducing SO_2 wet removal has little effect on simulated LWP response to a given CCN perturbation and slightly increases aerosol indirect forcing in CAM5, even though it increases the PD and PI sulfate burdens by 28% and 40%, respectively..".

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13. p. 3418, l. 28: "longwave warming": this sentence refers to FLNT (positive upward) which has been decreasing from the PI to the PD simulations (Table 1). A decrease corresponds to a cooling which is due to the fact that changes in aerosols have been taken into account, but not changes in greenhouse gas concentrations. The sentence on p. 3418, l. 28 does not refer to LWCF.

We clarify this by changing sign of upward longwave fluxes to be negative in Table 1, which makes it consistent with the sigh of shortwave fluxes (positive means incoming), and a footnote is added in Table 1.

14. p. 3428, l. 29 to p. 3429, l. 1: "In contrast, cloud LWP decreases with increasing AOD over ocean, which is opposite to CAM5 and many other aerosol-climate models." - as a future study, it would be interesting to try to better understand the details of the possible causes for this. For example, next to the plausible explanation of different scavenging formulations, the different PDFs of vertical velocity in oceanic and continental clouds could also play some role, as could some other factors.

We agree with the reviewer that it would be interesting to better understand the difference between the MMF and other models. We are working on examining the interactions between aerosol, clouds, and precipitation in the MMF model, and will report our results in a separate paper. Also see our answer to Question #13 from Reviewer #2.

15. Table 1: the PD net radiative imbalance at the top of the atmosphere is greater than 2Wm−2 in CAM5 and in MMF (which is fairly large, but in my opinion o.k. for the purpose of this study). Have the models been tuned to yield a similar imbalance, and if then how?

We did not tune the PNNL-MMF. It is an expensive model, and we do not have the computing resource for the tuning. As for the 2 W m^{-2} imbalance at the top of the atmosphere, as pointed out by the reviewer, it is of little consequence for this study since the model is not coupled with the ocean model, and the SST is fixed in our experiments.

16. Table 1: The CloudSat IWP is 75±30 gm−2. Maybe also take into account upper limits given by error bars from CERES. Where possible, cite papers either instead of or in addition to WWW-sites.

We added the total IWP ranges from the CLOUDSAT, and also cited papers in addition to WWW-sites in the revised manuscript. As for the radiative fluxes, we updated numbers according to Loeb et al. (2009).

17. Fig. 6: The generally higher droplet number concentrations over the ocean in MMF appear to be in line with a weaker anthropogenic aerosol effect in MMF.

Figure 6 shows cloud-top droplet number concentrations in PD. It is not necessarily the case that high droplet number concentrations will lead to weaker aerosol indirect effects, since aerosol indirect effects also depend on droplet number concentrations in PI. Comparing Figure 12 with Figure 6, we can see that although cloud droplet number concentrations are larger over the ocean in the MMF in PD, the difference between PI and PD is also larger in the MMF. As we pointed out in the paper, the weaker anthropogenic aerosol effects in the MMF are caused by the smaller response in LWP to a given CCN perturbation, and, to a lesser extent, by a smaller increase in aerosol burden from PI to PD.

18. Fig. 13: In MMF at 20N there is an increase in LWP and a decrease in cloud top droplet effective radius from PI to PD. Can you explain, why at 20oN the change in SWCF is nevertheless positive?

We examined the zonal plot of cloud fraction (not shown), and the positive change in SWCF at 20N is caused by a decrease in cloud fraction. This is added into Section 4.2.1, the third paragraph.

19. Figs. 13 and 14: It looks like there might be some synoptic scale changes involved in the responses to aerosol changes. Did you look into these? Do you think the results could be influenced by deficiencies in the representation of low clouds?

Some of the features of the spatial patterns in the difference between PD and PI can be caused by natural noise due to the short integration time. However, the annual global mean changes are quite consistent, and we can still see a similar signal even if we use only one-year mean. The deficiency in representation of low cloud can certainly affect the results, but we do not have any idea about to what extend it may affect our results. This is why we acknowledge the need to improve low clouds in the abstract: 'Further improvements in the representation of ice nucleation and low clouds are needed to refine the aerosol indirect effect estimate'**. We are working on testing a higher-order turbulence scheme in the PNNL-MMF and the results will be reported in a future study.**

20. Fig. 14: Does a map of _SWCF yield a similar pattern of change as LWP in the northern sub-tropics?
We examined the change in SWCF between PI and PD (PD-PI) for both the MMF and CAM5. The pattern of the change in SWCF is quite similar to that of LWP in both the MMF and CAM5 in the northern sub-tropics. This is especially true for the MMF model, where it produces positive change in SWCF over subtropical North America and South Asia.

Technical comments:
p. 3400, l. 8 "within each grid cell" -> within each vertical column of the GCM grid
Corrected.

p. 3401, l. 25: "As implemented in most GCMs, cloud lifetime effects assume that," -> In most GCMs, it is assumed that sentence starting
Done.

p. 3401, l. 27 "increasing cloud droplet number concentrations from anthropogenic aerosols always slows": concentrations -> concentration (or else: slows -> slow)
Corrected.

p. 3411, l. 5: -0.5 -> -50.5
Corrected.

p. 3411, l. 19: rate -> rates
Corrected.

p. 3414, l. 2: show -> shows
Corrected.

p. 3418, l. 18: 0.53 vs. 0.52 in the table
Corrected. It is 0.53 in the Table 3.

p. 3418, l. 19: 2.10 vs. 2.11 in the table
Corrected. It is 2.11 in the text.

p. 3418, l. 25: "Aerosol effect" -> The aerosol effect
Corrected.

p. 3419: In the standard version of CAM5, simulated PI to PD changes in shortwave cloud forcing, changes in longwave cloud forcing, aerosol direct effects in the clear sky (assuming entirely clear grid boxes), and total aerosol effects are −1.79, 0.37, −0.45, and −1.66Wm−2, respectively. better change to: In the standard version of CAM5, the simulated PI to PD change in shortwave cloud forcing is -1.79Wm−2, the change in longwave cloud forcing is 0.37Wm−2, the aerosol direct effect in the clear sky (taking into account entirely clear grid boxes) is -0.45Wm−2, and the total aerosol effect on top of the atmosphere net radiation is -1.66Wm−2.
Thanks for these changes, and we updated the manuscript following the suggestion.

p. 3420, l. 25: contribution -> a contribution
Corrected.

p. 3421, l 20: in -> in the
Corrected.

p. 3425: evaporate -> evaporated
Corrected.

p. 3427, l. 2: clear-sly->clear-sky
Corrected.

