

Interactive comment on “Comparison of several wood smoke markers and source apportionment methods for wood burning particulate mass” by et al.

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Review of paper "Comparison of several wood smoke markers and source apportionment methods for wood burning particulate mass", by Sandradewi et al.

General comments:

Wood smoke from wood burning stoves can be a major source of air pollution in some locales. This paper examines several ways of apportioning the ambient carbon-containing particulate mass (PM) into either "wood burning"; (PM-wb) or "traffic"; (PM-traffic) categories. An "other" category is also included for cases that include significant amounts of other carbon containing material, such as secondary organic aerosol.

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The authors then explain two ways of discriminating between fossil and wood burning carbon-containing PM based on either aethalometer measurements or carbon-14. These two methods are compared with one another, showing consistency between the two methods, as illustrated in Fig. 1. The authors then compare PM-wb (or OC-wb and EC-wb) to various markers of wood burning (potassium, anhydrosugars, etc.) For all the above comparisons, data from five campaign periods are used: three of these periods took place in rural valley where residential heating by wood stoves was widespread, and the other two periods took place in more urbanized areas.

Comparing the results of the various methods of source apportionment/markers composes a useful and interesting study. Most of the correlations shown, for example, levoglucosan plotted versus OC-wb as shown in Figure 3(a), are fairly strong and convincing. On the other hand, some of the linear correlations exhibit different slopes over the five cases as evinced in Table 2 (PM-wb versus m/z 60, or AMS Potassium). But these inconsistencies are acknowledged and discussed by the authors.

I believe this work is novel, important to the field of air pollution, and interesting, and I am recommending publication of this manuscript, with the caveat given below under specific comments.

I note in passing that I found the effect of either hardwood or softwood combustion on the results (section 4.3) to be very interesting.

Specific comments:

I have one major problem with this paper that must be addressed before publication: the comparison between aethalometer method and the carbon-14 method seems circular. This could be a misunderstanding on my part, but if it isn't, then this issue should be addressed. The aethalometer method is described more fully in Sandradewi et al., 2008, (Aerosol Sci. Tech.). From this description it appears that the aethalometer method is "calibrated"; by adjusting α -wb to achieve the most favorable comparison between the aethalometer and carbon-14 results. I am wondering if the comparisons

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shown in the paper under review (e.g., Fig. 1) are the result of this tuning. If so, would the method be achieve equally good results in other places where tuning was not possible?

Technical corrections:

Page 8102, line 14: "combustions" should read "combustion"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8091, 2008.

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