Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-843

Anonymous Referee #1

Referee comment on "A new method for the quantification of ambient particulate-matter emission fluxes" by Stergios Vratolis et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-843-RC1, 2023

The idea of the paper of obtaining fluxes of emission for dust and sulphate is very interesting. However, the presentation is poor and the support to results and conclusions is weak in my opinion.

I am in the interphase of major revision or rejection.

- Change in tittle and all text 'emission factors' by 'emission fluxes'. 'Emission factor' in 'emission and projections' has a very well-defined meaning, with kg or t/unit of activity. When referring to emission or deposition per area and time, the term 'fluxes' is used.
- References for health studies are ok but old, please update at least with the most recent papers on the Global Burden of disease and 2021 WHO AQ guidelines
- Very repetitive the paragraph below. Try to send the messages only once and add references in all cases:

'The identification and quantification of sources and corresponding source areas of aerosols require significant effort by the Scientific Community. When these this information are is at hand, mitigation

measures can be applied and air quality can be improved. Source apportionment methods can support air quality planning activities, by providing information on the relationship between air pollutant sources and their concentrations. Reliable and quantitative information on the origin of pollution and on pollution sources is required in order to support the design of air quality plans and explain the origin of exceedances. This information regarding the quantification of the sources of air pollution, both in terms of their sectorial and spatial origins, constitutes an essential step of the air quality management process (Wesseling et al., 2019).'

- Better justify that the method used can be applied for secondary PM components, such as sulphate. For dust it is clear but in different seasons the SO2 oxidation velocity might change and sulphate being formed faster or slowly and then the distance to the origin might change artificially for this. At least evaluate what effect it might have.
- Not clear to me how regional from long range sulphate and dust can be distinguished.
- You stated in text that 16 cities are studied and only 14 are indicated in the maps of Figure 1. You explain that only 14 were selected, but why 2 were excluded give reasons in methodology.
- For a number of cities did you applied PMF with less than 50 samples. Is this right? I do

not think so.

- R143-147 you select and exclude sites without supporting reasons.
- R161 cite Figure 3.
- All this section on sulphate is very confuse, you select some cities, then you do not include because low samples and then use others. At the end the reader does not know what you have done and why you exclude and select ones or the others.
- Why you did not use OMI or TROPOMI for SO2 concentrations in addition to Eclipse maps? These show much better the SO2 hotspots. Furthermore, I do not see properly that Eclipse and your maps show similar high SO2 regions.
- Figure 4 you do not reach a good agreement for Vilnius but you do not mention for the other cities. You state that it might be precipitation the reason for the lack of this agreement but no support is given for this.
- I am not able to identify in the result section on sulphate how and what are the emission fluxes you cited in the abstract.
- Dust I am not able to see in the map that NW Africa is the main source, especially for Tirana. IN many cases is N Africa, but not the well known large sources from NW Africa, Central and S Argelia, Mauritania, Sahara,…. That are not covered by the map patches.
- Summary: 16 or 14 cities?
- Summary you give the quantitative emission fluxes for both dust and sulphate without showing results on it in the prior sections?????