Atmos. Chem. Phys. Discuss., 9, C10576–C10586, 2010
www.atmos-chem-phys-discuss.net/9/C10576/2010/

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

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Received and published: 5 February 2010

Comments

I reviewed 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 thresholding 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.

Response: We compute dry AOD as the lower bound on AOD. The clear-sky AOD will, of course, be above the dry AOD due to water uptake by aerosols. At high latitude regions, where the model AOD is a factor 3-5 times higher than satellite-retrieved AOD (shown Fig 3), the model dry AOD is lower than the observation but agrees within a factor of 2-3. However, we still think that AOD over high latitude regions contains a large uncertainty that give a difficulty on the model evaluation. The following sentences are included in the revised manuscript.

"As a lower bound on AOD, we also computed the dry AOD predicted by our model. The clear-sky AOD will, of course, be greater due to water uptake by aerosols. At high latitudes, where the model AOD is a factor 3-5 times higher than satellite-retrieved AOD (shown Fig 3), the model dry AOD is lower than observations by a factor of 2-3 (not shown). Performing a meaningful model-satellite comparison in this region is greatly complicated by several related challenges: difficulties in cloud-screening the satellite data, infrequent and possibly unrepresentative sampling, and determining the appropriate amount of aerosol water uptake in the model for comparison against satellite observations."

Comments

Also, rather than a reference to a web document, the MODIS Collection 5 algo-

ACPD

9, C10576–C10586, 2010

rithm 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

Response: We include the suggested reference.

Comments

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.

Response: Corrected.

Comments

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

Response: Changed to "aerosol water".

Comments

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?

Response: LMNB and LMNE are computed with equations below.

$$LMNB = \frac{\sum_{i=1}^{N} \log 10(C_{model,i}/C_{obs,i})}{N}$$
$$LMNE = \frac{\sum_{i=1}^{N} \log 10(C_{model,i}/C_{obs,i})}{N}$$

MNB and MNE are commonly used metrics in model evaluation. However a problem in MNB and MNE has been pointed out that MNB and MNE in % give less weight to underpredictions than to overpredictions; 100% MLB for a factor of two in overprediction,

ACPD
9, C10576–C10586, 2010

-50% MLB for a factor of two in underprediction. This problem disappears using LMNB and LMNE. The revised manuscript includes the equations for LMNB and LMNE.

Comments

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).

Response: Original reference used in Dentener reference is: van der Werf, G. R., Randerson, J. T., Collatz, G. J., Giglio, L., Kasibhatla, P. S., Arellano, A. F., Olsen, S. C. and Kasischke, E. S.: Continental-scale partitioning of fire emissions during the 1997 to 2001 El Nino/La Nina period, Science, 303, 5654, 73-76, 2004. Therefore, the reference is changed to van der Werf et al. (van der Werf et al., 2004). Note that van der Werf et al. (2006) is later version of GFED, named as GFED v2.

Comments

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.

Response: First paragraph in Section 4 (page 19483, line 22-25) explains causes of this underprediction.

Comments

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

ACPD

9, C10576–C10586, 2010

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.

Response: The model emissions parameterization is based on Clarke et al. (2006), hereafter called Clarke emission. To be clear, additional sentence (shown in underline) is included after the following sentence in the revised manuscript.

"Figure 5 in Pierce and Adams (2006) presents four different sea-salt emission schemes and the model simulation with Clarke et al. (2006) results in the overprediction of sea-salt mass concentration at Palmer Station; currently our model uses sea-salt emission based on Clarke et al. (2006); currently our model uses sea-salt emission based on Clarke et al. (2006)."

These discussions are included in the revised manuscript.

"In fact, this leads to high AOD over SH high latitude. AOD averaged from 40° S to 60° S using Martensson et al. (2003) and O'Dowd et al. (1997) are only 20A minor factor contributing to the AOD over-prediction in these areas may stem from a planetary boundary layer that is somewhat too deep. The planetary boundary layer obtained from MERRA (Modern Era Retrospective-analysis for Research and Applications), a NASA reanalysis for the satellite era using a major new version of the Goddard Earth Observing System Data Assimilation System Version 5 (GEOS-5), in these regions is typically ∼600 m, corresponding approximately to our lowest model layer (984 to 934 hPa). Approximately half of the column sea-salt in our model remains in this lowest layer while ∼ 25% and ∼ 15% are found in the second (934 to 854 hPa) and third (854 to 720 hPa) layers respectively. The presence of sea-salt in these layers suggests a

ACPD
9, C10576–C10586, 2010

sea-salt layer that is too deep, but the effect on model AOD is unlikely to be greater than a factor of 2 and, therefore, smaller than the issues discussed above."

Comments

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.)

