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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observationally-constrained estimates of global small-mode AOD" *by* K. Lee and C. E. Chung

Anonymous Referee #3

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This paper presents an integrated global aerosol product covering the period of 2001-2010 at 2.8 deg resolution. The product includes several aerosol parameters of AOD, SSA, AAOD, AE, and sAOD constructed from two satellite observations from MODIS and MISR, ground-based measurements from AERONET, and a global model simulations from GOCART. AOD and AE are obtained by several steps: first, the satellite product bias is quantified by comparisons with AERONET; second, a "bias-corrected, gap-filled" global map is produced by adding or subtracting a global mean bias, identified in the first step, to the satellite data and by filling the data gaps with the GOCART model simulations; finally, the "best" map is finalized by incorporating the AERONET data into the product. Three methods have been tested for obtaining sAOD (AE-FMF relationship from AERONET and applied to global AE product; integration of FMF; or integration of sAOD), with the best one found to be the one derived from the AE-FMF

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relationship. On the other hand, since satellite data do not have SSA or AAOD, they are mainly from the GOCART simulation and "corrected" by the AERONET retrieval. Finally, the global and regional trend of AOD and sAOD in the past decade are assessed using the integrated data.

I found the paper is interesting and well written, and the methods and steps are clearly described and easy to follow. However, while it is a good idea and nice effort to estimate the AOD and sAOD changes on global and regional scales, there are several major issues in the approach and product uncertainty that need to be addressed. Therefore I rank this manuscript in the "major revision" category. A few major points are listed below followed by specific comments.

1. Satellite data bias against AERONET: the comparisons shown in Figure 2 are somehow mismatch, as the satellite data are averaged on 2.8 deg lat x 2.8 deg lon but the AERONET data are still the "point" measurements. A minimum effort should be to average the AERONET data that fall into the same 2.8x2.8 grid, then compare. I actually don't understand the reason for choosing the T42 resolution (2.8x2.8 deg); the level-3 MODIS and MISR products are all at higher spatial resolution (1x1 deg for MODIS, 0.5x0.5 deg for MISR) and it would make more sense to choose, for example, 1 deg resolution. Also, the accuracy and uncertainties of each observational product from MODIS, MISR, and AERONET should be listed from the original sources.

2. Bias correction: One single, global bias-correction value is applied to the satellite data to make them close to AERONET. This is far from sufficient since it is well documented that the satellite data have different accuracy and bias in different regions and seasons affected by the surface reflectance, cloud contamination, among others. Such a correction is especially problematic over the ocean, where essentially very little AERONET data are available.

3. AE and FMF fitting: The fitting shown in Figure 5 is not very good, especially the curve at AE> 1.5 that does not seem to represent the data. To me, a simple liner fitting

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for all points would be more accurate.

4. GOCART model: The model seems to play an important role in filling the areas where no satellite data are available. However I see several problems here. (1) The model output used in this study is from 2000-2002, which does not match the study period of 2001-2010. It might be ok for "climatology", but certainly is not suitable for studying the 10-year trend. (2) If the GOCART is used, its evaluation should be presented and bias assigned. This is particularly important, because at locations where no satellite data are available, the variables are solely from GOCART. (3) The "GOCART AE" in equation 1 is wrong – AE=1.4 for sea salt is way too high given most sea salt is in the coarse mode, and from that equation the maximum combined AE will be no higher than 1.7, which does not seem correct either. Why not directly use the AE from GOCART, which has been published (e.g., Chin et al., 2009)?

5. Regional analysis: The regional domains are not well designed for the purpose of this study. Most regional domains (shown in Fig. 1) are too big. At least the regions should be separated into land and ocean, polluted and remote, given the focus of this paper on sAOD trends.

6. Uncertainty: Many steps are involved in data manipulation to generate these global variables, yet there is no estimation of uncertainties. This is absolutely necessary, especially in the trend assessment.

7. AAOD and SSA: They are not used anywhere in the trend analysis, so they should be removed from the manuscript.

8. Other works: There are many publications assessing the satellite data quality and trends, including a recent, thorough work by Zhang and Reid (2010) regarding the MODIS and MISR data bias and uncertainties. They should be discussed and compared with the present study.

9. Product accuracy: It has been emphasized several times that the present study pro-

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vides the global integrated products at the accuracy of AERONET. This is misleading and incorrect, considering the above points in uncertainties and assumptions in the approach.

Specifics:

P 31664, line 2-4: the sentence “an area average of the small-mode. . .aerosol emission” does not make sense.

P 31665, line 10: How big is “big in size”? Give a number.

P 31665, line 12-14: It is too general to state that “anthropogenic aerosols tend to have lower SSA than natural aerosols”. Sulfate has very large anthropogenic fraction but it is non-absorbing. Aerosol from wild fire is natural but is quite absorbing.

P 31666, line 3: It is over optimistic that this work is able to provide the AOD and sAOD at the level of the AERONET data accuracy. What are the accuracy of AOD and sAOD from AERONET to begin with? How can that accuracy be achieved from your approach with the problems I mentioned earlier?

P 31666, line 4: The sentence of Chung et al. 2005 presenting a global AOD estimate at the AERONET accuracy is inflated.

P 31666, line 10: “independent” from what?

P 31667, line 10-11: Why using median would reduce the cloud contamination?

P 31667, line 26: Uncertainties in MISR sAOD, mAOD, lAOD, and SSA are quite large and those values at a lot of times are qualitative. What are the data quality flags associated with them? Do you select those data according the quality flags?

