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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.

N. Meskhidze et al.

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We thank the reviewer for the thorough and thoughtful 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".

There are important model developments (described in sections 2.2.2, 2.2.3 and 2.3) that justify the usage of "model development" in the title. In addition, specifying "indirect forcing" in the title determines which aspect of aerosol-cloud interactions we are addressing.

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?

Good point. In retrospect, we see that this statement could cause some confusion. Our reference to liquid water path implied a stronger wet deposition of sulfate, which in the end controls the variability in CDNC and indirect forcing. Statement is corrected now as follows "*roughly 80% of the variation is attributed to changes in the meteorology*". We have also elaborated on how these percentages are calculated, as well as provided a map of the LWP in each meteorological field.

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).

Liu et al., (2005) provide a very short description; we provide a description that includes the equations for completeness.

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?

Indeed. This issue is addressed by prescribing a lower limit for droplet number concentration of 40 cm^{-3} (invoked whenever the sulfate concentrations are too low to give

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sufficient CCN), which is a typical concentration of remote marine clouds and gives reasonable values of effective radius for remote oceanic areas. For biomass burning aerosol, we are assuming that the organic component is externally mixed with sulfate and does not influence cloud droplet formation. We understand that this may underestimate the indirect forcing and have pointed this out in the text. Future work involving simulations with explicit microphysics within GMI will address these issues.

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 "AIF".

We thank the reviewer for bringing this to our attention. The manuscript has two acronyms: "IF" and "AIE" (not "AIF"); the former stands for (first) "indirect forcing" and the latter for "aerosol indirect effect". The use of these abbreviations is consistent throughout the manuscript now.

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?

The simple equations used to describe the cloud radiative forcing (as well as not explicit treatment of aerosol direct forcing) is the primary source of overestimation. Given that the forcing, even if overestimated, scales as expected with changes in sulfate loading and CDNC for all meteorological fields considered, it can be scaled-down to the value obtained by another model that employs a more sophisticated radiative transfer routine (in our case, the GISS GCM II').

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 convec-

tion...)?

Good point. Compared to FVGCM, GISS and DAO are characterized by stronger vertical transport and weaker dry deposition. As a result, FVGCM has higher concentrations in the lowest modeling layer, but overall has the lowest global burden of all three met fields. A relevant discussion has been included in the manuscript.

The unit for sulfate global burdens is not correct. It should be Tg S not Tg S yr-1.

We apologize for this oversight. Units corrected.

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

In page 14298, lines 26-27 we mention that: "*clouds are allowed to form in any model layer (with the exception of the layer nearest to the surface to constrain wet deposition)*", i.e., only the first modeling layer is not allowed to form clouds.

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?

The BL formulations for "marine" and "continental" regions are derived from observations of limited spatiotemporal coverage (four studies from marine regions and only one continental study from NE America), and, does not extrapolate well to the much higher aerosol concentrations found in polluted regions (this, in addition to uncertainties associated with unresolved size distribution, composition, and cloud updraft). FN is based on physics, and predicts droplet number that is reflective of the "local" aerosol/cloud conditions, whatever they may be. A relevant discussion has been included in the revised manuscript.

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

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Interactive Comment

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.

The text now reads "*The COD differences between different meteorological fields are much larger than those between different cloud droplet schemes*."

In terms of the COD differences between models and observations, we corrected a typo in the calculation of average MODIS COD, and the agreement with simulations is substantially improved. For example, using the FN parameterization we have:

Regions	MODIS	DAO	FVGCM	GISS
NE US	11.56	7.40	10.25	15.75
Central Europe	15.28	7.88	13.62	14.02
SE China	11.33	7.75	12.02	15.54
Mid-Atlantic	8.64	7.43	11.61	10.79

The relevant discussion on intercomparison with satellite data has been updated to reflect the changes.

The term "cloud optical depth" is now used throughout the manuscript.

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.

ISCCP plots for effective radius and cloud optical depth have been presented elsewhere (Han et al., 1994). In sections 4.1 and 4.2 we provide a reference for that.

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).

The figure is changed as suggested.

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

The text is changed as suggested.

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).

We thank the reviewer for raising this issue. Our discussion referred to the global average; indeed, the effect of droplet schemes can outweigh the meteorology regionally. Statement reads now as "*... Different droplet schemes ... to the meteorological fields used. Regionally (e.g., near large sources), the contribution of droplet schemes may dominate over meteorology*."

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?

Please see response to comment #2.

Technical Corrections

1. page 14299, line 20 "than" $\rightarrow$ "then"

2. Eq(6), missing integration sign.

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

All changes made.

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

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