Table 1, caption lines 10–11: "radiative fluxes at the top of the atmosphere" -> net radiative fluxes at the top of the atmosphere
Corrected.

Table 1, caption lines 2–3: CDLLOW -> CLDLOW
Corrected.

Table 1, CAM5 (PI) CLDHGH: 37.4
Updated. It is 37.40.

Table 2: could you include vertical spaces between each two lines to make it more readable?
Done.

Fig. 11: please increase the size of the x-axis labels.
Done.

Fig. 13: the caption says: "and shortwave net flux at the top of the atmosphere (FSNT) from anthropogenic aerosols in both the MMF (red lines) and CAM5 (blue lines)

simulations" while the figure title in (f) suggests that both, short- and longwave are included in the net.

It is the total net flux (shortwave+longwave), and it is now corrected, and the caption reads: " and net total flux at the top of the atmosphere (FSNT+FLNT)".

Anonymous Referee #2

Received and published: 24 March 2011

General Comments:

This is a study on aerosol indirect effect based on results simulated using an aerosol-coupled multi-scale modeling framework (MMF). The aerosol-coupled MMF is a novel and innovative tool for modeling the aerosol-cloud interaction phenomena and then its simulated results are unprecedented. In this paper, the authors describe general features of the model's results regarding the aerosol-cloud interaction in comparison with traditional climate model (CAM5) and satellite observations. I think the results shown here are worth publishing although I have several specific concerns listed below. I'm especially concerned about a lack of discussion on relationships and/or consistencies among some parameters (e.g. relationship between cloud fraction and cloud radiative forcing, and consistency between the models and satellite observations regarding the CCN concentration and the cloud droplet number concentration; see below for details). If the authors appropriately address these concerns, I would recommend publication of this paper in ACP.

Major Point:

1. P.3405, L.15-17: I don't understand how the ECPP approach treats the interstitial aerosols and cloud-borne aerosols separately in the framework where aerosols are represented only at GCM grid. Although readers should refer to Wang et al. (2010), can you briefly explain this?

The activation of the interstitial aerosols into cloud-borne aerosols and the resuspension of cloud-borne aerosols into interstitial aerosols are treated using the mass fluxes and vertical velocity statistics from the CRM model for each transport class (updraft, downdraft, or quiescent) in the ECPP. This activation is not the same as the aerosol activation in the CRM model, as we explained in Wang et al. (2011): 'Aerosol activation calculated in ECPP, which affects the aerosols, is distinct from the activation calculated in the CRM microphysics scheme (Sect. 2.2), which affects droplet number. In the CRM microphysics scheme, the vertical velocity at each CRM grid point and the GCM grid-cell mean aerosol concentrations are used for activation, while in ECPP, the subclass vertical velocities and aerosol concentrations are used. Though the treatment of activations in ECPP is somewhat inconsistent with that in the CRM, they are in fact coupled since ECPP uses updraft statistics from the CRM, and the CRM uses aerosol statics from ECPP. Some inconsistency is inevitable due to the 'parameterized' aspects of the Explicit-Cloud-Parameterized-Pollutant approach." **We refer readers to Wang et al. (2011) for these details.**

2. P.3408, L.27-28: Why is the ice crystal number concentration in MMF larger than in CAM5 even though the heterogeneous ice nucleation process is omitted in MMF?

In the upper free troposphere, heterogeneous ice nuclei (IN) concentrations are typically low, while sulfate particles, on which homogeneous nucleation initiates, are abundant. Including heterogeneous freezing on heterogeneous IN in cirrus clouds can reduce the supersaturation levels and therefore reduce the frequency of homogeneous freezing, which typically leads to low ice crystal number concentrations. This is the case in CAM5. We have the following discussion in the text (P. 9, l. 16-24): "In CAM5, sulfate can form ice crystals in cirrus clouds through homogeneous freezing, and dust can act as heterogeneous ice nuclei in cirrus and mixed-phase clouds (Gettelman et al., 2010). Large uncertainties exist in simulated column-integrated ice crystal number concentrations in global climate models (ranges 0.1-0.7×10^{10} m^{-2} in Lohmann et al. (2008); 0.02-0.09×10^{10} m^{-2} in Wang and Penner (2010)). Including heterogeneous nucleation in cirrus clouds generally leads to lower column-integrated ice crystal number concentrations, in better agreement with observed ice crystal number concentrations in the upper troposphere (Wang and Penner, 2010). Aerosol effects on ice nucleation in the MMF will be the subject of a future study.".

3. P.3409, L.11-17: Although the numbers for cloud fractions and radiative forcing are listed here, I don't understand how they are related to each other. Can you explain or discuss their relationships?

