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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effect of biomass burning over the western North Pacific Rim: wintertime maxima of anhydrosugars in ambient aerosols from Okinawa" *by* C. Zhu and K. Kawamura

Anonymous Referee #2

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The manuscript by Zhu and Kawamura investigates the impacts of biomass burning on TSP (total suspended particulate matter) samples collected at a distinct site (Okinawa Island). The topic is of great scientific concern because biomass burning has important and complex effects on the environment which are still far from being well understood. On the other hand, the results are meaningful for understanding the characteristics of the Asian outflows. However, I think the manuscript is not publishable in its present form and major revision is required.

Major comments

(1) The estimation of non-sea-salt water-soluble potassium (nss-K+). As mentioned by the authors themselves, there was no correlation between levoglucosan and nss-K+ during autumn, and a likely cause was the contribution of soil materials to K+. However, the authors did not make any effort to account for the influences of soil materials. Although the correlation between levoglucosan and nss-K+ was more apparent during the other seasons, contribution of soil materials to K+ could also be considerable, especially in spring and winter (when the air mass to the sampling site was mainly from the Asian continent). Therefore, the role of soil materials can not be ignored. The authors can refer to Pio et al. (Atmos. Environ., 42, 7530-7543, 2008) for this point.

In addition, the referee understands that compared to sea salt, the influences of soil materials are more difficult to account for. Another choice is to compare levoglucosan and K+ directly and use a receptor model (e.g., PMF) to quantitatively estimate the biomass burning contribution to K+. In fact, based on the comparison of levoglucosan and nss-K+, the authors just concluded that biomass burning was not necessarily the major source of K+. There is nothing new about this point (e.g., Zhang et al., Atmos. Chem. Phys., 10, 6839-6853, 2010). Therefore, this paper will be more relevant if the authors can quantitatively estimate how much K+ is associated with biomass burning.

(2) The discussion on the stability of levoglucosan. The authors discussed the possible degradation of levoglucosan based on (i) the gradient of anhydrosugars from Okinawa (the sampling site of this study) to Chichijima (1400 km east of Okinawa), and (ii) the gradient of levoglucosan to OC ratio between Changdao, Okinawa and Chichijima. However, results from these locations were measured in different years. It is ridiculous to use this kind of results to discuss the possible degradation of levoglucosan!

(3) Contribution of biomass burning to OC, EC and TSP. There are numerous studies investigating the levoglucosan to OC ratios in biomass burning source emissions, in addition to the two references mentioned by the authors. It has been well documented

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that the levoglucosan to OC ratios in biomass burning source emissions can vary by a factor of up to 10 among different types of biomass fuels, which is also strongly affected by the combustion conditions. With respect to this study, both the source areas (e.g., China, Mongolia, and Russia) and the source types (e.g., wood combustion vs. burning of crop residuals; open burning vs. domestic burning) of the biomass burning aerosol are highly variable, indicating that it is very difficult to get a representative levoglucosan to OC ratio for the biomass burning source emissions. The authors should try to use a receptor model (e.g., PMF) to estimate the contribution of biomass burning.

(4) Page 25593. The observed correlation between levoglucosan and ammonium can NOT support the conclusion that ammonium is either emitted by biomass burning directly or from plant nitrogen. The authors should refer to some publications about the emission inventory of atmospheric ammonia. The authors should also keep in mind that correlation does not necessarily mean a same source (in fact this point was mentioned by the authors themselves on Page 25594).

Specific comments

(1) Page 25584, the last paragraph. When describing the amount of biomass burnt by fuel types, domestic fuel is missed. (2) Page 25585. In addition to the relative humidity, the temperature at which the quartz filters were equilibrated should also be mentioned. (3) Page 25585. Only two filters were kept as blank in this study. Are the blank concentrations (e.g., OC and levoglucosan) comparable between them? (4) Page 25587. How about the low detection limit of mannosan and galactosan?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25581, 2014.