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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variation in Global Chemical Composition of $PM_{2.5}$: Emerging Results from SPARTAN" *by* Graydon Snider et al.

Anonymous Referee #2

Received and published: 16 March 2016

Dear Editor,

This MS presents the results from the chemical characterisation of PM2.5 from 12 stations across the globe in the framework of the SPARTAN project. Nephelometry data are also presented, as well as an assessment of the influence of hygroscopicity. The study is well designed and interesting mainly due to its global scope, although precisely because of this global scope the results seem at times too general and it is difficult to understand the ultimate objective of the authors' work. Another issue is that limitations of the work are barely presented or discussed, especially obvious ones such as the absolute lack of data representation in Europe. This should be discussed. Also, the temporal representativeness of the data should be discussed given that the sampling period varied largely between sites (2 to 22 months) and clear outliers were present (dust storms, forest fires, etc.) Overall I would recommend publication after the

changes below are addressed, and especially after the overall aim of the manuscript (otherwise the MS as it is is merely descriptive) and the issue of temporal representativeness are discussed.

Specific comments:

- line 22: "2-22 month periods", what was the average period for all sites? Whether 2 or 22 months of data were available for a given station is highly relevant with regard to its representativeness. Please clarify in the abstract and in the text.

- line 34: the term "residue" is rather confusing, especially if one sees later on in the text that it refers mainly to organic matter. I would suggest to find a different term.

- line 51, treatment of outliers: how were outliers processed? This line refers to a major sulphate event, and in the final section of the MS for several stations the authors describe the impact of dust events, major forest fires... How were these outliers dealt with? Depending on the duration of the sampling (see the first comment above), these outliers may have been not representative and strongly impact the mean aerosol concentration. A section should be added to discuss how frequent these outliers were, and how they were treated.

- lines 116-121: please highlight as a limitation that no data are available for Europe. This is a major spatial gap, despite data being openly available (e.g., chemical speciation data and nephelometry from the EMEP stations, stored in the EBAS database).

- line 135, what was the diameter of the filters, 47 mm?

- line 155: were all the filters from all sites shipped to Dalhousie Univ. for analysis? Please describe how filters were stored + transported to guarantee sample conservation, as this could be a major issue. How many filters/month were sampled, and how many in total per site? Please add a Table (even if in Supplementary material).

- line 298: please add a reference for the 0.18 coefficient applied to Na+

- line 320: please rename the fraction "residue matter" and state clearly from the beginning that it refers mainly to organics

- section 4.10: this entire section is very descriptive, but hardly any interpretation of the results is provided.

- line 357: this result is surprising for Beijing: even despite the influence of dust storms, a major urban area like Beijing should have a high Zn/Al ratio (high anthrop. influence). Whys is this not the case? Could it be due to the potential lack of representativeness of the samples, linked to how outliers were dealt with? Please discuss and clarify in the text.

- line 376, 60% in Kanpur: if RM is so high in Kanpur it implies that CM will be very low, which seems surprising for an Indian city. Could this be because sampling took place during the monsoon season, maybe? This refers again to the temporal representativeness of the sampling. Some of the chemical composition results seem unexpected, please include a section discussing the potential sources of uncertainty, e.g. sampling period, filter transport, technical issues during sampling... What could have gone wrong, with such a disperse network of stations?

- line 394, strong correlation between ASo4 and ANO3: this is unexpected due to the thermal instability of ANO3 in summer (when formation of ASO4 is highest). On an annual basis both components might correlate, but not on a monthly basis.Please clarify.

- section 4.11: this section is very unclear, what studies are the authors comparing their SPARTAN results with? Please specify. What are "study A" and "Study B" in the Figure?

- line 417: "expectation that RM is organic", this is not an expectation, it is a definition of the SPARTAN methodology (given that all other components are already accounted for)

- line 410: "10-30%", this is an overall limitation of the work: because such a broad range of station types, geographical regions and locations is used, the results also become rather broad and non-specific. E.g., stating that ASO4 contributes with 10-30% to PM2.5 mass is not a very specific result, it could represent almost anywhere in the world. Therefore, please state this limitation in the intro or results section, and extract the use of this kind of very global data, e.g. probably (in my opinion) for modelling studies, etc. What is the ultimate purpose of this work? Otherwise it becomes simply a descriptive manuscript.

- section 4.12.1: same issue as above, please discuss the relative contribution of natural and anthropogenic dust in Beijing. The large number of samples available (100) should allow for this kind of interpretation.

- line 445: due to volatilzation but also to the fact that the authors are comparing their results with those from different periods in time, in the other studies.

- line 520, "organic matter burning": this is another example of a potential outlier. In addition, it would be useful if the authors could add somewhere in the text a brief assessment of major common emission sources, e.g., biomass combustion, agriculture, natural dust... This would help to integrate the results from the different sites rather than simply state ranges of chemical components (e.g., ASO4 = 10-30%) which don't provide much specific information.

- line 572: festival in Israel, another probable outlier

- line 691: black carbon should be equivalent black carbon

- line 697: "3-year span", please clarify that this is not a continuous monitoring period in all sites, but instead a sequence of consecutive 4-month (approx) periods in different stations. This is a very big difference.

- Conclusions: the conclusions section is again very descriptive, it is rather a summary. Actual conclusions and applications for their data should be extracted by the authors.

As proposed above, a reference to major common emission sources could be added (this is slightly hinted at in paragraph 7369-744).

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