

Interactive comment on “Absorption properties of Mediterranean aerosols obtained from multi-year ground-based and satellite remote sensing observations” by M. Mallet et al.

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 The sections on satellite data should not be published in their current form, as the large uncertainties of the data sets do not allow quantitative statements as presented by the authors.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

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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.

Minor comments

1. p.9276, I.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?
2. p.9279, I.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.
3. p.9280, II.1-6 — A reference to a recent study by Jethva and Torres (ACP, 2011) would be in place here
4. p.9283, II.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.
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?

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6. p.9285, II.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.
7. p.9287, II.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.
8. p.9289-9290, II.28-29 and II.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.