Cloud radiative forcing depends on not only cloud fraction, but also in-cloud cloud optical depth. Cloud optical depth depends on cloud water path and cloud droplet number/ice crystal number. Liquid water path in the MMF is larger than that in CAM5 (55.9 g m^{-2} in the MMF vs. 48.4 g m^{-2} in the CAM5). Column-integrated ice crystal number concentration in the MMF is about twice that in CAM5, which leads to smaller ice crystal effective radius in the upper troposphere in the MMF (see. Fig. S1). Both of these can partly explain why the shortwave cloud forcing is similar in two models and the longwave cloud forcing is larger in the MMF though the MMF produces smaller cloud fraction. We clarify this in the text, and it now reads (P. 9, l. 27-32; P. 10, l. 1-2): "Shortwave cloud forcing is -50.5 W m^{-2}, which is in the observed range (-47 to -54 W m^{-2}) and is close to that in CAM5 (-50.1 W m^{-2}). The larger LWP in the MMF model can partly explain why the MMF and CAM5 produce similar shortwave cloud forcing despite that cloud fraction in the MMF is smaller. Simulated longwave cloud forcing is 26.0 W m^{-2}, slightly smaller than ERBE (30 W m^{-2}) and CERES (29 W m^{-2}) observations, and is larger than that simulated in CAM5 (21.9 W m^{-2}). The larger longwave cloud forcing in the MMF is partly caused by higher ice crystal number concentrations in the MMF, which leads to smaller ice crystal effective radius in the upper troposphere (not shown).". **Also see our answers to Questions #3 and #4 from Reviewer #3.**

4. P.3409, L.28 – P.3410, L.3: Although the authors state that "Rain formation over the high latitudes is likely dominated by warm collision-coalescence processes and drizzle from low clouds", the warm rain processes should be important over tropics rather than high latitudes. Is your statement correct?

In high latitudes, cloud formation is mainly caused by large-scale cooling or moistening, but not strong convective motion. So rain formation is likely dominated by warm collision-coalescence processes and drizzle. This can lead to high particle number concentrations and small rain drop size. In contrast, over tropics, most rain formation is from deep convective clouds. So rain is generated from the freezing process in the deep convective clouds, which leads to larger rain drop, but low rain drop concentrations. We clarified this, and now it reads (P. 10, l. 13-18): "As cloud formation over the high latitudes is mainly through large-scale cooling or moistening, but not strong convective motions, rain formation over the high latitudes is likely dominated by warm collision-coalescence processes and drizzle from low clouds rather than melting from graupel and snow, which explains why rain mass mixing ratios are low but rain droplet number concentrations are high over the high latitudes.".

5. P.3410, L.17-18: What is the reason for this threshold value (1gm-2) that defines the cloudy column? Please provide any references if any.

This threshold value is used to exclude the very thin clouds that may not be seen by satellite data. Cloud optical depth (Tau) can be estimated from LWP and droplet effective radius (Reff), based on the following formula: Tau = 3*LWP/(2*rhow*Reff), where rhow is water density. So the threshold value of 1 g m^{-3} for LWP roughly corresponds to a cloud optical depth of 0.03-0.1 with cloud droplet effective radius varying from 15 micrometer to 5 micrometer. In comparison, the threshold cloud optical depth ISCCP can detect is 0.3. The threshold we applied is slightly smaller than the detection threshold of ISCCP. This is a place where we can make further improvement in the future. A better way to compare the model results with satellite data will be to use instrumental simulators to mimic how satellite instruments sample the atmosphere. We are working on running an offline COSP simulator package that includes a suite of satellite simulators to process the MMF ouput. The results from these comparisons will be reported in future studies. The following discussion is added in the end of that paragraph (P. 11, l. 9-15): 'We note that the threshold LWP of 1 g m^{-2} roughly corresponds to a cloud optical depth (τ) of 0.03-0.1 with droplet effective radius (R_{eff}) varying from 15 to 5 μm, based on the following cloud optical depth formula: $\tau=3LWP/(2R_{eff}\times\rho_w)$, where ρ_w denotes the liquid water density. This threshold cloud optical depth is smaller than the detection threshold of the ISCCP, which is about 0.3. In future studies, we will apply instrumental simulators to assure a fairer comparison between models and satellite observations (e.g., Marchand and Ackerman, 2010).'.

6. P.3410, L.20-27 and Figure 3: It would be desirable to provide low, middle and high cloud fractions separately from MMF in comparison with ISCCP to understand the relationships between cloudiness and cloud radiative forcings (shortwave and longwave) shown in Figure 4. It would also be useful to compare the results with corresponding statistics from CAM5.

As we discussed above (see our answer to Question #3), cloud forcing depends

not only on cloud fraction, but also on cloud water path and droplet/ice crystal effective radius. So cloud fraction alone may not explain all the differences among models and observations. Fig. S2 shows zonal-mean plots of low, middle, high cloud fractions, shortwave cloud forcing, and longwave cloud forcing in the models and observations (observations are from ISCCP for cloud fraction, and from CERES for shortwave and longwave cloud forcing). This figure does not help much in understanding the differences in cloud forcing among models and observations, so we decided to not include it in the text. As we acknowledged in the text and abstract, further improvements are needed for low clouds. We are working on testing a higher-order turbulence scheme to improve the simulation of low-cloud fraction in the MMF model, and the results will be reported in a future study.

7. Figures 3 and 4: It would be desirable to show zonal mean latitude-pressure cross sections of cloud fractions and radiative forcing to make it easy for readers to identify similarities and differences between MMF and satellite observations.

Though CloudSat does provide the latitude-pressure cross section of the frequency of hydrometers, this frequency of hydrometers includes the contribution from larger hydrometers, such as rain, and can not be directly compared with cloud fraction simulated in the model. To have a fair comparison between the models and CLOUDSAT, it is better to run instrumental simulators. We are working on this, and the results will be reported in future papers. As for the radiative forcing, it has only two dimensions (longitude and latitude), and it does not have the vertical dimension.

8. Figure 6: How is the cloud-top number concentration computed from MODIS retrievals? Do you assume adiabatic model?

The cloud-top droplet number concentration from MODIS is calculated by Quaas et al. (2006), assuming an adiabatic model. This information is added into the paper, and now the text reads: 'The satellite data is derived from version 4 of the MODIS by Quaas et al. (2006), assuming adiabatic clouds.'.

