**Atmospheric
Chemistry
and Physics
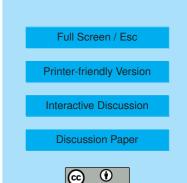Discussions**

Interactive
Comment

Interactive comment on "Characterization of high-resolution aerosol mass spectra of primary organic aerosol emissions from Chinese cooking and biomass burning" *by* L.-Y. He et al.

Anonymous Referee #1

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The manuscript by He et al. is a summary of an interesting set of experiments to characterize AMS markers for different cooking and biomass burning organic aerosols. It is simple and generally well written. There are few aspects though that I see the need for the authors to elaborate on before publishing since they each may affect the results and the interpretations afterward.

1. The effect of dilution on the characteristics of the observed markers (for example the relative contribution of one marker vs. the other) is really not talked about. In Section 2.1, it is mentioned that the smoke is diluted by a factor of 10-100 times. Recent work of Robinson et al. shows that partitioning of semivolatiles can be important for OA for-

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Interactive Discussion

Discussion Paper

mation. Since dilution affects vapor pressure of these semivolatiles, it will be important to note how dilution might have affected the results. Were there experiments with the same primary source which were carried out at different dilution rates? What was the effect on the various markers? 2. What was the total residence time in the system, from the point of burning/cooking to sampling by AMS? Can SOA also be contributing to the observed OA? Were there any gas phase measurements of NOx, PAN, NOy, O3, VOCs etc. that will clue into secondary processes taking place in the set up? It is somehow an incomplete picture if one discusses only AMS observations without relating them to any gas phase measurements. 3. What temperature was the vaporizer in the AMS run at? Was the temperature tested with any chemical sampling? The reason for this concern is that in many AMS's , the thermocouple reading of the temperature is not precise or uniform; This may lead in some cases to have the vaporizer running at a very high or low temperature. In either of these cases, the observed fragmentation pattern may be affected and so generalization out of these observations may not be valid.

Minor comments: It's unfortunate that there are no line numbers or page numbers on the manuscript. That makes commenting point by point difficult. I'm doing my best here to be clear about where I am suggesting these changes for. 1. Page 3, rephrase as "The Aerosol Mass Spectrometer (AMS) manufacture by Aerodyne Inc. (Billerica, USA) can determine chemical composition of submicron aerosol online with high . . ." Same is true in the first sentence of 2.3. 2. Page 3, rephrase as "Mohr et al (2009) reported on . . . and pointed out some of the AMS signatures of . . ." 3. Page 4, add ". . .and allows for a better factor analysis of MS . . ." 4. page 4, consider replacing and with 'which' in the sentence ". . . and then goes through the dilution tunnel.." 5. page 7, last sentence of 1st paragraph "..there appears to be more fragments in the range of m/z. . ." 6. page 7, in the middle paragraph, rephrase as "A recent AMS measurement of primary cooking emission from heating of seed oil. . . However, . . .emissions from meat charbroiling. . .Our results suggest that signature of OA generated from frying is more dominant than OA . . ." 7. page 8, 2nd paragraph: consider "It is seen in Fig 1

that the major MS signatures, such as the most prominent ions and fraction of oxygen-containing ions, of the COA and BBOA match ...., supporting the ....." 8. page 10, "When considering rice straw incorporation as a good ..." 9. Fig 1 caption- Indicate that pie charts show elemental composition of the OA for each panel. Also, clarify that PMF-resolved OA factors are from ambient data. 10. Fig 4- are the values of different fragments normalized to total?! Axis label as is indicates they're not, but if they truly are not fractions, then they very much may depend on total OA signal and as absolute number are meaningless for any comparison.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21237, 2010.