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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical characterization of long-range transported biomass burning particles from North America at three EARLINET stations" *by* Pablo Ortiz-Amezcua et al.

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The authors thank the reviewers for the efforts, time and the thorough review of our manuscript. Please, find below a detailed response to the reviewer's comments.

General comment: The paper discusses the microphysical properties of long-range transported biomass burning from N America within Europe, as determined from lidar measurements. The paper is in general clearly presented and the results properly discussed. The paper can be published after minor revisions.

Specific comments:

- pp 5, line 25 and fig 1; please define "smoke surface concentration"; is it PM?

Answer:

The right panel in Fig. 1, provided by NAAPS (http://www.nrlmry.navy.mil/aerosol/), corresponds to a forecast of the smoke concentration at the surface level. According to Rubin et al. (Atmos. Chem. Phys., 16, 3927–3951, 2016), smoke emissions from biomass burning are derived from satellite-based thermal anomaly data used to construct smoke source functions via the Fire Locating and Modeling of burning Emissions(FLAMBE) database. In order to make it clearer, we included a new sentence in the manuscript: "NAAPS (Navy Aerosol Analysis and Prediction System) model of Marine Meteorology Division, Naval Research Laboratory (NRL), (http://www.nrlmry.navy.mil/aerosol/) was used for forecasting aerosol optical depth and particle density of smoke at the Earth's surface, using smoke emissions derived from satellite-measured thermal anomalies." (pp 4, lines 16-19 in the new version).

- fig 3; please comment/explain why Warsaw data are not from the same smoke episode as for Granada and Leipzig; no measurements available? it would have been preferable to analyze the same smoke (i.e. having the same origin in time and space); also, there were no data available in Leipzig at the time of measurements in Warsaw; I would expect the smoke be seen both in Leipzig and Warsaw; was it a Calipso overpass constraint?

Answer: During end of June and July 2013 several events of smoke transport were observed. The particle properties for those smoke events were varying, as the transport paths were not the same. Even for the cases at Leipzig and Granada, it is shown in the manuscript that the exact source regions may be different (either Canada or East USA). It is true that we employed the same CALIPSO overpasses to track the plumes for both stations (due to its availability), but we did not try to mean that the same event was detected.

We agree that an interesting idea would be to analyze the same smoke arriving at

different stations, but the aim of this work is to characterize three different events of transatlantic smoke transport that happened within a certain time period, and that presented close sources and transport paths, in order to highlight similarities and differences among them.

About the data availability, we performed a search in the databases of each station and the presented cases were the existing measurements coinciding with smoke detection and that could be analyzed (unfortunately, not all the measurements can be analyzed because of cloud cover, signal instabilities, etc.).

Therefore, we welcome the suggestion, although we will not be able to include measurements of the same events measured at different stations.

- pp 11, line 10: please check LR for 532 for GR; according to Table 2, LR for GR should be around 37 (82/2.2).

Answer:

We checked the results of our optical profiles, and we found that indeed, the value LR532= 51$\pm$11 sr for Granada was a misprint, and the right value is 47$\pm$11 sr. However, it still does not apparently coincide with the value obtained by directly dividing (82$\pm$16)/(2.20$\pm$0.09) = 37$\pm$9 sr from Table 2 (although they are not dramatically different taking into account uncertainties). This fact is the result of the different smoothing and procedure applied: we obtained LR profiles (as in Fig. 8) from the ratio of α and β profiles, each one retrieved with different smoothing as a consequence of the signals involved and the method used for retrieving each property; then, the average of the layer was taken from each individual profile, obtaining the values shown in Table 2. We think this is a more trustable procedure than directly dividing mean values, since we would then be involving different smoothings.

- pp 15, after lines 21; please comment on large differences for IRI between lidars and sun-photometers retrievals; Aeronet retrievals show larger IRI for GR and WA (around

90% difference with lidars) and much smaller IRI for LE (around 500% difference wrtlidars); different IRI are clearly reflected in different SSA; also, there are large difference in the concentration values as calculated from lidar and retrieved from sunphotometer

Answer:

It is true that the relative differences between lidar and sun-photometer retrievals are large, and it was not discussed in the text: we have now included a comment on it (as suggested) in the manuscript: “Imaginary parts of refractive index values (IRI) showed larger differences with respect to values retrieved with lidar, what is also reflected in SSA. However, the SSA discrepancies remain less than 7% and then still represent low particle absorption.” (pp 15, lines 21-23 new version).

It is still important to notice that the comparison cannot be very strictly done, considering several points: firstly, that the differences between around10-3 (lidars) and around 10-2 or around 10-4 (photometers) do not imply too much difference in SSA (less than 7%), what means that in those ranges, the particle size distribution seems to play a more important role for the calculation of SSA than IRI; secondly, that according to Dubovik et al. (J. Geophys. Res., 111, 1984-2012, 2006), the uncertainties in IRI for small particles can be large. Additionally, one has to be careful when comparing particle microphysical properties retrieved for a certain altitude (as in lidar retrievals) and those retrieved for the whole atmospheric column (as AERONET retrievals), since the second retrievals include information about other aerosol layers in the atmospheric column not accounted in the lidar analysis at a specific layer. As commented in the manuscript, the fact that most of the properties are similar between lidar and AERONET retrievals mean that for the analyzed cases the columnar properties seem to be strongly influenced by the detected smoke layers, but it may not necessarily mean that all properties exactly coincide.

Concerning the concentration values, we would like to clarify that the values included in Table 4 (and named Cv), in Tables 5 (named VL) and in Table 6 (named V) do not

correspond to the same magnitude. Cv stands for the particle volume per unit air volume, and thus it is a magnitude defined for the single altitude we are investigating; VL and V are height-integrated magnitudes, and thus refer to particle volume per unit air area, integrating only over the smoke layer to obtain VL, or over the whole atmospheric column to obtain V. We included these different magnitudes in the tables in order to show the peak concentrations Cv (Table 4) and to assess the impact of the smoke layers (Table 5) on the whole column (Table 6). The percentages written in pp.15, lines 5-8, were indeed calculated dividing values in Table 5 over the ones in Table 6.

- pp 17, line 4: concerning the similarity for the intensive properties in the smoke layers... it looks to me that there is a good similarity for effective radius and RRI but not for IRI; please reconsider

Answer:

According to the answer to the previous comment, and with the sentences added in the corresponding section, the issue related to IRI similarity is also solved. We have included in the conclusions (page 17) that “the majority” of the properties are similar (referring to the discussion in the previous section).

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