Response: First of all, we found a mistake in wet deposition lifetime during September (less than 2 days) stated in the manuscript during investigating this precipitation issues. Corrected lifetime in September is $\sim$5 days.

In our model, biomass burning emission rate is constant over time, and the emission enters into the surface layer. We updated our discussion related to biomass burning emission. Please refer the following paragraphs included in the revised manuscript.

"Biomass burning emission has large spatial and temporal variations, and the year 2000 GFED emissions are the lowest year in the 1997 to 2006 time period (van der Werf et al., 2006). Use of relatively low biomass burning emissions in this work means that we expect a certain amount of underprediction compared to multi-year AERONET AOD (from 1999 to 2005, see Table 3 for details regarding time period). In Kinne et al. (2006), global models based on the GFED emission year 2000 have generally underpredicted AOD over the tropical biomass burning regions even when compared to

ACPD
9, C10576–C10586, 2010

AERONET AOD observed in the same year. AERONET AOD over Abracos Hill (11° S 62° W) in South America is approximately 0.5-0.6 in August and September, while AOD averaged among participating global models is around 0.3-0.4 and the lowest AOD is around 0.1-0.2. Our model AOD is 0.12 (August) and 0.17 (September). Global models have generally underestimated AOD over biomass burning regions. Our tendency to underpredict AOD in biomass burning regions is similar to some other global models but towards the low end of the range.

In our model, this general tendency to underestimate biomass burning AOD values seems to be related to wet deposition, although it appears that the bias stems from the wet deposition parameterization rather than errors in precipitation. Wet deposition is a major removal process for the carbonaceous aerosols, but its lifetime is expected to be long during the typical dry season, in August and September, when the biomass burning emission reaches to the maximum. However, the model-predicted wet deposition lifetime over the AERONET sites in South America is $\sim$16 days for August, $\sim$5 days for September, and $\sim$4 days for October and November. For comparison, our global mean aerosol OM lifetime with respect to wet deposition is $\sim$5 days. For September and October, corresponding to the mid and later biomass burning and dry seasons, our model-predicted aerosol wet deposition lifetimes of $\sim$5 days (similar to the global average) seem too short. However, when comparing model precipitation to monthly average precipitation measured by the TRMM satellite (obtained at http://agdisc.gsfc.nasa.gov/Giovanni/aovas) from August to November in 2002, no clear sign of model overprediction is found. Therefore, our model appears to be wet depositing organic aerosol too quickly in these areas despite having reasonable precipitation rate. Future work should investigate errors in precipitation distribution and frequency as well as aerosol scavenging efficiency to understand the source of these biases.

The predicted organic and elemental carbon concentrations from the simulation with the Bond emission inventory are compared to the Large Scale Biosphere-Atmosphere

ACPD
9, C10576–C10586, 2010

Experiment in Amazonia – Smoke, Aerosols, Clouds, Rainfall, and Climate (LBA-SMOCC) experiment 2002, which was conducted in Rondônia (10° S 62° W), Brazil during September to mid-November 2002 (Decesari et al., 2006). The model fine OC concentration (5.5 μg m-3) and EC concentration (0.9 μg m-3) during the dry season are 17% and 80% of the observation (31.6 μg m-3 for OC and 1.1 μg m-3 for EC), respectively. For the simulations with the GFED emission year 2000, the model fine OC concentration is slightly higher but only 26% of the observation. Although this is a rough comparison, it links the underprediction of AOD with the underprediction of surface mass OM concentration during the dry season. ”

Comments

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.

Response: Figure 17 in Kinne et al (2003) present how well AOD at AERONET site (local measurement) represent regional event and shows clear evidence of local pollution in Mexico City. It can not be captured by a coarse grid cell using in global models.

Comments

The second aspect is more 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?

Response: AOD is unlikely to provide significant insights regarding the advanced

ACPD
9, C10576–C10586, 2010

features in our model: the evolution of aerosol number distribution during condensation and coagulation processes. Aerosol mass concentration is a major determinant of AOD, with size distribution and mixing state playing more minor roles. Lesins and Lohmann (2006) shows the differences in AOD are caused by errors in the model aerosol mass burdens instead of aerosol size. Early sensitivity studies (e.g. Nemesure et al., 1995; Pilinis et al., 1995) compared the relative importance of different aerosol characteristics to AOD and direct forcing. For example, Pilinis et al found that varying the fine mode particle diameter from 0.2 to 0.5 μm for a lognormal distribution (geometric standard deviation of 2) changes direct forcing only by 20% . This is similar to uncertainties in the satellite AOD retrievals. Remer et al. (2009) and Kapustin et al. (2006) discuss the significant challenges involved in using remote sensing data to say anything about CCN and aerosol size distributions.

