Dear Editor,

First, we would like to thank the reviewer for all the remarks. We made important modifications in the new version, mainly by removing part 3.3.3, focused on the satellite observations (see details bellow). We also re-worked the AERONET part following the different points highlighted by the reviewer.

Anonymous Referee #1

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This is an interesting paper in which aerosol absorption over the Mediterranean Basin is studied by considering data from several AERONET stations and from satellites. The authors find that, in addition to mineral dust and biomass burning smoke, organic carbon absorbs a significant amount of solar radiation. The authors performed a rigorous analysis of AERONET data, however I cannot find myself in some of the conclusions they draw. The sections on satellite data should not be published in their current form, as the large uncertainties of the data sets do not allow quantitative statements as presented by the authors. First, it should be clarified where the large differences between the three satellite data sets come from and which data set is most suited (which may depend on aerosol characteristics, surface type, or other parameters).

In summary, I recommend this manuscript for publication in ACP after the authors' consideration of the minor comments listed below and in the annotated manuscript (see supplementary material) — but only after thorough revision (or even omission) of the sections pertaining to satellite measurements.

As mentioned in the first version of the article, the analysis of satellite data included in this article was a first exploitation of SSA satellite observations over the Mediterranean basin. The aim of this work was to present the regional SSA pattern obtained over this specific region from satellite observations, which is complementary to local AERONET data, and to present possible geographical gradient depending on the aerosol types. Anyway and as mentioned by the reviewer, this part is not robust enough in its present form. We already attempted to evaluate SSA in the MISR, MODIS and OMI products over this region using AERONET data (MPASS website) but Level 2 data conjoined with satellite over passing was not sufficient for producing relevant statistical estimations. This is why we did not present AERONET versus satellite SSA comparisons in this work.

To our knowledge, a rigorous evaluation of SSA MODIS-DB, OMI and MISR satellite products does not exist at the present time (except the work of Jethva and Torres, ACP 2011, for OMI) and clearly requires an enlarged number of AERONET sites (certainly at the global scale), including AERONET sites associated with large aerosol concentrations (AOD higher than 0.4 in the visible bands) and affected by different aerosol types (smoke, dust and pollution). In that respect, the amount of data over Mediterranean basin seems to be not sufficient. Therefore, we have decided to remove the last part of the article focused on satellite analyses (3.3.3).

We plan to reanalyze these results and possibly submit a second article focused on satellite data analysis. A specific study (evaluation of SSA from MISR, MODIS Deep Blue and OMI) is now ongoing using a large number of AERONET sites as mentioned below but is beyond the scope of the present study. Hence, we have re-written the introduction and conclusion, and changed the title of the article which is now: «<u>Absorption properties of Mediterranean aerosols obtained from multi-year ground-based remote sensing observations</u>».

Minor comments

1. p.9276, l.26 (and Table 3) — In the table it says that you use only OMI data from 2005-2008. Why? Is this to avoid the row anomaly? Why don't you use the non-affected data from the time period 2008-today ?

At this stage, the OMI data for the period 2008 to today was not avalaible compared to MISR and MODIS Deep-Blue products.

2. p.9279, l.18 — What do you mean by "fossil fuel Black Carbon"? The way it is written here, it looks as though BC from fossil fuel is somehow different from BC from other sources.

This refers to Black Carbon emitted directly at the source from incomplete combustion processes such as fossil fuel. The AAE value close to 1 represents the theoretical AAE value for black carbon (Bergstrom, 1973; Bohren and Huffman, 1983). We have modified this point in the new version.

3. p.9280, ll.1-6 — A reference to a recent study by Jethva and Torres (ACP, 2011) would be in place here

We have now added this reference in the new version (part 3.2.1) using the following sentence : « In parallel, the presence of OC in biomass burning plumes has been confirmed by OMI satellite observations (Jethva and Torres, 2011)."

4. p.9283, ll.7-9 — Are there any in situ measurements showing the various contributions of OC, BC, etc. to the aerosol mix? It would be interesting to compare with the findings from AERONET.

To our knowledge, there is no chemical observations over Barcelona that could be used to interpret these results on AAE. Anyway, Russell et al. (2010) report an AAE of about 1.5 for biomass burning aerosols. In parallel, results obtained from in situ measurements of aerosols by absorption photometer, nephelometer, and aerosol mass spectrometer on the C-130 mostly over Central Mexico during MILAGRO (Shinozuka et al., 2008) reveal that AAE increases with increasing organic fraction of submicron non-refractory mass. In their work, an AAE of about 1.5 corresponds to an organic fraction of about ~0.4. This reference is now added in the new version.

5. Section 3.3.1—The authors make a lot of informative comparisons between their results and those from previous studies. However, it should be mentioned what measurements are compared: single values, campaign averages, or even yearly means?

All those different points (single values, campaign, etc..) are now clearly specified in the new version of the manuscript (section 3.3.1).

6. p.9285, ll.24-27 — I do not think this is a sound conclusion, certainly not if the error bars (or standard deviation of the measurements) are taken into account. To me, the results for the Eastern and Western AERONET sites shown in Fig. 12b are identical.

We agree with this remark and we have now removed the part associated with satellite observations in the new version (part 3.3.3).

7. p.9287, ll.4-10 — The authors cite a study by Kahn and co-workers which shows that MISR SSA is unreliable for small AOD, yet they do use these numbers in their analysis. The MISR data looks very unreliable, probably due to the effects mentioned in Kahn's study; in particular, the north-south gradient observed in OMI is reversed in MISR, and SSA over the desert is very close to 1. In addition,

there appears to be a land/ocean issue, indicated by the unphysically large SSA gradient at the coast. These issues need to be addressed before any meaningful quantitative analysis can be performed.

We agree with this remark also and as mentionned previously, the satellite SSA observations have been removed (part 3.3.3). A specific study (evaluation of SSA from MISR, MODIS Deep Blue and OMI) is now ongoing using a large number of AERONET sites but is beyond the scope of the present work.

8. p.9289-9290, ll.28-29 and ll.1-8 — The authors over-interpret the satellite data; as mentioned above, the correctness of those data needs to be investigated and the comparison between various instruments improved before any quantitative analysis can be performed.

See previous answer.