P 31668, line 21-24: You should choose the mostly commonly available wavelengths (e.g., 440 and 870 pair) instead of changing the wavelength, or give some evaluation of the consistency of 550 nm AOD interpolated from different pairs of wavelengths.

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P 31668, line 26-27: Linear extrapolation of SSA between two wavelength is not correct. You should first do the logarithmic interpolation of AAOD, then calculate SSA from (1-AAOD/AOD.

P 31669, section 2.4: GOCART model appears here rather suddenly, because it has not been introduced in the abstract and introduction that a model is also involved in the data integration.

P 31669, line 7-8: it is not correct that the results are archived . . .form January 2000 to July 2002. This is probably what you have, but the GOCART is available on the Giovanni website with much longer period and higher spatial resolution.

P 31669, 2nd paragraph: These SSA values are at what wavelengths?

P 31669, GOCART AE: As I mentioned earlier, GOCART AE is published 3 years ago.

P 31670, line 1-2: "biomass burning aerosol AE of 1.6 roughly translates into 1.65 for BC and 1.31 for OC": this is wrong. This would imply that 85% biomass burning AOD is from BC, which is unrealistic. 15% is more likely.

P 31670, line 5: Why the accuracy of GOCART is not a major concern? If GOCART is used to fill the observation gaps, should the accuracy be a major concern because at those places all you have is from GOCART?

P 31670, line 8: ". . .provide the best estimates of SSA and AAOD": How can you do so if they are mainly from GOCART (AERONET is "sparse") and you don't even concern the accuracy of GOCART?

P 31670, line 11: You should introduce Fig. 1 first before Fig. 2.

P 31670, section 3.1: The satellite data accuracy should be much better and quantitatively addressed. "small differences", "generally negative" does not provide any information on the accuracy. Are the differences over land? ocean? or everywhere? If you want to achieve AERONET accuracy everywhere, you will have to quantify the

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differences by locations/regions.

P 31671, line 17-25: The references here are for the older versions of MODIS and MISR. You should use the numbers and references for the versions you use in this study.

P 31671, line 28: Are these "global and decadal mean values" obtained from the same locations, i.e. over AERONET sites? Mostly over land? How do you get the global average AOD from AERONET, given the "sparse coverage", as you stated, of the AERONET locations?

P 31672, line 21-22: MISR and MODIS have different accuracy at different land regions and you should not always assume MODIS>MISR.

P 31672, line 24 and 29: As I mentioned earlier, a global number for bias correction is not sufficient and will not give you the "accuracy of AERONET".

P 31673, line17: Do you have any reference regarding the unrealistic MISR SSA?

P 31674, line 7: How large is "very large" AAOD? Give a number.

P 31674, line 12-13: I wonder why can't you use the same/similar wavelength pair, e.g., 470 and 660 nm, for both land and ocean?

P 31674, line 18-20: I disagree with your description on the land/ocean contrast from Figure 4 - you should provide at least some quantitative measure of this discontinuity. The major differences between MODIS and MISR are over ocean where MISR is significantly higher; not so much of the land/ocean contrast.

P 31675, last paragraph: As I pointed out earlier, the blue line in Fig. 5 does not fit the data well, far from "the most realistic best-fit". This fitting should not be used to represent the AE-FMF relationship for AE>2 (or 1.5 in the figure).

P 31677, line 3-4: The quality of MODIS FMF over ocean is much better that that over land (see MODIS publications). Using a universal number of 1.4 should not apply to

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ocean if the comparisons are mostly done over land with AERONET.

P 31677, section 4.3: Similarly, one universal number adjustment is not correct.

P 31678, title of section 5: Why the trends should be linear? They are not necessarily linear from Figure 7.

P 31678, line 18-20: You attribute the changes to “South America” and “South Asia”, but no one can tell from the Figure – It is the ROW that determines the change of both AOD and sAOD. Maybe you should have more regions in the analysis, not just ROW.

P 31678, line 24: Why the economic recession in 2008 made the sAOD in Europe reduced but not in North America?

P 31679, line 3-4: Both AOD and sAOD show increase trends in the first several years over those regions, not a monotonic decrease.

P 31679, line 9-11: This region is too large. You should separate land and ocean to say something about trends over China.

P 31679, line 15, last sentence in this paragraph: Why is area-average sAOD estimates are the novelty of this study? This is self-praising.

P 31679, line 17-21: Did anyone report an increase of PM2.5 over North America or Europe from 2001 to 2003?

P 31679, last 3 lines: If reduction of carbonaceous aerosols is not a good explanation, then what? Does it make sense that all the increases of sAOD are from non-absorbing aerosols?

P 31680, line 2: India and E China are not the only place with pronounced increase of sAOD. Arabian peninsula, Africa and even large part of oceans have significant increasing "trends".

P 31680 line 2-3: The most significant reduction of AOD is in the very eastern part of

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China, Central Asia, SE Asia, and part of southern oceans. Western US and Canada show increase, not widespread decrease.

P 31680, line11: It is important to give uncertainties in the product if you intend to let it be used to “validate aerosol simulations”.

P 31680, line 14: “more accurate than pure satellite-based values”: What is the evidence? You should at least compare your AOD with that from the satellite-based data from Zhang and Reid (2010).

P 31680, line 16-17: Not all sAOD is anthropogenic.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31663, 2012.