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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Chemical Characterisation of Water-soluble Ions in Atmospheric Particulate Matter on the East Coast of Peninsular Malaysia" *by* Naomi J. Farren et al.

Anonymous Referee #2

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The manuscript "Chemical Characterisation of Water-soluble Ions in Atmospheric Particulate Matter on the East Coast of Peninsular Malaysia" by Farren et al. investigated the particulate matters on the east coast of Peninsular Malaysia. Chemical components of particles (mainly soluble ions) were measured. Air mass trajectories were applied to indicate the potential source regions of various aerosol components. A thermodynamic model is used to estimate the aerosol acidity. Generally, this study revealed the characteristics of atmospheric chemistry over the East Coast of Peninsular Malaysia, which has been rarely reported. This manuscript served to fill in the gap of the Southeast Asia region which has been poorly characterized of its emissions, air quality, etc.

However, the chemical characteristics of aerosol over this region is not well studied. The design of the measurement is inadequate based on a Jan. – Feb. sampling of about thirty samples. No sampling during the biomass burning season is conducted. The presentation of the data analysis (almost all the figures) is not good. Substantial revisions are suggested before this manuscript can be further reviewed.

The major comments are list below: 1. Section 2: The method section should be reorganized. The sampling part should be described in the beginning of the section. Then the analytical procedures are presented. This study used quartz fiber filters for particle collection and the subsequent ion analysis. However, it is known that the quartz fiber filters have high background values of some cations such as Ca2+, Mg2+, etc. Did the authors perform ion analysis of the blank filters? What are the values of the ions of the blank filters?

Section 3.1 should be moved to the methodology section as it is related to the uncertainties of the chemical analysis but not the analysis results.

Line 395 – 405: The description of ISOROPPIA-II should be moved to the Method Section.

2. Line 222 – 224: It is not appropriate to compare the results with the previous one as the study period is quite different. Furthermore, why don't use the concentrations of PM2.5 based on your data? 3. Line 269 – 270: Is there any volcano activity during the study period. If not, this citation is not necessary. 4. Line 287 – 288: The pollution rose plot (Fig. 6) cannot show the SO2 concentrations under calm conditions as the rose plot is based on conditions with wind speed higher than zero. Thus, the writings "The majority of higher SO2 events were observed in calm conditions when the air arriving at the site had passed over land to the south west of Bachok." is not based on sound analysis. 5. Line 310 – 320: It is concluded that clusters 4 and 10 are associated with high SO42- concentrations of 20.4 and 18.1 μg m-3, respectively. It is understandable that cluster 4 passed over industrialized areas such as southern

China and Southeast Asia, thus bringing considerable amounts of sulfate. However, it is explained that the high sulfate in cluster 10 is attributed to Manila First, it should be noted that the total emissions and emission intensity of Manila should be much lower than mainland China. Secondly, Cluster 10 travelled long distances over the ocean, which is supposed to have a clean effect on the aerosol concentrations.

6. What is the definition of chlorine-containing very short-lived substances (Cl-VSLSs)? What species are included as Cl-VSLSs? It seems that the authors regarded Cl-VSLSs as a tracer for anthropogenic emissions and use it for further analysis of SO42- during the pollution and less polluted periods. This is problematic as SO42- and Cl-VSLSs should have different origins and behavior during the long-range transport, e.g. dry/wet deposition, decomposition rate.

7. Line 430 – 437: These paragraphs are basically not related to this study.

8. Line 474 – 475: As indicated by Fig. 4, the levels of K+ were less than 1ug/m3 throughout the whole study period, suggesting no significant biomass burning events. Thus, it is wrongly concluded that "biomass burning is a secondary source of oxalate".

9. Section 3.3.5: This section discussed about the Cl-/Na+ ratio and found the ratio was lower than its value of the seawater. It is concluded that the anthropogenic pollution via the long-range transport (Fig. 10) exerted the impact on the depletion of chloride. This is questionable as the study period is Jan. – Feb., which is the winter heating season in China. Based on Fig. 10, the air masses passed over vast areas of northern China, indicating aerosol rich in chloride from coal burning should be derived. If the monitoring site is influenced by emissions from China as discussed by this study, the Cl-/Na+ ratio should be much higher than 1.0.

10. Line 522 – 523: It is hard to say that sulfate suppressed the formation of nitrate. The possible cause should be the deficiency of NH3, leading to the incomplete neutralization of sulfate and nitrate.

Minor Comments:

Page 6, Line 191: Taiwan is not a country.

Line 204: Ashfold et al. ; Line 222: Dominick et al.; Line 322: Oram et al.; Line 445: Freitas et al.; Line 456: Carlton et al.; Line 466: Huang et al.; The format is incorrect. Line 335: what does "a NAME trajectory" mean?

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