References

Clarke, A. D., Owens, S. R. and Zhou, J. C.: An ultrafine sea-salt flux from breaking waves: Implications for cloud condensation nuclei in the remote marine atmosphere, Journal of Geophysical Research-Atmospheres, 111, D06202, doi:10.1029/2005JD006565, 2006.

Decesari, S., Fuzzi, S., Facchini, M. C., Mircea, M., Emblico, L., Cavalli, F., Maenhaut, W., Chi, X., Schkolnik, G., Falkovich, A., Rudich, Y., Claeys, M., Pashynska, V., Vas, G., Kourtchev, I., Vermeylen, R., Hoffer, A., Andreae, M. O., Tagliavini, E., Moretti, F. and Artaxo, P.: Characterization of the organic composition of aerosols from Rondonia, Brazil, during the LBA-SMOCC 2002 experiment and its representation through model compounds, Atmos. Chem. Phys., 6, 375-402, 2006.

Kapustin, V. N., Clarke, A. D., Shinozuka, Y., Howell, S., Brekhovskikh, V., Nakajima, T. and Higurashi, A.: On the determination of a cloud condensation nuclei from satellite: Challenges and possibilities, Journal of Geophysical Research-Atmospheres, 111, D4, 2006.

Kinne, S., Schulz, M., Textor, C., Guibert, S., Balkanski, Y., Bauer, S. E., Berntsen, T.,

ACPD

9, C10576–C10586, 2010

Berglen, T. F., Boucher, O., Chin, M., Collins, W., Dentener, F., Diehl, T., Easter, R., Feichter, J., Fillmore, D., Ghan, S., Ginoux, P., Gong, S., Grini, A., Hendricks, J. E., Herzog, M., Horowitz, L., Isaksen, L., Iversen, T., Kirkavag, A., Kloster, S., Koch, D., Kristjansson, J. E., Krol, M., Lauer, A., Lamarque, J. F., Lesins, G., Liu, X., Lohmann, U., Montanaro, V., Myhre, G., Penner, J. E., Pitari, G., Reddy, S., Seland, O., Stier, P., Takemura, T. and Tie, X.: An AeroCom initial assessment - optical properties in aerosol component modules of global models, Atmos. Chem. Phys., 6, 1815-1834, 2006.
Lesins, G. and Lohmann, U.: Using MODIS and AERONET to determine GCM aerosol size, J. Atmos. Sci., 63, 4, 1338-1347, 2006.
Martensson, E. M., Nilsson, E. D., de Leeuw, G., Cohen, L. H. and Hansson, H. C.: Laboratory simulations and parameterization of the primary marine aerosol production, Journal of Geophysical Research-Atmospheres, 108, D9, 2003.
Nemesure, S., Wagener, R. and Schwartz, S. E.: Direct shortwave forcing of climate by the anthropogenic sulfate aerosol: Sensitivity to particle size, composition, and relative humidity, Journal of Geophysical Research-Atmospheres, 100, D12, 26105-26116, 1995.
Odowd, C. D., Smith, M. H., Consterdine, I. E. and Lowe, J. A.: Marine aerosol, sea-salt, and the marine sulphur cycle: A short review, Atmos. Environ., 31, 1, 73-80, 1997.
Pierce, J. R. and Adams, P. J.: Global evaluation of CCN formation by direct emission of sea salt and growth of ultrafine sea salt, Journal of Geophysical Research-Atmospheres, 111, D06203, doi:10.1029/2005JD006186, 2006.
Pilinis, C., Pandis, S. N. and Seinfeld, J. H.: Sensitivity of Direct Climate Forcing by Atmospheric Aerosols to Aerosol-Size and Composition, Journal of Geophysical Research-Atmospheres, 100, D9, 18739-18754, 1995.
Remer, L. A., Kahn, R. A. and Koren, I.: Aerosol indirect effects from satellite: Skeptics vs. optimists, Geochimica Et Cosmochimica Acta, 73, 13, A1088-A1088, 2009.
van der Werf, G. R., Randerson, J. T., Collatz, G. J., Giglio, L., Kasibhatla, P. S., Arellano, A. F., Olsen, S. C. and Kasischke, E. S.: Continental-scale partitioning of fire

ACPD
9, C10576–C10586, 2010

emissions during the 1997 to 2001 El Nino/La Nina period, Science, 303, 5654, 73-76, 2004.
van der Werf, G. R., Randerson, J. T., Giglio, L., Collatz, G. J., Kasibhatla, P. S. and Arellano, A. F.: Interannual variability in global biomass burning emissions from 1997 to 2004, Atmos. Chem. Phys., 6, 3423-3441, 2006.

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