9. P.3413, L.21-23: "Simulated aerosol number concentrations in the MMF are higher than that in CAM5 and agree better with observations." What observations do you refer to here? Do you claim that the aerosol number concentration in the MMF is more realistic than in CAM5 here?

That statement is based on Figure 7, the aerosol size distribution observed in the marine boundary layer from Heintzenberg et al. (2000). We do not claim that aerosol number concentrations in the MMF are more realistic than in CAM5 in general.

10. P.3414, L.2-4 and Figure 9: How is the supersaturation computed in the models? Does this invoke the Abdul-Razzak-Ghan parameterization? Can you briefly explain how to compute the supersaturation here?

The CCN concentration at 0.1% supersaturation shown in Figure 9 is

diagnosed directly from size-resolved aerosol composition predicted in the model, based on the Kohler theory. This is not related to the supersaturation computed in the CRM model.
The supersaturation in the CRM for droplet activation is diagnosed from size-resolved aerosol compositions and vertical velocity, based on the Abdul-Razzak-Ghan parameterization of supersaturation.

11. Figures 6 and 9: It is obvious that larger CCN concentrations in MMF than CAM5 (Fig. 9) corresponds to larger CDNC in MMF than CAM5 (Fig. 6) and that the CDNC in CAM5 (Fig. 6 middle) is closer to MODIS (Fig. 6 bottom) than in MMF (Fig. 6 top). The authors, however, seem to claim that the aerosol and CCN concentrations in MMF are closer to observation than CAM5. That is confusing. How do the authors make a consistent picture between the CCN concentrations and CDNC for the models and observations? Otherwise, are they inconsistent or still puzzling?
It is true that CDNC over oceans in CAM5 is closer to MODIS than in the MMF. However, CDNC over land in MMF is closer to MODIS than in CAM5. As for the CCN concentrations and aerosol concentrations in the MMF, we do not claim that they are closer to observations than CAM5 in general. Figures 7 and 8 show the improvement in the MMF in simulated aerosol number concentrations in marine boundary layers, and aerosol concentrations in the polar regions, respectively. So we do not really see inconsistency between these different comparisons. What is more, it is also very challenging to establish consistent pictures among different observations given the different spatial and temporary coverage (e.g., MODIS data does not cover polar regions; field observations only cover a particular period, while MODIS has continuous observations over years) of different data and uncertainties in both ground observations and satellite data. The observational data sets used in the paper have been used in many previous studies and are thought to be valuable dataset for evaluating model performances, though those observations are typically used separately in different studies. Here we present these comparisons together to have a broader viewer of our model performance. It is not our intention to establish the consistent pictures among different comparison, but rather to present the differences among the MMF, CAM5 and observations. The following discussion is added in Sect. 3.2 (P. 14, l. 11-18): "We note that cloud-top droplet number concentrations over the oceanic regions in CAM5 are lower than those in the MMF and agree better with the MODIS observation (Fig. 6 in Sect. 3.1), which seems not consistent with the comparison shown in Fig. 7. However, the spatial and temporary coverage between satellite and field observations is different (e.g., field observations only cover a particular period, while MODIS has multi-year continuous observations). Moreover, the aerosol size distribution observations in Fig. 7 were made near the surface, while cloud droplet number concentration from MODIS is at cloud top. These differences make it challenging to establish consistent pictures among different comparisons.".

12. Figure 16: I don't understand what the difference between blue and red curves is.

Can you explain it more clearly?

We clarify this in the figure caption (it is Fig. 17 now): "blue lines are for the relative changes in the cumulative PDF of LWP; and red lines are for the relative changes in the cumulative LWP. The cumulative PDF of LWP (*cumu_PDF*) at a given LWP bin (*LWP'*) is: *cumu_PDF*= $\int_{LWP_{\min}}^{LWP'} PDF(LWP)dLWP$, where *PDF(LWP)* is the PDF of LWP, and LWP_{min} is the smallest LWP bin. The cumulative LWP (*cumu_LWP*) at a given LWP bin (*LWP'*) is the averaged in-cloud LWP from the smallest LWP bin (LWP_{min}) to the given LWP bin: *cumu_LWP*= $\frac{1}{cumu\_PDF}\int_{LWP_{\min}}^{LWP'} PDF(LWP)*LWPdLWP$. ".

13. P.3423, L.23-25: "the much smaller increase in LWP in the MMF is caused primarily by the much smaller response in LWP to a given change in CCN" Is this consistent with the sensitivity analysis in Figure 11b? Figure 11b shows that the MMF sensitivities of LWP to aerosol amount shown in Fig. 11b have opposite (positive and negative) values over land and ocean, and it looks like that these opposite tendencies tend to cancel each other to provide small response in LWP to CCN change when merging the land and ocean analyses. Is this interpretation correct? Even when this interpretation is correct, the sensitivities over land (Fig. 11b) are larger in MMF than in CAM5. Does this imply that the anthropogenic response of LWP is also larger in MMF than CAM5 when limited to land area? I'm wondering how the results in Fig. 11b and Figs. 17b,c are consistent.

