Atmos. Chem. Phys. Discuss., 15, C6814–C6817, 2015
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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Fine particulate matter associated with monsoonal effect and the responses of biomass fire hotspots in the tropical environment" *by* M. F. Khan et al.

**Anonymous Referee #2**

Received and published: 13 September 2015

The manuscript of Khan et al. focuses on the health implications of fine particulate matter (PM2.5) and its main constituents in the western part of Malaysia. The study period covers two monsoonal regimes, one coming from the south-west and one from the north-east and levels of PM, heavy metals and main ions are determined for both regimes. Overall it has been found that 48% of the samples were 4 times or more higher than guideline values, with the north-east monsoon regime exhibiting somewhat higher values. Based on the enrichment factor of metals it occurred that the majority of heavy metals were attributed to anthropogenic sources, while arsenic was identified as a potentially significant health concern as its concentrations were nearly equal

to the WHO and US EPA guideline values, posing an exposure risk for inhabitants in the specific location. Using a mass closure model a mineral component was found to comprise 35% of the PM2.5 concentration, followed by 11% by secondary inorganic aerosol, while 45% was unidentified, possibly a large part of it being the organic aerosol fraction. Using positive matrix factorization (PMF) analysis as a source apportionment tool, motor vehicle and biomass burning emissions were the dominant sources, followed by marine and sulfate aerosol, coal burning, nitrate aerosol and mineral dust. Finally, based on the health risk assessment and the PMF-identified sources, the non-carcinogenic risk posed by exposure of PM was at a safer level than the respective one in the South and East Asian region while the cancer risk posed by the exposure to toxic metals was 3 to 4 in 1 million people in the specific location. Motor vehicle emissions and trans-boundary pollution were the major identified reasons for change in the chemical composition of PM in tropical Peninsular Malaysia.

The manuscript is well written and interesting, with an added value of the presented results being from an area of the globe that is not very well documented. It is a pity that analysis of the organic fraction is not available, as it is expected that a large portion of the unidentified mass from the mass closure model will be the organic component. There are several details missing and more thorough discussion should be made in specific sections. Other than that the paper can be recommended for publication after addressing the issues listed below.

1) One of the features of the manuscript that the authors draw our attention to is the comparison between the PM2.5 from the PMF and from the high volume samplers (HVS) which was found to have a slope of 0.91 and a very good correlation. It appears somewhat strange that even though 45% of the PM2.5 mass remains unidentified, the comparison of the mass concentrations from the PMF and the HVS results in a slope of 0.91. Please comment. Also, do the authors believe that the remaining 45% of the unidentified mass, with the organics possibly being a large part of this mass, will be part of the already identified components? Please clarify in the text that the PMF



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analysis results are based on the obtained filter analyses results.

2) Authors mention that the K+ ion has been widely used in the literature as an excellent tracer representing wood burning, and authors examined the correlation between K+ concentrations and MODIS fire counts. To my opinion, and correlation coefficient of 0.36 does not suggest that K+ can be used as a biomass tracer. I would propose to differentiate between nss-K+ and total K+ and check if the correlation is stronger. ss-K+ ca be calculated using Na as a reference and the K/Na ratio in bulk seawater.

3) Based on the presented biomass fire hotspots, it is clear that from December 2013 to March 2014, the hotspots are a lot more compared to the respective ones from June to September 2013. Maybe authors should consider adding a comment of why this is the case. Is December-March the dry season? Is this period of intense biomass fire hotspots present every year or was the presented year uncharacteristic?

4) In the enrichment factor section (2.5) in the whole paragraph EFs of two, five or even 10 are cited from the bibliography as thresholds for anthropogenic sources. Nevertheless authors choose a cut-off of EF=1. Please comment.

Technical corrections:

P22217, L15-18: Not clear, especially the part ". . .with respect to inhalation follows the order. . .". Also in PMF 5.0 delete the ">".

P22219, L17: . . ..a period from July to September

P22220, L27: . . .ionic composition was determined

P22221, L18: Replace "fresh" for the calibration curves. Maybe use "updated" instead.

P22223, L14: . . .metals in PM2.5 originated from. . . (delete "had")

P22227, L18: . . .classified by the International Agency. . .

P22230, L28: Boreddy et al. (2014) also notice a chlorine depletion due to atmospheric



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processing in the western north Pacific.

P22233, L1: Based on Figure 6a the slope is 0.91 and R2=0.88 and not the other way round.

P22235, L8: Zhang et al. (2015) demonstrate that during the dry season there is an important biomass burning activity in the Pearl River Delta (China), which can result in trans-border transport and a regional scale character of biomass burning. Therefore under the north-east monsoonal regime it is possible that outflow from that area can maybe influence the specific area.

P22235, L22: An r=0.36 does not suggest that K+ can be used as a biomass tracer.

P22238, L17: ....follows the following order: As > Ni ....Also "follows" and "following" is a repetition, maybe replace one of the two

L18: ...PM2.5 concentration and As > Pb... (delete ";")

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