

Interactive comment on “AIRUSE-LIFE +: Estimation of natural source contributions to urban ambient air PM₁₀ and PM_{2.5} concentrations in Southern Europe. Implications to compliance with limit values” by Evangelia Diapouli et al.

Anonymous Referee #3

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General Comments

This paper deals with desert dust outbreaks in southern Europe, more specifically with the contribution of natural aerosols to mass concentrations measured in five urban environments in Southern Europe. This is an interesting work, well written and very well conducted, with results properly presented and examined (with the exception of the uncertainties on measured and calculated values). In this respect, I really appreciated the sensitivity analysis on the estimation of African dust contributions. However, if this study addresses some relevant scientific questions, many aspects of

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desert dust outbreaks in the Mediterranean environment have been broadly studied in recent years (e.g. Stafoggia et al., *Environ. Health Perspect.*, 124 (4), 413-419, 2016 and references therein or Calastrini et al., *Advances in Meteorology* (2012), <http://dx.doi.org/10.1155/2012/246874> and references therein). Therefore, the novelty of this work is limited anyway and it is difficult for me to assess the real contribution of this study to a better knowledge of the Mediterranean atmospheric environment. As the authors pointed out, in the studied urban areas, the natural contribution to the atmospheric particulate load during days in exceedance is very limited, except in Athens, which is not really new (see for example Grivas et al., *STOTEN*, 389 (2008) 165-177).

From a general appraisal point of view, I suggest to the authors to strengthen their discussion about uncertainties in the quantification of the natural contributions, to reinforce their conclusions, before considering publication of this work in a high ranked journal as ACP.

Specific Comments

Page 4, lines 9 to 13: Please add references about the BSC-DREAM8b and FLEXTRA models.

Page 5, line 13: Equation (2) is not the correct formula reported in the Marcazzan's study! In Marcazzan et al. (2001), the mineral dust concentration is reconstructed from: Mineral Dust = $1.15(1.89Al + 2.14Si + 1.67Ti + 1.4Ca + 1.2K + 1.36Fe)$.

Please check your “Min-Stoch” data to verify if they have been obtained with the equation (2) or with the original Marcazzan et al. (2001) formula.

Table 2 (page 16) and Table 3 (page 17): Please report uncertainties regarding mass contributions (g.m⁻³) and relative contributions (%) of natural sources to PM₁₀ and PM_{2.5} concentrations for the five studied cities.

Figures 6 to 9 (pages 22 to 24): They are clearly intercepts different from 0 in some reported regression lines, which are not considered in the regression equations. . .Could

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the authors examine and discuss the impact of these simplifications on their conclusions ?

Page 9, lines 4 to 6 and Figure 12 (page 25): They are undoubtedly no correlation between measured and calculated dust concentrations for concentrations below 10 g.m⁻³. I suggest to the authors to clearly indicate that in their discussions on the use of the SKIRON and BSC DREAM8b v2.0 models.

Technical corrections

- Page 4, line 27: please change Al for Al in brackets for the non-sea salt Na calculation.
- Page 22: Fig.6 not Fog.6 - Page 25, Fig.11: please, use a log-scaling for the y axis (Mass Fractions), as in Fig.12, for example.

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