Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "From air quality to climate: Impact of aerosol sources on optical properties at urban, regional and continental levels in the north-western Mediterranean" by Marina Ealo et al.

Anonymous Referee #2

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This study showed not only aerosol source contributions based on a positive matrix factorization (PMF) analysis, but also mass scattering and absorption efficiencies (MSE and MAE) of different aerosol sources by using multilinear regression method at urban, regional and remote backgrounds in the Spain. Although the results and discussion were documented well, there are several important shortcomings.

âĂć While the detailed pedagogical description of the approach is appreciated, the paper overall must be shortened. There are many repetitions in the text or not essentially needed with many references. This review strongly suggest that text must be made

more succinct. For example, in INTRODUCTION, the authors explained overall aspects of aerosols in climate-air quality research. Most of them are overstated and not directly related with the results of this study. The words "From air quality to climate" in title also should be removed. Summary and conclusions also should be shortened; highlight the major findings succinctly and provide directions/implications of this work.

âĂć Generally, source apportionment by PMF model shows the contributions of aerosol chemical properties from various sources, such as traffic (vehicle), biomass burning, dust (road dust), marine, industry, secondary nitrate, secondary sulfate, ship, etc. However, the sources given in this study are aerosol compositions, except for industrial/Traffic and Marine. Firstly, the authors should show the chemical compositions and discuss the characteristics during the study period. The explanation of major emissions sources of the aerosols also provided. Secondly, more detailed descriptions for source profiles should be given. For example, V-Ni at MSY originated mainly form shipping emissions (see section 3.3), why EC is not considered at the source profile.

âĂć It is not fully explained how the authors determined the scattering and extinction efficiencies (MSE and MEE) of chemical species by using the Multilinear regression method. Eqs. 1 and 2 are not enough. This reviewer strongly suggests that detailed steps for getting the MSE and MEE of chemical species by using the multilinear regression have to be present in main text or in the supplement.

âĂć Regarding to the reconstruction (section 3.5), what is the root-mean-square difference for data given in Figure 4? This reviewer suggests to use percentage difference of bias rather than fractional bias.

âĂć Long-term trends in Section 3.6: There is no detailed explanation on Figure 6. There is a critical issue that how we can trust these reconstructed values given in Figure 6 and associated trend analysis results. For example, Collaud Coen et al (http://www.atmos-chem-phys.net/13/869/2013/acp-13-869-2013.pdf) reported that, in the Europe, significant trends were not observed for aerosol optical properties at most

sites (see Table 8 in the paper). However, this study, this study reports about a scattering TR(total reduction) of -52% since 2004 and -44% in the absorption coefficient. Therefore, the authors should investigated all published literature, especially in Europe, and give a reasonable and reliable explanation with evidences. If there are such distinct decreases in aerosol scattering and absorption properties, is this trend also consistently seen from aerosol optical depth measurements from AERONET/SKYNET and satellite sensors? Also, do PM2.5 concentrations show a decreasing trend over the study period?