

***Interactive comment on “AERONET-based microphysical and optical properties of smoke-dominated aerosol near source regions and transported over oceans, and implications for satellite retrievals of aerosol optical depth” by A. M. Sayer et al.***

**Anonymous Referee #4**

Received and published: 21 January 2014

Review comments on AERONET-based microphysical and optical properties of smoke-dominated aerosol near source regions and transported over oceans, and implications for satellite retrievals of aerosol optical depth

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This paper presents an updated analysis of AERONET derived aerosol properties in

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regions where smoke aerosol is present. This has value in the sense that such properties are needed for aerosol models in satellite retrievals and climate models, but is essentially an update of several previous papers. Although there is discussion of improvements made to the AERONET database since Dubovik et al (2002), it is not made clear whether any of these improvements would be expected to influence the aerosol properties such that a reanalysis of largely the same data set is justified. It may be that such an analysis is justified, but there is not a compelling argument made in the text. I would recommend the authors make it clear what benefits their analysis has over previous ones, and the Editor consider how much this part is incremental.

The study then attempts to compare the properties close to source with those of smoke aerosol transported over the ocean. This too would have value for improving the optical models of aerosol, but unfortunately the data available from AERONET appears to be far from sufficient to make this comparison. Climatological properties of near source smoke are compared to occasional smoke transport to coastal and island AERONET sites. Observations at remote sites are linked to source via HYSPLIT (what resolution and winds are used etc) and other sources. A validation of the “subjective” method for finding matches should be undertaken, i.e. find the times when Buenos Aires is more similar to Alta Foresta or Cuiaba and then look at the paths to see if this identification makes sense. It’s not obvious that this has been done. The largest outlier in this analysis is Ascension Island. It is stated that there can be more frequent instrument problems here than at other sites, but that the data has more post processing because of this. Why is Ascension Island chosen for the section on implications for satellite retrievals when the evidence for transport affecting this site is not very strong?

The final part of the study is to assess the likely implications of these errors in the existing microphysical models in satellite retrievals. Again, it’s apparent that the methodology adopted doesn’t exactly match the aim. Page 25034 states “To test the effect of aerosol absorption on satellite measurements. . . ” and then later on the same page “ . . . this does not mirror who the individual satellite algorithms mentioned previously

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function". Why is the appropriate methodology to use the revised properties in a full retrieval test not used here?

Summary: This was a difficult paper to review. The subject area is important, and the paper potentially offered much, being from a team with considerable expertise here. However, in order for it to be published in ACP I do think it requires a reasonable amount of rewriting in order to justify and better explain the methodologies used, and link back to the literature more fully.

Specific points Page 25017 line 2: I believe there is also a missing reference to Abel et al (2004) where the evolution of biomass burning aerosol properties is observed in aircraft measurements, although there are more recent publications that also discuss this, and the mechanisms responsible remain a matter of some debate.

Page 25019 line 16: I find the reference to Sayer et al, 2012b here somewhat spurious since these equations are in many textbooks, which should be cited instead if indeed a citation is needed.

Page 25023 line 21. Is it acceptable to discuss the table here and describe the figures as being "illustrated later"? surely there should be a reference to a figure, or indeed the figure described at this point.

Page 25026 line 18 change "with" to "by"

Page 25031 line 11. Although the coarse mode errors are unlikely to be significant for AOD, they would be for the phase function which is important for the backward scattered radiation measured by satellites.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25013, 2013.