We thank Reviewer 4 for their comments on our manuscript. Below we have reproduced their comments in *italic*, and our response in regular type.

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

A.M. Sayer, N.C. Hsu, T.F. Eck, A. Smirnov and B.N. Holben

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

In the revised manuscript, we have tried to make clearer the justification for the analysis, and the improvements and changes since the prior AERONET version 1 analysis of Dubovik et al. (2002), hereafter D02. As part of this, much of the Introduction, AERONET description, and discussion have been changed, to better highlight this. For example, some differences are observed in the aerosol microphysical/optical properties at locations analysed previously by D02, and we also considered biomass burning regimes not examined previously by D02—which is important if one wants to try and create a set of optical models encompassing the global variability of biomass burning properties. In the revision we have also extended the manuscript to include, for all sites, some aspects not considered by D02:

- Discussion of absorption Ångström exponent.
- Analysis of how well the climatologies for each site represent the optical properties retrieved at each site (i.e. intra-site variability)
- Analysis of how well the climatologies for each site represent the optical properties retrieved at the other sites (i.e. inter-site variability and distinguishability)
- Extension of optical models to include UV wavelengths
- Calculation of lidar ratios for common lidar wavelengths, and their AOD-dependence
- Two additional sites were added to the discussion. Firstly, Tomsk 22 serves as a useful secondary site for sampling of smoke in Siberian, alongside Yakutsk, both of which had smaller data volumes than most of the other sites considered. Secondly, we added analysis of summertime burning at Moscow from the years 2002 and 2010, which was more strongly influenced by peat burning than the other boreal sites. Although we realise that Moscow will have some local urban contributions to the aerosol loading too, we mention this limitation, and feel that our data selection gives confidence that smoke is optically-dominant for the selected AERONET inversions.

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 Aries 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.*

This section has been rewritten for clarity in the revised manuscript (see also our Response to Reviewer 3). Any such mapping of this type is inherently subjective to some extent, and we provided two examples (Figures 7, 8, and 9 in the original manuscript) illustrating the methodology, along with several peer-reviewed journal articles detailing other smoke events corresponding to dates used in this paper, which were used to aid in the determination of whether an AERONET inversion at one of these remote sites is likely to be influenced by smoke or not. Although only these examples were presented due to article length concerns, the determination was made on a case-by-case basis, and dates were provided (Table 5 in the original manuscript) with the hopes that these cases may then be useful to other researchers. As stated, the intent was not to perform a one-to-one mapping between smoke sources and receptor sites for each individual inversion, but rather illustrate that the optical properties fall within the range of variability (i.e. it does not matter particularly whether an individual inversion at Buenos Aires sampled smoke from sources near to Alta Floresta or from Cuiaba, rather that the optical properties are similar on aggregate to inversions from these sites). We feel that this is sufficient to back up our assertions, and hope that the reviewer would agree with this based on our revised manuscript, particularly now that we have focussed it more clearly on optical rather than microphysical properties.

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?

Ascension Island was chosen because this is the most well-known remote ocean site sampling transported smoke with limited local aerosol sources, despite the occasional AERONET instrument problems at this site. Transport of smoke to the central Atlantic (where Ascension Island is located) is well-supported by studies, including some cited in the manuscript. We have added some more references about African aerosol transport to this region in the revised manuscript. Other coastal/island sites tend to have smaller data records and/or be more susceptible to the influence of other aerosol types, which we felt may confound the analysis. In the revised manuscript, when discussing satellite AOD errors, we have been more restrictive about the data used in the equivalent of the original manuscript's Figure 14 (discarding low-AOD cases), and updating to the newest version of the MODIS data product (released since the submission of the original version of the manuscript); we hope that this will present a more compelling argument.

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

Each of the satellite AOD retrieval algorithms we discussed in this section is created and maintained as part of ongoing funding, involving teams of people working for years at a time on new versions. Reproducing individual algorithms exactly with new smoke optical models is therefore beyond the scope of this study. Additionally, we feel that the current approach (simulation for individual bands) is more illustrative of the issue at hand (i.e. insufficient absorption) as it allows the effects of this algorithmic assumption to be assessed individually, while on larger-scale application other factors (e.g. sensor calibration, cloud screening, surface reflectance, spatial/temporal variability, ability of the algorithm to pick an appropriate aerosol optical model) also contribute to the satellite discrepancy against AERONET. Our simulations may also be of use for other algorithms designed to be applied to these sensors, or algorithms designed for other sensors, but using similar wavelengths. Again, this section has been rewritten in an attempt to clarify this. We would also like to note that we discussed this analysis with the respective aerosol algorithm teams prior to submission of the manuscript, and those of us (Sayer, Hsu) involved in satellite AOD retrieval algorithm development are planning on incorporating these models as candidates in our own future algorithms.

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.

In the year since the original submission of the manuscript was put together, the paper has been rewritten extensively, following the numerous helpful comments from the reviewers. This has included edits for clarity, additional discussion of other literature on the subject, and addition of new analysis (as discussed above). We hope that the reviewer will agree that the revised manuscript will present a useful resource for optical models of smoke aerosols, for use in radiative transfer applications such as those required by satellite AOD retrieval.

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.

We did not find Abel et al. (2004) but did find Abel et al. (GRL, 2003), which we think the reviewer may be referring to, and have added that in to the text at the location. Some additional references are also included in the revised version of the manuscript.

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.

We cited Sayer et al (2012b) here as the notation used for the present study is the same as that for our prior study in which some of these relationships are derived—other sources, while presenting the same relationships/derivations, sometimes use different terminology and notation. However, this section has been shortened in the revised manuscript, as we felt that some of the definitions could be removed to save space, given the other significant extensions to the paper.

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.

The ‘illustrated later’ comment has been removed in the revised version of the manuscript as we felt that, with the changes to this section, it was no longer necessary to specify. In the original manuscript no reference was given as otherwise figures would have to be cited out of order, or reordered, which we felt would harm the flow of the paper.

Page 25026 line 18 change “with” to “by”

This change has been made in the revised manuscript.

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.

We agree that coarse mode properties are likely more significant for phase function calculations. The revised manuscript now includes some discussion of asymmetry parameter and of lidar ratio, which are related to this topic.