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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Correlation of black carbon aerosol and carbon monoxide concentrations measured in the high-altitude environment of Mt. Huangshan, Eastern China" *by* X. L. Pan et al.

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The authors present very interesting results on the behavior or two major pollutants, CO and "BC", in the atmosphere over Eastern China. It should be noted that one of the species investigated, "black carbon (BC)", is not a chemically defined substance. Instead, it is an operationally defined property, and suffers from a confusion of terminologies (Andreae and Gelencsér, 2006). For this reason, it is essential that the measurement procedures are described as precisely as possible and the terminology is fully explicit. I would like to ask the authors to clarify some issues in this context.

First, they report that measurements of "BC" were made with a MAAP instrument at 670 nm. A recent report has shown that the MAAP, contrary to the manufacturer's specification, actually measures attenuation at 637 nm (Müller et al., 2011).

Second, the authors state that they have compared their MAAP results with measurements of "EC" by a Sunset Labs thermo-optical analyzer and found a difference of 50% between the BCe from the MAAP and the ECa from the Sunset Labs (for my terminology, see Andreae and Gelencsér, 2006). They then state (page 4453, line 13): "Here, we employed a factor of 1.4 in converting the MAAP-measured BC mass concentration to an "EC" category." Yet, in the rest of the paper the term "EC" never appears again, and the reader wonders whether the "BC" in the paper is actually the BC as reported by the MAAP, or the "EC" as recalculated by dividing the MAAP data by 1.4? In the latter case, they should report their results as ECa.

Third, the authors use two types of properties, a mixing ratio (ppb) in the case of CO and a mass concentration (ng m-3) in the case of "BC". While the former is invariant to changes in temperature and pressure, the latter will change with altitude (pressure) and temperature. This is especially important when reporting data collected at altitudes much greater than sea level, as in the present case. Such data must be corrected to standard conditions (273.15 K and 1000 hPa are recommended by IUPAC) and correction must be explicitly stated.

Two other technical issues should be corrected: The authors report an excessive number of significant digits, in some cases up to five digits for values with an uncertainty of 25%! This is not acceptable, and values should be rounded accordingly (see any manual on scientific reporting). Also, only metric units should be used, not inches as in line 20 on p. 4452.

Finally, I note that the authors deplore that there are only few investigations of the $\Delta BC/\Delta CO$ ratio in China. I draw their attention to our paper (Andreae et al., 2008) where we report on measurements made in Guangzhou, using a relatively similar tech-

nique for the determination of ECa, and found comparable results for the ratio of the two tracers.

References

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Andreae, M. O., Schmid, O., Yang, H., Yu, J., Zeng, L., and Zhang, Y., Optical properties and chemical composition of the atmospheric aerosol in urban Guangzhou, China: Atmospheric Environment, 42, 6335-6350, doi:10.1016/j.atmosenv.2008.01.030, 2008.

Müller, T., Henzing, J. S., de Leeuw, G., et al., Characterization and intercomparison of aerosol absorption photometers: result of two intercomparison workshops: Atmos. Meas. Tech., 4, 245-268, 2011.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C180/2011/acpd-11-C180-2011-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 4447, 2011.