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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraints on first aerosol indirect effect from a combination of MODIS-CERES satellite data and global climate simulations" *by* X. Ma et al.

Anonymous Referee #2

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This paper correlates the dry sulphate and organic aerosol masses predicted from the CanAM4 GCM with the MODIS derived cloud droplet effective radius for warm clouds in order to characterize the magnitude of the first aerosol indirect effect. This is an important topic as there are difficulties in attempting to reconcile estimates of the indirect effect between models and observations. This paper's main contributions are to show that organic aerosols play a similar role as sulphates, that dependence on liquid water path is not properly handled in models and that their method gives results consistent with previous studies. I do not see any new insights provided by this work and their method does not allow the indirect effect to be constrained any better than previous studies, but it is a useful study to help reinforce some of the issues that are

important in this area. The paper is clearly presented and well-written but I have a number of scientific points detailed below that the authors should respond to before moving to a full paper.

Major Issues

1. The title of the paper suggests, probably too boldly, that constraints are being placed on the magnitude of the first aerosol indirect effect. By mixing satellite observations of cloud properties with model aerosol properties in order to determine the slope parameter (dlog(reff)/dlog(SO4)) it is not possible to constrain the effect in a consistent way. I realize that determining the "true" value of this slope and its error bounds are not objectives of the paper but the title suggests otherwise. I would replace the work "constraints" by "studies" or some other term.

2. Their results (e.g. Figure 3) suggest that the model clouds are less susceptible to aerosols than satellite-determined cloud properties. This is contrary to previous studies that show that GCMs are likely over-estimating the magnitude of the first aerosol indirect effect. What is the explanation for this different result?

3. The study focuses on clouds with tops below 700 hPa which encompasses many cloud situations. Although the criterion will reduce the likelihood of mixed phase clouds it does not eliminate them. What fraction of the cases do they estimate consist of clouds with some ice present? Also their study includes low clouds with very different dynamical regimes (stratus to stratocumulus to cumulus) and for the entire globe except polar regions. Geographical and dynamical influences in their cloud susceptibilities could yield important insights. Is it possible for the authors to include such an analysis?

4. In the middle of Section 3 (top of page 13950) it is argued that long time averaging is desirable because it reduces the weather-related variations compared to climate features, and hence they use seasonal means of the MODIS retrievals of cloud properties. However it has been pointed out (e.g. Stevens and Feingold, 2009 and others) that the multitude of microphysical and dynamical processes can lead to a "buffering"

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of the indirect effect and I would claim that doing long time averages will contribute to this problem. More discussion and recognition about this issue is needed.

5. I am somewhat surprised that the cloud droplet activation is being parameterized simply by the old-fashioned aerosol mass concentration instead of having a prognostic equation for aerosol number. Why is not practical to have a more advanced treatment of activation with a prognostic equation for aerosol number? What evidence is there that the aerosol mass concentration parameterization works sufficiently well to predict the cloud droplet number? Obviously all dynamical effects are being ignored with this parameterization. I can understand using Eqn. 1 for long-term averaged quantities but it is my understanding that Eqn. 1 is being used at the time and space step of the GCM run.

6. One of the main results of this paper is that organic aerosols are contributing to the indirect effect. Does the organic contribution include both the hydrophilic and hydrophobic components? If it is just the hydrophilic component then it seems to be an obvious result. Why should this be considered a new finding? The higher sensitivity to sulphate when using Eqns 2 and 3 is also obvious since the coefficient in front of the log(SO4) term is 0.50 instead of 0.20. Why didn't the authors develop their own improved parameterization for cloud droplet number with a better balance between sulphate and organic effects on clouds? Also the parameterization should probably use the sum of the sulphate and organic masses instead of a logarithmic product to avoid their problem when the organic mass goes to zero.

7. In Figure 3 the aerosol dependence is given as a column burden. However the cloud droplet parameterization (Eqn. 1) is based on aerosol concentration at the level where the cloud forms. This inconsistency can lead to biases in the results. How have they accounted or corrected for this problem?

Minor Issues

1. The age of the cloud is likely an important factor in altering the apparent correlation

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between aerosols and cloud effective radius. The authors should point this out and explain why they are unable to address the issue.

2. What years were used in compiling the MODIS observations?

3. In the second paragraph in Section 4 (bottom of page 13950) they state there are large cloud droplets over oceans where aerosol concentrations are low and cloud liquid water contents are high. Isn't the cloud liquid water content more a function of the average temperature (i.e. latitude) rather than land versus ocean? Also shouldn't the average cloud updraft speed, which is typically lower over the oceans, be included as a factor? Strong updrafts will activate more aerosols leading to smaller cloud droplets even for fixed total aerosol number.

4. Since GCM aerosols are being used why wasn't the anthropogenic component of the indirect effect computed?

5. In Section 3 it is stated that simply using the cloud droplet radius from the top cloud layer in the model is too simple and they use a more sophisticated approach by Klein and Jacob (1999). It is not shown whether the more sophisticated approach is any better than the simpler approach. This needs to be discussed.

6. Why isn't there an estimate of the radiative forcing due to the indirect effect based on their results?

7. In Figures 4,5,8 and 9 I recommend that the y-axis extend only to 30 um in order to stretch the plots vertically. They are very small and hard to see. Also please add the number of points plotted on each sub-figure (perhaps under the slope information).

8. Last paragraph in Section 2, the word "microphysics" is misspelled twice.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 13945, 2010.