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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatology of aerosol optical properties and black carbon mass absorption cross section at a remote high altitude site in the Western Mediterranean Basin" *by* M. Pandolfi et al.

Anonymous Referee #1

Received and published: 26 March 2014

- "**General comments**".

  The manuscript presents two-years in-situ measurements of aerosol optical properties at a European mountaintop observatory in the western Mediterranean Basin, run according to ACTRIS standards. This is a unique and novel dataset that is worth considering.

  Data are compared to a regional background station run according to ACTRIS standards. Extrapolated data of in-situ aerosol properties in the free troposphere are compared to other mountaintop sites. Measurements show the site to be

  in the medium/upper range of values measured at other similar European sites. African dust and regional recirculation during the warmer seasons are found to be the major sources of the relatively high values observed, indicating the potential impact of these two sources in the whole lower troposphere of the western Mediterranean. This is a substantial issue addressed by the manuscript.

  Despite that, I do believe that the manuscript still deserves a deeper analysis to fully reach the scope of the work, and substantially improve conclusions. Additional relevant scientific questions should be addressed: in particular, an important point is that concerning absorption properties and Black Carbon sources that are merely presented, but barely discussed.

  I therefore recommend publication of the manuscript after taking into consideration (at least) the following specific comments.

- "**Specific comments**".

  - Pag. 3790, line 24: *Data measured were scaled to a wavelength of 550 nm by using 1 as AAE* (Absorption Angstron Exponent). The AAE=1 value represents an aerosol with optical properties of pure Black Carbon: that means that the absorption is totally dominated by Black Carbon, and that there are no additional absorbing compounds, such as Brown Carbon or dust. Both brown carbon and dust can increase AAE, indeed. AAE also varies with aerosol size. By using AAE=1, authors constrain results. In fact, AAE might be much larger than 1, the absorption coefficient at 550 nm resulting larger than the value calculated here. The comparison to other similar sites (fig.4) may result in different conclusions. Authors should discuss the point.

  - Pag. 3794, line 4-17: Authors observe conditions of low PM1 concentrations coupled to low Single Scattering Albedo and low g. They associate these conditions to *small particles with higher absorption properties*, similarly to AND2011 associating *low SSA at very low aerosol loadings with an aerosol mixture in which*

large scattering particles are removed and relatively smaller and darker aerosol left. This is an interesting point, that may be carefully analyzed. It would be interesting to speculate on possible sources causing these aerosol properties at the mountaintop observatory examined here. These sources of Black Carbon may be discussed after due consideration of their coupling to African dust and summer regional recirculation (pag.3797, lines 4-6). The question of possible Brown Carbon sources (e.g., associated to summer regional recirculation) increasing AAE (see previous comment) may also be added. This has the potential to improve the understanding of polluting scenarios affecting the whole lower troposphere of the western Mediterranean.

- The paragraph 3.1 merely presents data, with no or scarce discussion/interpretation. In particular, absorption properties should be discussed with more attention. In some parts of this paragraph, text can be reduced or eliminated (e.g., pag. 3787, lines 2-12). A table could summarize comparison with other similar studies (pag. 3787, from line 23). It would be nice to include in a separate paragraph this table and all text comparing to previous related work, with a clearer indication of the original contribution of this work.

- Pag.3791, line 25: It is stated that *there is a relative decrease in FT-data extensive properties , clearly being a function of seasons*. However, either fig.4 can't show this decrease because box plots are too small, or this is not correct. Please, comment on that. Reasoning for seasonal difference of the FT vs all-data extensive properties (other than scattering seasonal DC in the fig.5) should also be given.

- "**Technical corrections**".

  - The abstract seems to be ineffective in explaining the quality of the manuscript. I thus suggest to improve it. Most importantly, specific conclusions should be added.

- Correct *WAE* with *WREG* in tab.1.

- Add the site representativeness in tab.1 for MSC and MSY.

- Pag.3793, line 16: correct *from -2 to 6* with *from -2 to 4*.

- Titles of the subparagraphs 3.2 (*Diurnal cycles and cluster analysis*) and 3.3 (*FT vs. all data*) might be changed to describe contents of the paragraph in a clearer way.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3777, 2014.