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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A comparison of $PM_{2.5}$-bound polycyclic aromatic hydrocarbons in summer Beijing (China) and Delhi (India)" *by* Atallah Elzein et al.

Anonymous Referee #1

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Review of "A comparison of PM2.5-bound polycyclic aromatic 1 hydrocarbons in summer Beijing (China) and Delhi (India)" by Elzein et al.

The authors presented measurement results of 17 polycyclic aromatic hydrocarbons (PAHs) in Beijing, China, and Delhi, India in summer. The sampling was conducted with higher time resolutions ($\sim$ 3 hours for daytime samples and $\sim$15 hours for nighttime samples) as compared to traditional 24-h samples. The PAHs were quantified with GC-Q-ToF-MS. Results showed that PAH concentrations were higher in Delhi than those in Beijing, and the summer PAH concentrations were lower than those in winter in Beijing. From the measured PAH profiles, sources of PM-bound PAHs in these two mega-cities

in developing countries were inferred. In addition, health risks were calculated from the measured PAH concentrations. The study is well designed and the analysis is rigorous. The manuscript is fairly well written. I recommend Minor Revision before publication, with a few comments as follows.

Major

1. It was stated in the abstract (and in the conclusion) that "in Delhi 25% of the emissions were attributed to long-range atmospheric transport". The only evidence the authors used to support this is on L425-427/P9, showing that 25% of data in Delhi had a Bap/(BaP + BeP) ratio of less than 0.5. This evidence is a little bit thin to support such a statement. I suggest the authors to either elaborate this with more evidence, or tone down such an unsupported statement.

2. The issue of oxidation during sampling to the interpretation of results. First, in the paragraph of L192/P5, the authors noted that this effect could be an additional source of uncertainty (10 – 30%) to conventional analytical uncertainties (25 – 30%). The question is, what is the overall uncertainty if both of these two errors are taken into account? Second, after acknowledging this source of potential negative artifact, the authors used it in Section 3.3 to infer particle aging and then to regional transport of PM. Such inference may be conflicting without quantitative assessment on how such "on-filter" oxidation affect the indicator, i.e., the BaP/(BaP + BeP) ratio. Please clarify.

3. Section 3.5. It is not clear why the authors preferred to use only BaP for the cancer risk calculation. Both Table 2 and L524-527/P12 indicated that other PAHs may contribute another half of the risk. Would the reported LECR per million people values be under-estimated if other PAHs are not taken into account?

Minor

1. L146/P4: suggest to change "17-PAHs" to "17 PAHs", and change in a number places (e.g., L223/P5) "24 h mean concentration" to "24-h mean concentration".

2. L156/P4: please change “PAHs concentrations” to “PAH concentrations”.

3. L187/P4: why higher error could be attributed to samples analysed previously in wintertime? Memory effect? If so, why were the Delhi samples not affected? Were the Delhi samples analysed after Beijing summer samples?

4. L199/P5: Tsapakis and Stephanou 2003: please use proper citation.

5. L207/P5: please add “that” after “than”.

6. L414&L425/P9: please use BaP/(BaP + BeP) consistently.

7. L418/P9: please change “[ratio = 0.5]” to “(ratio = 0.5)”.

8. L431-434/P10: this seems like two sentences. Please revise.

9. Figure 3: in addition to the non-preferable “17-PAHs” on the graph and in the caption, I do not see the usefulness of putting “17-PAHs” on the graph. Please remove them on the graph and change to “17 PAHs” in the caption.

10. Figure 5: please change the title of the y axis to “Bap/(BaP + BeP)”, as well as that in the caption.

11. Table 1: please change “PAHs concentrations” to “PAH concentrations”.

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