This is a good point. It was our original intention to use Fig. 11 to gain insights about the differences in simulated indirect forcing between the MMF and CAM5 and about different LWP response to a given CCN perturbation. After exploring this for a while, we realized that the approach we used in Figure 11 may not be appropriate for this purpose. LWP is largely controlled by dynamical factors rather than aerosols. So the slope of ln(LWP) and ln(AOD) or ln(AI) is not necessarily an indicator of aerosol effects on LWP, and it is difficult to separate the aerosol effects from non-aerosol effects on the slope of ln(LWP) and ln(AOD). This is why we do not see a good correlation between the ln(LWP)/ln(AOD) and aerosol indirect forcing in nine global climate models included in Quaas et al. (2009) (see their Figure 2 and Table 3). One example is the LMDZ-INCA model. Though it has the smallest cloudy-sky forcing, the ln(LWP)/ln(AOD) is not that small. This may point to the limitations of the approach used in Figure 11. This may also explain why it is difficult to get a consistent picture from Figure 11 and Figure 16. We are working on other methods in examining the relationship between aerosols, clouds, and precipitation, and the results will be reported in a future study. Since the method used in Figure 11 has been applied in several previous studies, we feel it is still interesting to the community to report the MMF results, though it is challenging to explain the results. We added the following discussions about the consistency in section 5 (P. 28, l. 9-21): "We note that the differences in slopes between ln(*LWP*) and ln(*AOD*) in the MMF and CAM5 (Fig. 11) are not consistent with the differences in aerosol indirect forcing and the LWP response to a given CCN perturbation in the MMF and CAM5 (Fig. 16 a-b). The slopes between *ln*(*LWP*) and *ln*(*AI*) show large differences over land and ocean (Fig. 11), while the slopes between the relative changes in LWP and the relative changes in CCN from PI to PD are similar over both land and ocean (not shown), close to the values shown in Fig. 16. While the slopes between *ln(LWP)* and *ln(AI)* over land are larger in the MMF than in CAM5, the slopes between the relative change in LWP and the relative changes in CCN from PI to PD over land are much smaller in the MMF than in CAM5 (not shown), similar to the differences shown in Fig. 16 a-b. This may point to the limitations of the approach applied in Fig. 11. As LWP and aerosol burden are coupled by many non-microphysical factors (e.g., swelling effects of aerosols by clouds, and large-scale convergence), it is difficult to separate the microphysical factors from the non-microphysical factors in the slopes of *ln(LWP)* and *ln(AI)* in PD, which makes it not a good indicator for cloud lifetime effects from aerosols.".

Minor Point:
There still may be a lot of grammatical errors in the manuscript. I would recommend to thoroughly check the text to make sure that all of the errors and/or typos are corrected.
Thanks, and we thoroughly checked the text and corrected those errors/typos found.

Listed below are only some examples I have found in my review.
cloud lifetime effects -> aerosol lifetime effects (Although this may be an issue in terminology, I believe that "aerosol lifetime effects" is more appropriate term to represent the second kind of indirect effect of aerosols first suggested by Albrecht [1989].)
We feel the term of 'aerosol lifetime effects' is confusing, for people may confuse this with the real change in aerosol lifetime. We now replace all 'cloud lifetime effects**' with '**cloud lifetime effects from aerosols**'.**

P.3404 L.23: homogenous -> homogeneous
Corrected.

P.3411, L.5: -0.5 -> -50.5
Corrected.

P.3414, L.2: show -> shows
Corrected.

P.3416, L.2: LWP and AOD -> LWP and AI
Corrected.

P.3422, L.2: less than 50gm-2 -> greater than 50gm-2
Corrected.

P.3427, L.2: clear-sly -> clear-sky

Corrected.

Anonymous Referee #3

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In this study the aerosol and cloud distributions, aerosol direct effect, and aerosol indirect effect are simulated with the Multi-scale Modeling Framework (MMF) that embeds a cloud-resolving model (CRM) which is in each grid of a general circulation model (GCM), CAM5. It is certain that traditional general circulation model with horizontal resolution over hundred kilometers is insufficient for presenting the aerosol indirect effect. On the other hand, long-term global simulations with horizontal resolution about several kilometer is not realistic because of the present computer resources. Then the model in this study which considers only the subgrid cloud-aerosol interaction based on the GCM is useful and shows one of the directions of the concerned modeling studies for the moment. Therefore I suggest that this manuscript will be able to be published if the authors address major and minor revisions indicted below.

<Major revisions>

1. This manuscript as a whole is wordy, especially in section 4.2.2. The same detailed explanations both in figure captions and main text is not needed. Enumeration of numbers in main text same as tables is also not needed. The wordiness results in difficulty in understanding advantages of the PNNL-MMF model. Clarify more predominance of the PNNL-MMF model in evaluating the cloud-aerosol interaction all over the manuscript, especially in abstract.

Section 4.2.2 was edited and reorganized in the revision. We moved the discussion of the PDF of LWP to the end of that section, and moved the discussion of the scatter plots of LWP and CCN right after the third paragraph in that section. We also shortened the discussion about the PDF of LWP. The original text in Sect. 4.2.2 is reduced by more than 15%. With these changes, we believe the revised version is better organized, and more readable. Other parts of the manuscript have also been updated to address questions raised by reviewers.

2. page 3406, lines 10-11: "Results from the 34 months are used in this study" Therefore the analysis in this study may incline about the seasonal cycle because it includes 3 years for 10 months and 2 years for the other 2 months. If the model need 2 months for spin-up, the MMF model has to be integrated for 38 months.

It would be desirable to have two additional months integration for the MMF results. Unfortunately, the MMF model is an expensive model (more than 100 times more expensive than CAM5) and we were limited by the available computing resource to run longer simulations last year. What is more, we do not expect adding two months integration will change our major conclusions in the paper. Both the MMF and CAM5 run with fixed SST and sea ice, which make the interannual natural variability of global means quite small in these simulations. This is especially true for the annual mean results, on which most of our major conclusions are based on. For example, the slopes between the relative change in LWP and the relative change in CCN in CAM5 (Fig. 17c) vary

little when we varied the averaging time from 24 months to 48 months (the slopes are 0.306 for the 24-month average, 0.290 for 34-month average, 0.286 for 36-month average, and 0.304 for the 48-month averages). Another example is the changes in shortwave cloud forcing from anthropogenic aerosols in CAM5. The global annual mean values are -1.791, -1.776, -1.782 and -1.759 W m^{-3} for 48 months, 36 months, 34 months, and 24 months averaging, respectively. The difference between the 34 months and 36 months results are very small. However, we do recognize that the short integration time for the MMF model can be a limitation if we want to examine regional changes, and we expect to have longer simulations in the future, and will use the longer simulation in future studies. A paragraph discussing these points was added at the end of Section 5, and it reads (P. 28, l. 22-32): "We note that a short integration time (34 months) is used for the MMF model in this study. The MMF model is an expensive model, and we are constrained by the limited computing resources to have longer simulations for this study. Since the simulations are driven by fixed climatological sea surface temperature and sea ice, the interannual natural variability of global means is quite small in these simulations and we do not expect our results to be sensitive to the short integration time. This is especially true for the annual mean results, which are the basis for most of the major conclusions presented here. For example, the slopes between the relative change in LWP and the relative change in CCN in CAM5 (Fig. 16b) vary little when we varied the averaging time from 24 months to 48 months (the slopes are 0.306 for the 24-month average, 0.290 for 34-month average, 0.286 for 36-month average, and 0.304 for the 48-month averages).".

