Atmos. Chem. Phys. Discuss., 11, C281–C287, 2011
www.atmos-chem-phys-discuss.net/11/C281/2011/


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Aerosol indirect effects in a multi-scale aerosol-climate model PNNL-MMF" *by* M. Wang et al.

**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.

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".

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.

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?

4. p. 3403, l. 25: is this the final version of CAM5?

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?

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 $0.1\,m\,s^{-1}$" -> How exactly is this done (reference?) and do you know how sensitive the main results are to this choice?

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?

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

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?

10. p 3408 l. 14: "It is also close to that retrieved from Cloud-Sat (around 80 $g\,m^{-2}$), and MODIS (60 $g\,m^{-2}$), but is much larger than estimates from ISCCP (around $35\,g\,m^{-2}$ ) and NOAA NESDIS (around 10 $g\,m^{-2}$) (Fig. 18 in Waliser et al., 2009)" -> also cite original references? CloudSat is $75\pm30 g\,m^{-2}$

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?

12. p. 3413, l. 4 ff: do you have an idea on how strongly these different assumptions with respect to $SO_2$ wet removal influence your results regarding the anthropogenic aerosol effect?

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.

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.

15. Table 1: the PD net radiative imbalance at the top of the atmosphere is greater than 2W $m^{-2}$ 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?

16. Table 1: The CloudSat IWP is 75±30 g $m^{-2}$. 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.

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.

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 20$^o$N the change in SWCF is nevertheless positive?

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?

20. Fig. 14: Does a map of ΔSWCF yield a similar pattern of change as LWP in the northern sub-tropics?



Technical comments:

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

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

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

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

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

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

p. 3418, l. 18: 0.53 vs. 0.52 in the table

p. 3418, l. 19: 2.10 vs. 2.11 in the table

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

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 −1.66 W $m^{-2}$, respectively.

better change to: In the standard version of CAM5, the simulated PI to PD change in shortwave cloud forcing is -1.79 W $m^{-2}$, the change in longwave cloud forcing is 0.37 W $m^{-2}$, the aerosol direct effect in the clear sky (taking into account entirely clear grid boxes) is -0.45 W $m^{-2}$, and the total aerosol effect on top of the atmosphere net radiation is -1.66 W $m^{-2}$.

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

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

p. 3425: evaporate -> evaporated

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

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

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

Table 1, CAM5 (PI) CLDHGH: 37.4

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

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

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.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3399, 2011.