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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol-cloud interactions in the NASA GMI: model development and indirect forcing assessments" *by* N. Meskhidze et al.

Anonymous Referee #2

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Aerosol-cloud interactions in the NASA GMI: model development and indirect ofrcing assessments By Meskhidze, et al. (Atmos. Chem. Phys., acpd-2007-0393)

General Comments

Aerosol effect on cloud and radiation remains one of the largest uncertainties in projecting future climate change. The NASA GMI project provides a suitable framework for assessing the uncertainties due to different components of the model (e.g., cloud parameterization and meteorological fields) with its modular structure. This study examines and compares the differences in aerosol indirect forcing due to the input meteorological data versus cloud nucleation parameterizations, and also validates the

calculations with satellite observations. This paper is relevant for publishing in Atmos. Chem. Phys. I have the following issues as listed below that needed to be addressed.

Specific Comments

1. The title of the paper "Aerosol-cloud interactions". I would change to "Aerosol indirect effect" because cloud effect on aerosols is not included in the paper. "model development and indirect forcing assessments". There is not much model development involved in this study. I would suggest change to "sensitivity to input meteorological data and cloud nucleation schemes";

2. Abstract, lines 17-18. "roughly 80% of the variation is attributed to changes in the meteorology (primarily from variation in liquid water path)"

How do the authors determine this "80%" attributed to changes in the meteorology (primarily from variation in liquid water path)? It seems to me that the variation is primary from sulfate mass concentration and spatial distribution differences caused by different meteorology. The liquid water content is prescribed in the GMI model. If the authors say liquid water path (LWP) is more important, then how different the LWP is between different meteorology?

3. section 2.2.1 cloud fraction calculation

I would suggest to simplify this section (eqs.(1)-(6)) and mention Liu et al. (2005) here since this section have been discussed in Liu et al. (2005).

4. page 14301, second paragraph. It looks to me that only sulfate mass is used to scale the aerosol size distribution for FN parameterization. Does this cause any biases over the ocean where sea salt can be important, and in biomass burning regions where carbonaceous aerosol can dominate?

5. page 14304, line 5. Aerosol indirect forcing (IF) Hereafter the "IF" is used to stand for "aerosol indirect forcing". However, there are many places in the manuscript where "AIF" is used. It should be consistent throughout the paper. I would suggest to use

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"AIF".

6. page 14304, line 25. I have a basic problem with scaling down GMI AIF using the scaling factor 0.5. Do you assume that the FAST-J is not accurate or the GMI AIF is too large?

7. page 14305, section 3.1 sulfate burden

You mention that FVGCM predicts 1.4 and 1.3 times high concentrations than DAO and GISS. However in the next paragraph the FVGCM sulfate burden is smallest. Can you add the factors accounting for these differences (e.g., precipitation scavenging, transport, and convection...)? The unit for sulfate global burdens is not correct. It should be Tg S not Tg S yr-1.

8. page 14305, section 3.2, lines 21-22 How to define the lowest cloud-forming level? By cloud water, cloud fraction, or else?

9. page 14306, Figure 3. It looks to me that the CDFC calculated with FN scheme is much higher than that with BL over the industrial regions. Can you explain?

10. page 14309, Figure 5. The COD differences between meteorology are much higher than those between cloud parameterizations. It should be added. Why the COD over Europe from models is much smaller than that from MODIS while COD over other industrial regions are comparable? The usage of "cloud optical thickness" and "cloud optical depth" is confusion. "Cloud optical thickness" is used in the title of section 4.2 (and also in Figure 5 caption), while "COD" is used in text.

11. Figures 4 and 5. I would suggest to add ISCCP observations for effective radius and COD since they are discussed in text and in Tables 4 and 5.

12. Figure 6. Figure 6 is dominated by red colors. Please re-plot Figure 6 using a different color bar (i.e., cold colors instead of warm colors).

13. page 14311, line 11 I would change "aerosol mass simulated online" to "sulfate

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mass simulated online" since only sulfate mass is used.

14. page 14311, lines 18-20, "leave the key role to the meteorological fields used"

However, CDNC difference is much larger between cloud schemes than meteorological fields in the industrial regions (see Figure 3).

15. page 14312, line 18, "associated with variability in liquid water path and long range transport"

Again I can not see why the variability in LWP plays a key role. For the variability in long range transport, is it due to difference in advection or in scavenging of aerosols?

Technical Corrections

1. page 14299, line 20 "than" -> "then"

2. Eq(6), missing integration sign.

3. Page 14303, line 1. "the grid Eq.(8)"

Delete "Eq.(8)"

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14295, 2007.