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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of volatile organic compounds during ARCTAS – Part 1: Biomass burning emissions and plume enhancements" *by* R. S. Hornbrook et al.

Anonymous Referee #1

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This paper describes the measurements of enhancement ratios of volatile organic compounds (VOCs) in biomass burning (BB) plumes encountered during the ARCTAS campaign. VOC measurements of three different instruments were combined and inter-compared and a large number of BB plumes originating from fires in California, Canada and Asia were investigated. The encountered plumes had varying ages ranging from a few hours to days, making this a very large and comprehensive data set for boreal forest fires emissions that clearly demonstrate the fire-to-fire variability and the influence of photochemical age. The paper basically summarizes the results of the measured enhancement ratios and also discusses the evolution of VOCs in BB plumes a little bit and is as such a very useful data compilation. It is well written and a pretty

straightforward paper. I have some comments that I would like to see addressed, but to my opinion none of them are really any major issues.

Main comments:

Page 14132 line 20-27: A description of what will be discussed in a future paper does not seem appropriate here. I think at most one sentence advertising the paper is enough.

Page 14138: For the inter-comparison the R coefficients should be given as well, because they tell a lot about the quality of the measurements. A good agreement of the slopes shows only that the calibrations of the different instruments agree. Especially looking at the TOGA-WAS comparison of the oxygenated VOCs a large scatter in the data is obvious, which is not the case for the alkanes. This means that the sampling time artifacts cannot be the cause for this large scatter. Oxygenated VOCs are generally hard to measure and often don't seem to be very stable in canister samples. A discussion of the measurement quality of each compound class using the three different instruments should be added and a recommendation could be given, which instrument is the best for which compound class.

Page 14138: If the measurements of the different instruments agree, the fastest instrument should always be used for the analysis to get the best correlation determining the enhancement ratios.

Page 14139 line 25: The term plume group is somewhat confusing. If I understand this sentence correctly a plume group is the same biomass burning plume that has been intercepted multiple times, which I would not call plume group, but simple a plume.

Page 14140: A short explanation of what the MCE means should be added here. It is given only late in the paper and should be moved here.

Page 14140: The difference between emission and enhancement ratios is often overlooked and the discussion here is important and well done.

ACPD

11, C5940–C5943, 2011

Page 14143 line 10: A short discussion about the possible urban sources of HCN for the Californian fires needs to be added here even though the enhancement ratios are lower compared to the Siberian and Canadian fires.

Page 14143: A discussion about the importance of acetonitrile as a biomass burning marker should be added. Especially since the enhancement ratios do not vary significantly between the source regions, acetonitrile is a good tracer.

Page 14144: The ratio of acetonitrile to HCN can be calculated very easily from the Tables and I don't think that has to be discussed explicitly here. It was explained that the enhancement ratio of acetonitrile with CO is relatively constant and HCN is rather variable, therefore it is pretty obvious that the acetonitrile/HCN ratio is variable in the same way as HCN/CO. So I suggest deleting lines 1-14.

Page 14144 line 18: typo ARCTAS

Page 14145: Relatively small acetone production in all the observed plumes, even in the summer ones, suggests relatively low OH in the plumes. Is this consistent with the low acetaldehyde enhancements in the Asian plumes?

Page 14148 line 7: Apel et al 2011 is not in the reference list.

Page 14148 and chapter 3.5: The chapter about the alkanes should be reorganized. The plume group 28 seems to be not only biomass burning in origin, and should be removed from this analysis. This plume group should certainly not be discussed at the beginning of this chapter. I would suggest doing the calculation of the alkane enhancement ratios without this plume and then discuss the results, after that briefly mention plume group 28. Chapter 3.5 should then be moved into the alkane section. Going back to the discussion of measurement results after the modeling part is not good for the flow of the paper. If correlations of the light alkane enhancement ratios versus propane are discussed, other compound classes should be treated in the same way. Correlations of the aromatics likely show the same behavior as the alkanes, whereas

ACPD

11, C5940–C5943, 2011

the oxygenates might not.

Page 13149 line 15: Is the difference of the propane enhancement ratios between spring and summer consistent with the acetone production?

Page 14151 line 23: I don't understand what you mean with "more polluted conditions". Are you referring to the mixing ratios in the biomass burning plumes or the atmospheric background conditions?

Page 14155 line 25: Why should averaging fire emissions over large areas lead to significant under- or overpredictions in emission inventories. This should only cause large scatter, when comparing individual plumes to model results.

Table 3: Please add the new Akagi et al 2011 paper to the comparison.

Table 4: Warneke et al 2009 is missing from the reference list.

A very useful addition to this paper would be to discuss the relationship of some VOCs with the MCE. I think this would be more interesting than the results presented in Figure 10.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14127, 2011.