3. page 3409, lines 13-14: Why is the shortwave cloud forcing almost same between MMF and CAM5 although the low cloud fraction and the droplet effective radius at cloud top in MMF is smaller and larger than those in CAM5, respectively, as shown in Table 1? Can you show the annual mean cloud droplet effective radius, for example, by the latitude-vertical plot with zonal mean?

Cloud-top droplet radii for low level and warm clouds are similar in both models. The similar shortwave cloud forcing is partly caused by the larger liquid water path in the MMF. The liquid water path is 55.9 g m^{-2} in the MMF model, while it is 48.4 g m^{-2} in CAM5. We examined the zonal-mean latitude-pressure plots of droplet effective radius (See Fig. S1). Though there are differences in the zonal-mean latitude-pressure plots in the two models, it is difficult to link these differences with shortwave cloud forcing. Given that the paper is already long and our focus is about aerosol indirect effects, we decided to not include this figure.

4. page 3409, lines 14-16: Why is the longwave cloud forcing in MMF larger than in CAM5 although the high cloud fraction in MMF is smaller than in CAM5? How is a difference in the ice crystal effective radius between them?

This is partly from the larger ice crystal number concentrations in the MMF. Column integrated ice crystal number concentrations in the MMF are about twice those in CAM5 (Table 2), which leads to substantially smaller ice crystal

effective radii in the upper troposphere (see Fig. S1) and further leads to the larger longwave cloud forcing.

5. page 3410, lines 25-27: A difference in the cloud fraction between MMF and ISCCP is very large (_10%), so that more detail discussion is needed. What is the other reasons?
Given the uncertainties in the observations of cloud fraction in both ground and satellite observations, and given the different definitions of 'clouds' in the models and observations, the difference between MMF and ISCCP is still acceptable. As we acknowledge in the text and the abstract, further efforts are needed to improve simulated cloud fractions, especially low cloud fractions. We are working on testing how a high order turbulence scheme in the MMF model will affect the simulation of cloud fraction, and the results will be reported in a future paper. We are also working on running an offline COSP simulator package that includes a suite of satellite simulators to process the MMF ouput, which will provide a fairer comparison between models and satellite observations (e.g., Marchand and Ackerman, 2010). The results from these comparisons will be reported in future studies.

6. page 3413, line 1: The sulfate burden in MMF is about twice as large as in CAM5. This is a critical problem. Compare them with observations and describe which is better.
Sulfate concentrations in the MMF model have been compared with several surface observational networks in Wang et al. (2011). Here we added Table 3 to summarize the model performance, along with the results in CAM5. Table 3 shows that both models overestimate surface SO_2 observations at both IMPROVE and EMEP sites, while the MMF simulates lower SO_2 concentrations, which agree better with observations. For sulfate, both models overestimate surface concentrations at EMEP and IMPROVE sites, but underestimate sulfate concentrations at remote oceanic sites. The MMF simulates high sulfate concentrations over remote oceanic sites, which agree better with the observations. As we also discussed in the fourth paragraph of Section 3.2, simulated sulfate concentration in the polar regions also shows a similar improvement over CAM5 as BC shown in Figure 8. We also added Table 4 to summarize the model performance in terms of simulated AOD over the AERONET sites (the scatter plots of simulated AOD and observations is shown in Figure 22 in Wang et al. (2011)). We can see that simulated AOD in the MMF agrees slightly better with observations than that in CAM5, both in terms of normalized mean bias and the correlation coefficients with observations. See our discussions about Table 3 and Table 4 in Section 3.2 (P. 13-14). However, these comparisons are still not conclusive and we still can not say which model is more realistic in terms of its simulation of sulfate.

Though the differences in simulated sulfate concentrations are quite large in two models, we do not expect this will affect the major conclusion of the paper. See the discussion in our answers to Question #2 and #12 from Reviewer #1.

<Minor revisions>

7. page 3408, lines 23-24: What is the differences in the microphysical schemes?

Given the large amount of differences between two schemes, we feel that detailed discussions about the differences in two scheme will not be conclusive. Nevertheless, additional discussion about the differences is added in that paragraph, and now the text reads (P. 9, l. 4-10): 'These differences may result from the differences in the microphysics schemes in the CRM components in the two MMF models. For example, the CRM component in the NASA fvMMF employs a single-moment bulk microphysics scheme (Tao et al., 2003), while the two-moment microphysics scheme is used in the PNNL MMF. Though the density of snow and graupel are the same in both MMFs (snow: 0.1 g m^{-3}; graupel: 0.4 g m^{-3}), the density of cloud ice is smaller in the PNNL MMF (0.5 g m^{-3} vs. 0.92 g m^{-3} in the NASA fvMMF). Given the fact that no global observation is able to distinguish different ice hydrometeors, it is still difficult to constrain the partitioning among different hydrometeors in GCMs.'.

8. page 3409, lines 1-3: Does black carbon act as cloud condensation nuclei or ice nuclei? And is this treatment same as this study (CAM5-CRM-MMF)?

