

Chemical Speciation and Source Apportionment of PM_{2.5} in Greek Urban and Suburban Areas: Insights from the PANACEA Initiative” (Papagiannis et al.; ar-2026-35)

Overall assessment

This is a well written, comprehensive and policy relevant contribution to the understanding of fine particulate matter in Greece. Its principal strengths are the harmonised chemical characterisation of an extensive multi-site dataset (545 samples across six stations in five cities), the inclusion of cities less frequently investigated, and the detailed receptor modelling analysis supported by meteorological and transport information. The marked spatial contrasts and the identification of residential biomass burning as a major wintertime source provide valuable evidence for air quality management. The manuscript is generally clearly structured and the discussion is informative. I support publication after revision, primarily to clarify the representativeness of the campaigns and the uncertainties attached to some source specific interpretations.

MAJOR COMMENTS

Temporal representativeness and mineral dust (Sections 2.1, 3.3; pp. 4–5, 25–26). The sampling covers selected summer and winter intervals rather than a full annual cycle. This is a relevant limitation for interpreting source contributions as representative of each city, particularly mineral dust: intrusions of Saharan/North African dust can occur in different seasons, and their frequency and intensity vary considerably between years. The authors already document a major event on 20–22 December 2019 using HYSPLIT (Fig. 7), which illustrates the potential sensitivity of campaign-mean dust estimates to episodic transport. Please discuss this explicitly, distinguish campaign-specific from annual source contributions throughout the abstract and conclusions, and, if feasible, quantify the influence of identified dust event days on mean mineral dust contributions. Local resuspension and transported desert dust should not be conflated when their separate quantification is unsupported.

Different sampling locations in Patras (Section 2.1 and Section 3.1; pp. 5, 12). The summer campaign at PAT was conducted in a relatively rural setting (ICE-HT), whereas the winter campaign took place at an urban background site (TEI). The authors acknowledge the relocation when discussing PM_{2.5} concentrations, but the implications extend to chemical composition and PMF source contributions. Please make this confounding effect prominent wherever apparent seasonal differences at PAT are interpreted; the differences cannot reliably be attributed to season alone. The table/site nomenclature should also convey that PAT combines two environments.

PMF solution robustness, particularly Volos (Section 2.3, Table 3; pp. 8–9, 20–21). The use of 100 runs, bootstrap and DISP diagnostics, and a reported mapping rate above 80% is welcome. Nevertheless, the VOL-URB solution comprises 44 samples (only 14 in winter), several factors and imposed constraints. This combination raises concerns about stability, source resolution and dependence on modelling assumptions. Please provide site specific diagnostic summaries (including the number of factors explored, Q/Q_{expected}, residuals, bootstrap mapping and DISP outcomes), and show or summarise how constrained solutions compare with unconstrained alternatives, particularly for VOL-URB. Clarify how the PM_{2.5} “total variable” was treated and whether the uncertainty of the reported source contributions is conveyed beyond factor stability.

Biomass burning attribution and chemical specificity (Sections 3.1 and 3.3; pp. 13, 22–24). The interpretation of biomass burning based on combinations of OC, EC and K is plausible and consistent with the strong winter signals, but these species are not exclusive tracers: K has other sources and carbonaceous compounds occur in traffic and secondary aerosol. Levoglucosan, an important marker for biomass combustion, was not analysed. This does not invalidate the factor assignment, but it limits its specificity and the precision of the quantitative estimate. Please acknowledge the absence of this marker and indicate which

independent evidence (seasonality, source profiles or external observations) most strongly supports the assignments. Avoid presenting the source attribution as unequivocal.

Mass closure, particle-bound water and OM/OC (Section 3.2; pp. 15–17). The unexplained fraction of $PM_{2.5}$ is substantial at some sites (reported as 7.0–22.3%). The authors correctly mention water associated with the samples among possible explanations; this deserves a more quantitative discussion. Water associated with inorganic salts can constitute an appreciable fraction depending on relative humidity and aerosol composition. If sufficient information is available, an estimate or sensitivity analysis using an equilibrium model (e.g. ISORROPIA) or an appropriate empirical hygroscopic parameterisation (e.g. Köhler theory) would be useful, while respecting the gravimetric conditioning RH. Please also discuss the use of one OM/OC multiplier (1.6) across markedly different sites and seasons and evaluate the possible effect of a plausible range on mass closure.

Traffic exhaust versus non-exhaust (Section 3.3; pp. 27–28). The manuscript appropriately reports that exhaust and non-exhaust factors could only be separated at three sites, while traffic remained a combined factor at the other three. Please discuss the intrinsic overlap of tracer profiles and the potential mixing of road-dust resuspension with the mineral dust factor. At sites where separation is possible, the robustness of the split should be substantiated; elsewhere, avoid interpreting the combined factor as if it independently quantifies the two components. Given the focus on $PM_{2.5}$, conclusions about non-exhaust emissions should also acknowledge that a substantial share may occur in the coarse fraction.

Additional points and presentation

Statistical variability (Table 2 and Fig. 2; pp. 11–12). Table 2 presents means only. Please include standard deviations (or supply them in a readily accessible supplementary table) for the chemical species, with sample counts where coverage differs. Add uncertainty bars or another indication of dispersion to Figure 2; means alone obscure the pronounced day-to-day variability, especially at IOA-UB. Although the text reports mean $\pm$ SD for $PM_{2.5}$ and gives significance tests, displaying the variability would improve interpretation.

ATH.DEM-UB carbon measurements and seasonal OC behaviour (Sections 2.2.2 and 3.1; pp. 7, 13). Page 4 reports PTFE sampling at ATH.DEM-UB, whereas the OC/EC results derive from a separate 3-hour semi-continuous Sunset analyser (clearly specified on p. 7). The methods are therefore not inconsistent, but an early cross-reference would prevent confusion; please explain temporal aggregation and pairing with the 24-hour filters. Moreover, the nearly unchanged OC at ATH.DEM-UB across seasons is interesting and merits a site-specific explanation, perhaps considering exposure to regional aged aerosol, local emission patterns and meteorological mixing; distinguish any hypothesis from demonstrated mechanisms.

Limits of regulatory interpretation (Abstract, Section 3.1, Conclusions; pp. 1, 10, 29). The text appropriately states that partial season means cannot test annual regulatory compliance. Please ensure that the abstract and conclusions do not overstate “high risk of non-compliance” from campaign averages alone. Figure 2 shows annual limit reference lines against seasonal means; its caption and visual framing should explicitly state that these lines are contextual benchmarks, not compliance assessments. Check consistency of the applicable EU limit values and their implementation dates.

Consistency of source contributions and terminology. Please check that the biomass burning contribution ranges reported in the abstract, main results and conclusions refer to the same averaging basis and populations (the abstract states 16.2–50.9%, while the conclusions state 27.5–50.9%). Likewise, clarify whether the grouped “secondary sulphates & organics” factor can be compared directly across all sites, given that at Ioannina it is described mainly as secondary sulphate. Distinguish modelled PMF factors from physically unique emissions sources where appropriate.

MINOR editorial changes. In the Introduction (p. 2), the general source statement supported by Viana et al. (2008) could be revised for grammatical clarity and supplemented with more recent references; the reference

need not be deleted if historically relevant. Please standardise $PM_{2.5}$ formatting with a subscript throughout (including headings, figures and captions), improve ion superscripts/subscripts, and proofread small typographical inconsistencies in the methods (e.g. the ion chromatography sentence on p. 7 and “set top Zero” in Table 3).