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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "A two year's source apportionment study of wood burning and traffic aerosols for urban and rural sites in Switzerland" *by* H. Herich et al.

**Anonymous Referee #2**

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This manuscript follows two existing papers in the application of online optical techniques for source apportionment analysis of biomass burning aerosols. The methodology applied is slightly modified with respect to the previous works, and the results are positive and presented in a clear and straightforward manner. As a result, the authors are able to suggest this methodology as a valid tool for source apportionment of carbonaceous aerosols. The paper is of interest and within the scope of AMT, and merits publication after revision. Page 3, line 11: this does not apply to Southern EU, as biomass burning is not a major source of PM. Please rephrase. Page 5, line 25: please provide some further details on the methodology, in case the readers have no



immediate access to the paper referenced here. What is the methodology based on? page 6, line 18: please specify, was it total K or soluble K? Only soluble K is a tracer of biomass burning, not total K page 6, lines17-22: the methodologies for the analysis of K and levoglucosan should be described. Figure 2:the OC/EC ratio is not included in the legend page 8, line 25: please add "in summer" for the PAY data. Page 8 line 10 to page 9 line 5: an interpretation of the monthly and daily cycles of the absorption exponent would be welcome, in addition to the factual description of the variations currently provided by the authors. The same applies to the description of the OC/EC ratios in Figs 2a and 2b. Page 10, line 8: regarding the selection of the absorption coeff of 0.9 for FF, what uncertainty do the authors associate to this value? The selection of this 0.9 coefficient in this work seems very similar to the selection of the minimum OC/EC ratio representing primary vehicular emissions in the EC-tracer method. Despite the fact that in the EC-tracer method the minimum value is meant to be selected, it is currently under discussion whether that minimum value really represents primary vehicular emissions, and it has even been proposed that a factor of 0.5 should be applied to the minimum OC/EC value to obtain the real marker of primary vehicular traffic emissions. In the present work, what uncertainty do the authors consider? Could a similar debate originate as with the EC-tracer method? What is the range of values that the authors considered before selecting the 0.9 coefficient for FF, and how would different values have affected the final result? A sensitivity analysis would be very welcome here, or at least a discussion on the uncertainty. Page 10, line 17: the assumption made by the authors that sources other than biomass burning and fossil fuels affect the carbonaceous matter mass is rather evident. However, the model seems to have worked in previous papers (Sandradewi et al and Favez et al). How do the authors explain this? Why should the length of the study period have an impact on this, as suggested by the authors? Figure 3: why is the correlation between measured EC and modelled BC so low in ZUE, and especially much lower than at PAY and MAG? Page 12, line 18: I find the total BC concentrations in ZUE rather low (0.99 to 1.34), do these results agree with previous studies and/or other works? Page 12, lines 9-20: the mean contribution of BCwb in wither is very similar for all three sites (29%, 27%, 24%). Was this result expected? Wouldn't WB emissions be much higher in the rural areas with respect to ZUE? Figures 5 and 6: the data are not very visual in the current scatter plots; the authors could maybe consider other ways of representing the data? Or simply separating the FF and WB correlations in 2 different plots, that might already help.

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Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5313, 2010.