Response to referee #1

We would like to thank the referee for the interest in our work and the helpful comments and suggestions to improve our manuscript. We have carefully considered all comments and the replies are listed below.

General comments

This work reports measurement data of a few biomass burning organic tracers in TSP samples collected from Lumbini, Nepal over a year. With the data, the authors investigated the influence of biomass burning from both local emissions and regional transport on the atmospheric aerosol under different meteorological conditions (pre-monsoon, wet monsoon season, post-monsoon and dry winter season). Contributions of biomass burning to OC mass were also estimated using levoglucosan data. By selecting three kinds of representative biomass burning tracers (anhydrosugars, lignin pyrolysis products and dehydroabietic acid), the authors also qualitatively evaluated the relative importance of different biomass fuel types in this region. Although the results are more or less expected, the data set is a useful addition to the database of ambient PM chemical characteristics in SE Asia, a region where such data is restively scarce. I list below specific comments for authors to consider in their revision.

Specific comments:

1. Were anions analyzed? (Cations were analyzed, as indicated in the experimental part). If anion data is available, please include them in Table 1 and also comment on sulfate concentration abundance and their seasonal variation characteristics, as sulfate might provide insights into extent of regional influence. If no anion data is available, the authors could take a look at NH_4^+ data, which was expected to be analyzed by IC along with other cations (Ca^{2+}, Mg^{2+}, K^+).

Response: Yes, the major ions (SO_4^{2-}, NO_3^-, Cl^-, NH_4^+, Ca^{2+}, Mg^{2+}, K^+, and Na^+) were detected in our study. However, considering the theme of this work focuses on the organic aerosols, especially the influence of biomass burning, the data of major ions were not fully presented and discussed. They will be reported in other manuscript by one of the co-authors. Therefore, we only discuss Ca^{2+}, Mg^{2+} and K^+ here.

The annual average concentration of SO_4^{2-} in Lumbini aerosols was 6.85±5.65 μg m^{-3}, accounting for 21.0%±6.93% of the total major ions (SO_4^{2-}, NO_3^-, Cl^-, NH_4^+, Ca^{2+}, Mg^{2+}, K^+, and Na^+). The concentration was similar with the studies in Agra (5.9 μg m^{-3} in TSP) and Durg (8.88 μg m^{-3} in PM10), India (Satsangi et al., 2013; Deshmukh et al., 2012) while lower than that in Beijing, China (13.6 μg m^{-3} in PM2.5) (Zhang et al., 2013) and Delhi, India (16.7 μg m^{-3} in PM10) (Chelani et al., 2010). For

the seasonality, SO_4^{2-} showed equivalent higher concentrations during winter (11.1±11.6 μg m^{-3}) and pre-monsoon (10.9±3.33 μg m^{-3}) but lower in post-monsoon (6.39±3.90 μg m^{-3}). The minimum concentration occurred in monsoon (4.90±3.01 μg m^{-3}). It was inconsistent with the seasonal variations of OC and EC (post-monsoon>pre-monsoon>winter>monsoon). However, it is difficult to figure out whether SO_4^{2-} comes from regional transport or local emission, based on our current dataset and discussion. So, such information was not included in our manuscript.

2. In the paragraph starting at Line 247, OC/EC ratio is discussed. The authors need to be more cautious in comparing the OC/EC ratio from their TSP samples to those in the literature, which are largely data associated with PM2.5 samples. OC on the coarse particles has significant contributions from non-combustion sources (such as vegetative detritus, dust). It has been reported OC/EC ratio in the coarse mode was much larger than those in the accumulation mode (e.g. Yu et at, ACP, 10, 5107–5119, 2010), due to the significant presence of non-combustion OC. It would be misleading without commenting on the influence of non-combustion primary OC on the OC/EC ratio.

Response: Thank you for pointing this out. Now we marked the particle size of the samples (i.e. PM2.5 and TSP) in the text (Line 253 and Line 255). We also added some discussion about the uncertainty of OC/EC ratios in TSP samples. "It should be noted that OC may also originate from non-combustion sources such as vegetative detritus and fungal spores in the coarse mode, leading to a high OC/EC ratio in the TSP samples (Yu et al., 2010). Therefore, those bio-aerosols and dust may interfere the OC/EC ratio somewhat and deserve a further study." Please see the details in Line 261 to 264.

3. In the paragraph starting at Line 324, the authors discussed the variation range of Lev/Man ratio and attributed the wide range observed (0.42-22.0) to photochemical degradation of levoglucosan. Such a reason is unlikely, as mannosan also degrades and its degradation rate is likely similar to that of levoglucosan, considering their similar chemical structures.

Response: Thank you for your comments. Yes, there is no solid evidence to support the speculation that levoglucosan and mannosan have different degradation rate, especially when we consider they have similar chemical structures. We agree with you that the current statements may be misleading and confusing. Now the corresponding discussion was changed as "The possible reason is due to the different biomass types burning such as the burning of softwood and hardwood. Nevertheless, the mechanism is unclear yet in Lumbini and more detailed studies are needed in the

future." in Line 329 to 332.

4. Lines 130-131: "The aerosol loading is very high at Lumbini,.." Please provide more quantitative information (e.g., annual average or typical seasonal average concentrations).

Response: Now we provided more quantitative information about the aerosol loading of Lumbini in Line 132 to 134 as "A previous study reported that daily average PM2.5 (ranging from 6.1 to 272 μg m^{-3} with the average of 53.1±35.1 μg m^{-3}) and PM10 (ranging from 10.5 to 604 μg m^{-3} with the average of 129±91.9 μg m^{-3}) levels frequently exceeded the WHO guideline value (25 and 50 μg m^{-3} for daily PM2.5 and PM10, respectively) during pre-monsoon season (Rupakheti et al., 2016)."

5. Line 143: The sampling schedule is on a weekly basis. Was it a regular schedule (i.e., one sample every 7 days or a random day in a week)?

Response: The sampling schedule was on the basis of a random day in a week, without a clear weekend effect. The choice of the sampling day was also determined according to the weather condition, avoiding the direct disturbance of rain events.

6.Line 152: Please provide more information on the field blank samples: how frequently was field blank filters collected?

Response: We add the information about the filed blank samples collecting in Line 153. We collected one field blank sample in each month during the whole sampling campaign.

7. Line 180: 121 m^3: does this correspond to the air volume passing through the entire filter? Why it was not 144 m^3 (0.100 m^3/min ×60×24)?

Response: Thank you for pointing this out. Actually, the sampling duration time was not exactly fixed in the field, mostly due to the power problem. In most cases, it ranged from 18 to 22 hours. Therefore, we used the average sampling duration (around 20 hours) to estimate the method detection limits. Now we added the explanation in the main text for this point.

References

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