

***Interactive comment on* “Low molecular weight organic acids in aerosol particles from Rondônia, Brazil, during the biomass-burning, transition and wet periods” by A. H. Falkovich et al.**

Anonymous Referee #1

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This manuscript gives a detailed characterization of biomass burning derived aerosol particles in the source region regarding chemical composition and size distribution. The simultaneous determination of these quantities using different techniques is highly important for ensuring data quality and for gaining additional insight. With that said, the reviewer found that although the results may have reaffirmed some properties of smoke aerosols, most of the findings have been reported before.

The size-dependent chemical composition obtained from the MOUDI samples is in principle highly important and is worthwhile reporting. Clearly, aerosol size distribu-

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tion is expected to change with time, and the key questions one can address with this type of data is source identification and particle evolution. However, the authors only superficially explained the reasons for and implications of the small difference in the size-dependent composition between the dry and the wet season samples. It seems that no major insight was obtained from the size-dependent concentration information. The authors need to explain to what extent the size dependent concentration information has improved our understanding of the smoke aerosols, and whether it is crucial to have this type of measurement included in future studies. May be a simple fine-coarse division will suffice?

Several other important pieces of information, namely, the total water soluble organic carbon, and total mass, were not explicitly reported. I assume that the PM mass was measured gravimetrically, but not sure about the WSOC. They should be included in this manuscript to make this report more coherent and complete. Possible measurement problems were not discussed in detail, including particle bouncing and interferences from gas phase species for both organic and inorganic compounds.

Specific comments follow.

1. page6869, line25: The term "ionic strength" has a specific definition, and is misused in this manuscript.
2. 6876, 29: Since the WSOC concentration was used to gauge the importance of the individual organic ionic species, a description should be given to let the readers know how the WSOC was determined.
3. 6872, 15: The typical sampling time of the each sampling systems should be spelled out here.
4. 6873, 20: The identities of the ion standards should be given; they are more important than the names of the manufacturers.
5. 6875, 10: What is the "complementary information"? Spell it out. Otherwise, it

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sounds like a mystery.

6. 6876, 22: What is the acronym "IFUSP"?

7. 6877, 12: Unless I missed it, how are the PM concentrations determined for each size bin? And in this regard, Figure 6 's y-axis label ΔC should be ΔC_i .

8. 6878, 8: Is there a possibility that the Na and Cl observed were due to contamination as they don't have major known sources other than the sea-salt aerosols?

9. 6879, 5: Since no denuders were used in any of the sampling systems, the authors need to address the possible interference from gas phase species. For example, the high NO_3^- level associated with the large particles might be caused by the absorption of gaseous HNO_3 . In this regard, the readers also need to know if the WSOC measurement might suffer from gas phase interference which could result in an underestimate of the fraction of the ionic organic components.

10. 6880, 24: The statement "Ēindicating that the fine aerosols consist mainly of smoke particles." is not necessarily true. It is more correct to state the other way around, namely, that the smoke particles are all confined in the fine PM fraction. Imaging if the original statement is true, then no particle is present if there is no vegetation burning.

11. 6881, 3: The reviewer doesn't understand why the closeness between C3-C6 DCA and $(\text{NH}_4)_2\text{SO}_4$ suggests that they are derived from vegetation combustion.

12. 6881, 15: The sentence here is merely repeating the observation, not offering scientific explanations. Could it be that the black carbon accounts for the missing mass in the smaller size particles? Does this fraction change as a function of time or age?

13. 6881, 19: C2-C6 should read C3-C6?

14. 6882, 1-19: not much is new in this section. In fact, one needs to eliminate the possibility of the gaseous HNO_3 interference first.

15. 6882, 17: This sentence is entirely confusing. First, what is the loss-free technique

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used to determine the NO_3^- ? And if it offers the true quantitation of NO_3^- (which showed that NO_3^- was greater than SO_4^{2-}), then the results reported in this manuscript showing SO_4^{2-} being greater than NO_3^- must be erroneous.

16. 6883, 5: If levoglucosan was also measured, the authors should have investigated its relationship with K, and the usefulness of both as biomass burning tracers. This should be very valuable.

17. 6883, 10: The authors need to explain why crustal material derived aerosols are more prevalent during the wet season than dry season. The rain during the wet season should lesson the chance of wind blown dust and the dry season should make dust contribution more important especially with the aid of turbulence caused by fire events.

18. 6884, 22: Using words like "sophisticated" doesn't mean much, why not tell the readers the name/principle of the technique.

19. 6886, 10: change "compositional data" to "composition data".

20. 6904, Figure 6: It is not possible to discern the size-dependence of the ionic species as plotted in Fig. 6. In order to reveal the size dependence patterns of each species, they should be plotted individually. The reviewer cannot be convinced by Fig. 6 that, for example, the aromatic carboxylic acid has the same size dependence as that of K.

21. 6886: This conclusion section is basically a very generalized discussion which doesn't even rely upon the data reported in this work. Perhaps the authors don't want to repeat what they have already said in the abstract?

22. 6897: Acetate is misspelled.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6867, 2004.

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