

Interactive comment on “Organic molecular tracers in the atmospheric aerosols from Lumbini, Nepal, in the northern Indo-Gangetic Plain: Influence of biomass burning” by Xin Wan et al.

Anonymous Referee #1

Received and published: 31 March 2017

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 re-

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sults 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 relatively 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 NH₄⁺ data, which was expected to be analyzed by IC along with other cations (Ca²⁺, Mg²⁺, K⁺).
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 PM_{2.5} 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 al., 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.
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.
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).

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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)?
6. line 152: Please provide more information on the field blank samples: how frequently was field blank filters collected?
7. Line 180: 121 m3: does this correspond to the air volume passing through the entire filter? Why it was not 144 m3 (0.100 m3/min x 60x24)?

Interactive comment on *Atmos. Chem. Phys. Discuss.*, doi:10.5194/acp-2016-1176, 2017.

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