

## ***Interactive comment on “An optimized tracer-based approach for estimating organic carbon emissions from biomass burning in Ulaanbaatar, Mongolia” by Jayant Nirmalkar et al.***

### **Anonymous Referee #1**

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An optimized tracer-based approach for estimating organic carbon emissions from biomass burning in Ulaanbaatar, Mongolia by Nirmalkar et al This paper by Nirmalkar et al analysed the chemical composition of daily PM<sub>2.5</sub> filter samples collected in Ulaanbaatar during winter and spring, with the aim of determining the contribution of biomass burning to the PM<sub>2.5</sub> load. The authors then applied multivariate correlation analysis (PCA) to determine the main sources based on the chemical composition and used diagnostic ratios to apportion the contribution from biomass burning. The authors concluded that biomass burning was a significant source, accounting for 68 and 63% of the organic carbon in winter and spring, respectively and that the very high contributions reflected the practice of wood burning for heating in the city. Ulaanbaatar has a well-

C1

known air pollution problem, and this is a nice dataset for investigating the sources during winter. While the dataset appears sound, in my opinion the data interpretation/analysis a bit light. There are much more the authors could do with the dataset to strengthen and support their conclusions. Furthermore, there have been numerous studies already investigating air pollution in Ulaanbaatar, yet the authors curiously do not mention how their findings relate to this body of work, choosing instead to focus on similar studies in other Asian cities. The paper would benefit from more detailed discussion, with some suggestions given below.

Using diagnostic ratios to estimate the source contribution is obviously dependent on the chosen ratio and the authors present an approach for determining the optimal ratio for OC/Levoglucosan to estimate the OC from biomass burning. As the authors mention, the OC/Levoglucosan ratio from biomass burning is highly variable and dependent on many variables such as fuel and burn conditions. I am not entirely convinced by proposed method for optimising the OC/Levoglucosan ratio source apportionment and would have liked to have seen more analysis justifying the proposed ‘optimal’ ratio. For example, some discussion on how did the optimal OC/Levoglucosan from winter and summer compare to the literature values? Does the optimal OC/Levoglucosan ratio make sense in terms what would be expected based on the main fuel used in Ulaanbaatar? What about if the source of biomass burning changed over time during the sampling period, and therefore presumably the ambient OC/Levoglucosan? One potential pitfall in this approach not discussed would be if some of the non-BB sources of OC had similar temporal trends to biomass burning emissions, which would mean that they would also be high when the levoglucosan was high, thus affecting the correlation analysis. For example, coal burning was noted by the authors to be a source of OC, yet I could imagine that during cold periods power station emissions would be also be high at the same time as wood burning due to the heating load. The uncertainties associated with this approach to determining the optimal ratio should be discussed in detail.

C2

#### Minor comments

Abstract: The authors could be more explicit that the optimal OC/Levoglucosan ratio determined is specific to Ulaanbaatar, and that its method for determining it is applicable for other studies.

Page 6, line 98: I presume that you mean it is difficult to determine the most suitable OC/levoglucosan ratio of BB emissions for ambient measurements?

Page 7, line 111: do these thermal power plants burn biomass? If so, emissions from these plants could have affected the results.

Section 3.1: This could perhaps be broken down into a few subsections to help the reader find relevant sections. For example, the PCA analysis could be one subsection.

Page 8, line 153: Are these the average contributions of OC to the total chemical species? It would also be good to give an indication of the variability, perhaps by showing the standard deviation.

Page 9, line 160-3: The statement that during spring the OC increased with temperature due to SVOC volatilization appears to contradict the earlier statement that high concentrations in the winter due to increased condensation of SVOC at low temperature? Why would SVOC volatilization account for the relationship of OC with temperature? Could it maybe be more related to increased biogenic emissions?

Page 9, line 179-81: I am not sure I follow the explanation for the relationship between temperature and EC. What is the source/mechanism that would explain the relationship between temperature and resuspension of soil?

Page 10, line 183: A time series plot of these tracers with temperature would help the arguments in this paragraph

Page 10, line 201-203: As power stations are large point sources, the authors could do some wind sector analysis (e.g. polar plots, concentrations as function of wind speed

C3

and direction) to test this hypothesis. This could also help to see if any of the OC and EC was also from power stations. In addition, Ca<sup>2+</sup> has also been associated with coal station emissions (see Pei et al 2016, <https://doi.org/10.1016/j.apr.2016.01.005>) and may explain the association of EC and Ca from earlier. The authors need to consider the emissions from power stations more closely in order to be confident in the OC:BB apportionment later in the paper.

Page 11, line 212: Since there was a large regional source of BB on these days, where they removed/accounted for in subsequent optimization of OC/Levoglucosan? Local and regional sources are known to have different ratios, and therefore will affect the analysis.

Page 11, line 214: The details of how the PCA analysis was performed need to be included, perhaps in the method section

Page 11, line 223: Were there any other reasons for choosing vehicles as the source of PC4 as there were other sources of EC as well (e.g. biomass burning). Furthermore, I am surprised that if biomass burning was such a strong source that EC did not come out in the same PC as the BB tracers. Perhaps the authors could comment on this. I am also curious as to why there was not a vehicle source found in spring, I would have thought that vehicle source would be consistent across both seasons. Why would there be a combined SIA and vehicles source in spring?

Page 11, line 229: Do the authors have any ideas why K<sup>+</sup> was associated with biomass burning in the winter but not in the spring? Was there a source change?

Page 12, line 243: It would be good to show the intercept as percentage of the total OC.

Page 13, line 238: You state here that the correlation between OC and K<sup>+</sup> indicates that biomass burning was a major source but in the previous paragraph you state that K<sup>+</sup> is coming from soil re-suspension in spring? Please clarify.

C4

Page 12, line 267: If the excess of K<sup>+</sup> during winter was due to biomass burning for cooking, do you see the same in the relationship or similar value for the intercept in the spring? I am assuming that cooking is also happening in spring and not just winter?

Page 14, line 284: it would be good here to give the actual ratios for these different sources from the literature to show how much overlap there is

Page 15, line 308: Is the result that the levoglucosan/mannosan ratio is consistent with softwood expected based on people activity in Ulaanbaatar? That is do people mostly burn softwood at home for heating? Earlier you have stated that coal is mainly burnt for cooking, so it appears that it may not.

Page 17, line 355: What is uncertainty associated the derived optimal OC/levoglucosan for winter and spring?

Page 17, line 357: How do the optimized ratio of 27.6 and 18 compare to the literature for sources. Earlier you stated that levoglucosan/mannosan ratio was consistent with softwood combustion, so are these OC/levoglucosan ratios also consistent for softwood combustion?

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