Both CAM5 and the MMF use the same 3-mode aerosol treatment in which black carbon particles are assumed to internally mixed with other accumulation mode aerosol species. Whether an aerosol particle will activate depends on its composition (bulk hygroscopicity) and size, and the internal mixing assumption causes black carbon to be fairly efficient CCN in most locations. Black carbon does not act as ice nuclei in either CAM5 or the MMF, as we discussed in detail in the 3rd paragraph in Section 3.1.1 (P. 9, l. 14-18): 'The ice nucleation treatment in the MMF model does not directly link heterogeneous ice nuclei to aerosols although aerosols can influence ice crystal number concentration through the freezing of cloud droplets activated on aerosols. In CAM5, sulfate can form ice crystals in cirrus clouds through homogeneous freezing, and dust can act as heterogeneous ice nuclei in cirrus and mixed-phase clouds (Gettelman et al., 2010).'.

9. page 3411, line 5: Change "-0.5" to "-50.5".

Corrected.

10. page 3411, lines 10-11: How is shape of snow particle, which is an important information for the calculation of radiative transfer.

The radiative effect of snow is treated in the same way as that in CAM5 (Gettelman et al., 2010). The particle shape recipe was based on crystal shape observations reported in Larson et al. (2006) at -45C: 7% hexagonal columns, 50% bullet rosettes and 43% irrugular ice particles. We clarify this in the text, and now the text reads (P. 11, l. 24-28): 'The radiative effect of snow particles is accounted for in this study, following the same treatment as in CAM5 (Gettelman et al., 2010), and is included in the cloud forcing. The particle shape recipe was based on observations reported in Larson et al. (2006) at -45°C: 7% hexagonal columns, 50% bullet rosettes and 43% irregular ice particles.'.

11. final paragraph of section 3.1.2: Discuss reasons for a difference in the cloud-top droplet number concentrations among MMF, CAM5, and MODIS.

As we noted in the text, the difference in cloud-top droplet number concentrations between the MMF and CAM5 is consistent with the difference in simulated CCN concentrations in two models (see more discussions in Section 3.2 about the difference in CCN concentrations between the MMF and CAM5). As for the difference between the satellite data and the model results, it is difficult to attribute these differences into specific reasons given the larger uncertainties in the satellite retrievals of cloud droplet number concentrations and a number of different processes can lead to different cloud droplet number concentrations in the models. So we decided to just present the difference between the models and satellite data.

12. page 3414, line 2: Change "show" to "shows".

Corrected.

13. page 3414, lines 6-8: Describe the reasons. Is it because differences in vertical diffusion and advection of aerosol between MMF and CAM5?

Aerosol and CCN concentrations are affected by many processes (e.g., vertical diffusion, convective transport, wet scavenging and long-range transport), and a lot of these processes are coupled together (e.g., convective transport and wet scavenging). Though the PNNL-MMF model is based on CAM5, all turbulence and moisture parameterizations are replaced by the embedded CRM. Given the differences in simulated clouds and precipitation in the PNNL-MMF and CAM5, it is difficult to exactly tell which individual processes contribute most to the difference in simulated CCN concentrations in two models. In an ongoing study, we focus on understanding the extreme low aerosol concentrations in the polar regions simulated in CAM5 as shown in Figure 8. We also plan to run both the MMF and CAM5 nudged with the reanalysis winds to separate the long-range transport from convective transport and wet scavenging. So we added the following discussion in the end of that paragraph (P. 15, l. 3-6): "Differences in convective transport, wet scavenging in stratiform and convective clouds, and long range transport between the MMF model and CAM5 may lead to these differences. Further studies are needed to identify the causes for the differences between the MMF and CAM5.".

14. 1st paragraph of section 4.1: Change "Angstrom" to "Ångström" (3 parts).

Done.

15. page 3415, line 20: Change "most" to "some". According to Fig. 2 in Quaas et al. (2009), some models show the similar slope to the satellite retrieval.

We change this, and now the text reads (P. 16, l. 8-9): 'We note that half of the global climate models included in Quaas et al. (2009) overestimated the slope.'.

16. page 3416, line 2: Change "AOT" to "AI".

Corrected.

17. page 3427, line 2: Change "clear-sly" to "clear-sky"
Corrected.

18. page 3428, line 26: It is not a "common" feature as described above.
We removed this statement.

19. caption of Table 1: Add "at cloud top" after "droplet effective radius".
Done.

20. caption of Fig. 11: Change "weighted average" to "weighted averages" and "is show" to "are shown".
Done.

