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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Speciated On-line PM_1 from South Asian Combustion Sources: Part I, Fuel-based Emission Factors and Size Distributions" *by* J. Douglas Goetz et al.

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This is a well-written paper describing a careful, comprehensive and challenging set of emissions measurements of significance throughout the developing world. The introduction provides convincing motivation for the NAMaSTE experiment as a whole and specifically for the emission factor measurements presented in the manuscript. As the authors state, this paper provides critical field measurements of under-characterised combustion aerosol emissions common to developing countries. This is an important subject and within remit of ACP, with its in-the-field online PM emission source measurements. The measurement techniques chosen were appropriate and state-

of-the-science. In general, the authors should be commended on the clear way they have presented their complex results in a manner that should prove useful to future estimates of the air quality impacts of S. Asian combustion sources. I have no major criticisms, but have one or two questions that might be clarified in addition to those of the other reviewer.

line 19 & 31 in the abstract & ...: "Particle phase HCl" - the authors should elaborate on what they think the form of HCl is - hydrochloric acid or organic chloride if its source is chlorinated plastics? and "non-refractory chloride [from BB]" is this condensation of gaseous HCl or a direct primary particulate emission? That the dung-fueled cookstoves were also found to emit ammonium and it was stated that this implied neutralisation, itself implies the form of the chloride as free acid. Since HCl is an extremely volatile gas if not neutralised, is it implied that the particles are simply not in equilibrium?

line 141 "...an attempt to sample at an adequate distance from the point of emissions (typically > 1m) and away from the plume centre to collect cooled and diluted emissions". The authors will appreciate the fact that temperature and dilution control the mass of semi-volatile components in the PM emissions. It is understood that the paper does not aim to provide a detailed analysis of component partitioning, but the authors should provide a brief discussion of their choice of downwind distance (and hence dilution ratio and temperature) and how it will effect the measurement and derived emission factor depending on the volatility profile of the emission and why it is judged to be "more atmospherically relevant" (line 143). This is not to state that these challenging measurements aren't atmospherically relevant, nor that they could have been conducted in a better way, but the possible influences on the reported values should be elaborated on, particularly given the implication of the statement on line 175 that "transmission was ... independent of the dilution factor for non-volatile aerosol". In this regard, the sentence between lines 338 to 341 is interesting in that the poor ventilation in the RETS lab was deemed to invalidate emission factor determination. A brief explanation of the

rationale in this context would probably be warranted.

The discussion of the effects of dilution on the presumably high volatility chloride loading should probably also be included.

Line 322 The contribution to f44 from C2H6N+ is interesting. What is the expected reason for such a high contribution of organic nitrogen from this source? Are there possible implications for f44 from other sources?

Table 1 - expand acronym HW

A clear conclusion of the work might be that the variability across individual source types is significant and raises questions about representativeness of categorisation of emissions within conventional categories used in building gridded inventories. Do the authors have recommendations about how their results should be reflected in inventory categorisation in S. Asia?

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