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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of aerosol distributions in the GISS-TOMAS global aerosol microphysics model with remote sensing observations" *by* Y. H. Lee and P. J. Adams

Anonymous Referee #1

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I reviewed the the Lee & Adams paper titled "Evaluation of aerosol distributions in the GISS-TOMAS global aerosol microphysics model with remote sensing observations." The paper is generally well written, with only minor typographical or organizational comments. The figures are generally clear in their presentation, and I appreciate that the paper is relatively compact.

I think the authors do a great job of explaining the limitations of the satellite data they are dealing with (although explicit statement of the uncertainties in the MODIS and MISR AOD would be a helpful addition). They make good points about (i) clear-sky bias in the satellite retrievals and (ii) bias against high latitude retrievals. In addressing

the first of these issues I have found it important to first sub-sample my model results only when/where MODIS makes a retrieval. But in my model–driven by assimilated meteorology–there is a correspondence between where the model puts clouds and where MODIS saw clouds. I realize your model is climatological, so you can't do exactly that, but I wonder if some thesholding on, e.g., cloud fraction in your grid boxes might improve the comparisons (or could at least be speculated about). Alternatively, you could compute a "dry" AOD as a lower boundary.

Also, rather than a reference to a web document, the MODIS Collection 5 algorithm can be referenced to: Remer et al. Global aerosol climatology from the MODIS satellite sensors. J Geophys Res-Atmos (2008) vol. 113 (D14) pp. D14S07 http://dx.doi.org/10.1029/2007JD009661

A note about the AERONET data: they don't report all these wavelengths for all instruments, and 935 nm isn't reported at all, I think; that channel is used for water vapor retrieval.

Figure 1: Please clarify in the text that the water shown is the aerosol water, right?

Table 4: Please explain in the text what LMNB and LMNE are. I think I understand mean normalized bias and error, but am unsure what you mean by "logging" it. How does that correspond to absolute biases and errors?

Regarding the Global Fire Emission Database, the correct reference is: van der Werf et al. Interannual variability in global biomass burning emissions from 1997 to 2004. Atmos Chem Phys (2006) vol. 6 pp. 3423-3441. But maybe the Dentener reference is the correct one if that is indeed the data source for your particular application (or you could note that you are getting the emissions from the AeroCom folks).

Given the amount of work done on dust modeling, why is your model so bad at getting the JJA dust transport out of Africa? Is it your coarse resolution not kicking up enough emissions? Or something about the transport? You need to explain this deficiency.

ACPD

9, C7157–C7160, 2009

Regarding the SH high latitude sea salt issues, Figure 5 in Pierce and Adams (2006) shows only one of the four mentioned sea salt emission schemes seriously overestimating sea salt at Palmer Station (although that happens to be the scheme that compares well at most of the other sites). Is this the particular scheme used in this paper? The discussion here is a little unsatisfying. It could be emissions; it could be water uptake. But it could also be vertical distribution and it could be poor retrievals from the satellites. It could even be size distribution, couldn't it? You could fix the surface mass maybe by having a better representation of the particle size distribution, or by adjusting the emissions. But you could still be way off in the AOD comparison either because of vertical distribution (too much aloft for some reason) or just because the satellite retrievals are so poor. So mass is better thing to key off of here, as you try to do, but there are additional explanations that could bear some consideration.

Regarding the biomass burning sources: you correctly point out that most models underestimate the AOT from biomass burning. There are a variety of reasons why this might be, and not just because of uncertainties in the sources (emission factors, yes, but also with the fundamentals of how these inventories are arrived at). There's also particle size, which is determinate in the AOD calculation. Your model should ideally be well suited to investigating that aspects. There's as well timing of emissions and injection height (how do you do these in your model?). Finally, you suggest fast loss processes. Can you provide some evidence you have too much precip in these regions during the dry season? (The short lifetime for wet removal isn't convincing; you could just have a particularly bad parameterization of that process. Showing excess precip would be more convincing.)

There are two novel aspects of this model that I think deserve more comment in the manuscript. The first is the coarse resolution, both vertically and horizontally, and what role they have in processes of transport, sources, and sinks. I suggested above a few places to look into that. I would suggest the discussion of Mexico City being poorly simulated because of resolution isn't adequately justified. The second aspect is more

ACPD

9, C7157–C7160, 2009

of a positive: your model has very detailed aerosol microphysics. So what elements of this comparison are impacted by that detail? Particle size and composition (SSA) should be better in your model than in bulk models (OK, maybe not necessarily better, but certainly more complex). How are your AOD simulations sensitive to those features of your model?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 19471, 2009.