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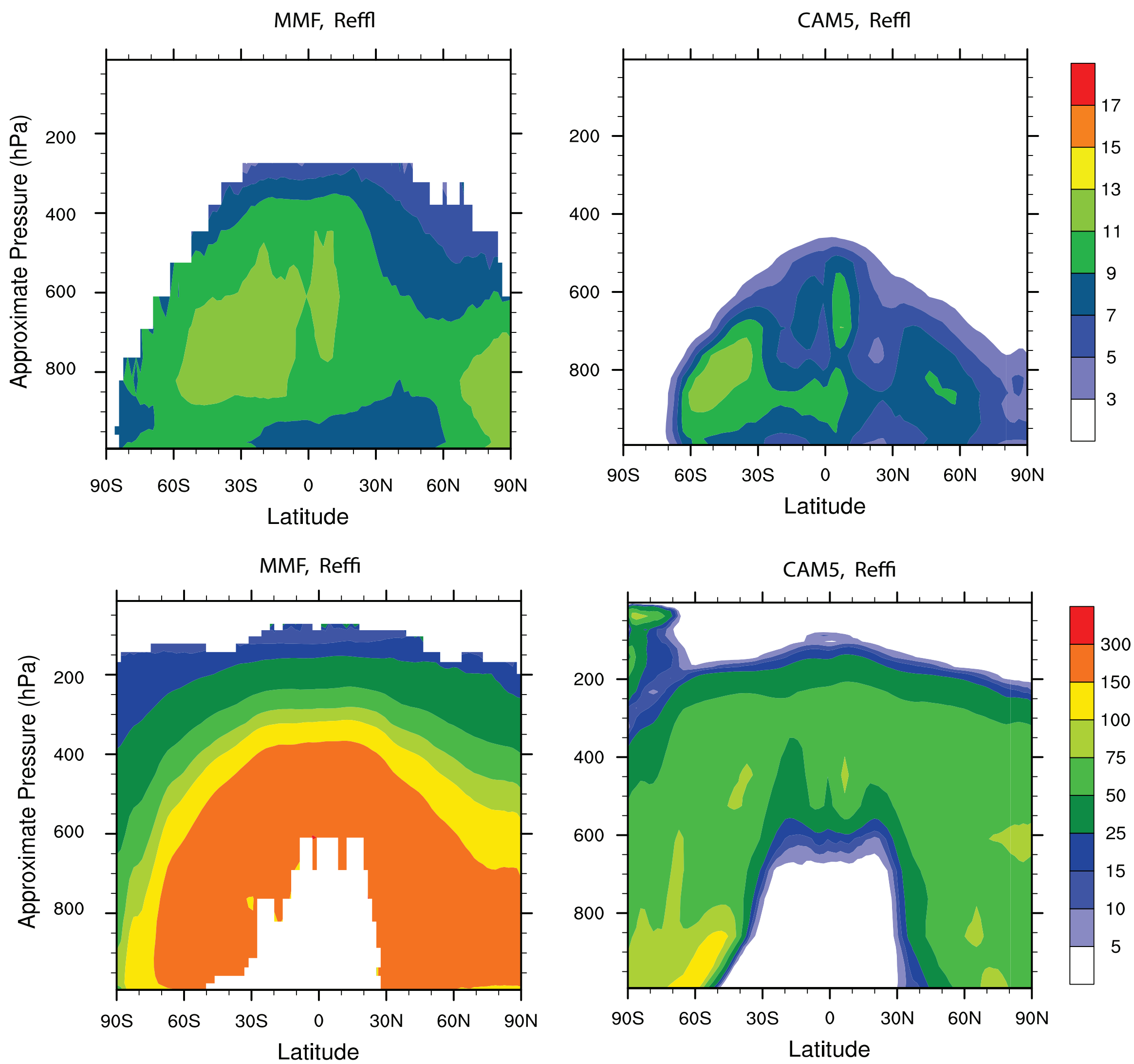

Figure S1. Annual average zonal mean cloud droplet effective radius (Reffl, upper panels) and cloud ice crystal effective radius (Reffi, lower panels) in the MMF (the left panels) and CAM5 (the right panels).

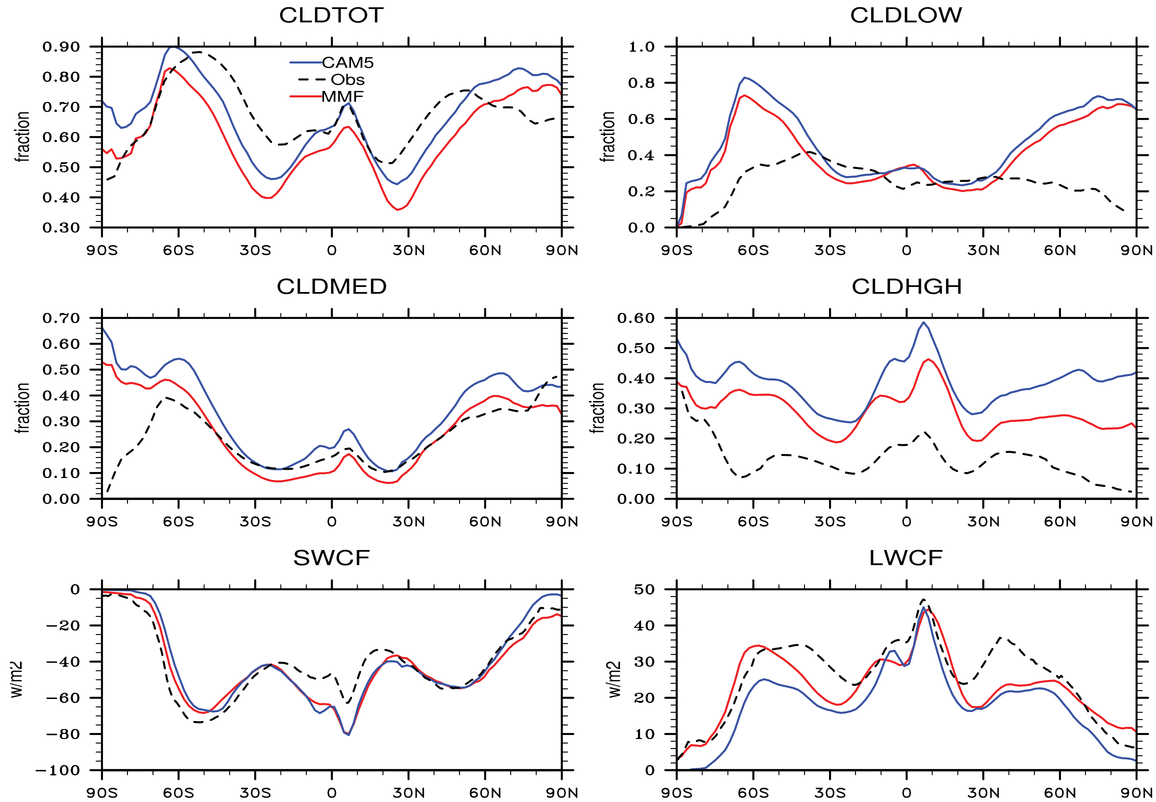

Figure S2. Annual average zonal mean total cloud fraction (CLDTOT), low level cloud fraction (CLDLOW), middle level cloud fraction (CLDMID), high level cloud fraction (CLDHGH), shortwave cloud forcing (SWCF), and longwave cloud forcing (LWCF) from the MMF, CAM5, and observations (obs).