Atmos. Chem. Phys. Discuss., 9, C299–C301, 2009
www.atmos-chem-phys-discuss.net/9/C299/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Relationships between size-fractionated indoor and outdoor trace elements at four retirement communities in Southern California" *by* A. Polidori et al.

Anonymous Referee #1

Received and published: 21 April 2009

General comments

This discussion paper presents an extensive dataset of outdoor and indoor measurements of trace elements in quasi-ultrafine, accumulation mode and coarse particles. As the authors rightfully state, several studies have provided information on the elemental composition of outdoor PM10 and PM2.5, but substantially fewer studies have considered also indoor PM. Indoor measurements were conducted in the main buildings of 4 retirement communities located at varying distances from Los Angeles, California. Elderly people have previously been shown to be susceptible to the adverse health effects of particulate air pollution, which adds to the relevance of this paper. In general,

people spend 80-90% of their day indoors and people with sedentary life style, including many elderly people with chronic diseases, even more. Thus, indoor concentrations of trace elements measured in this study are not that far from actual exposures.

The paper would benefit from some shortening and focusing – now all the results seem to be presented with similar levels of details, even those not contributing to the main messages of the paper. I'm also a bit worried about the large number of rather speculative explanations for the outdoor and indoor levels of various elements in the study. The speculations should be linked more tightly to the actual sources of outdoor PM at the study area (based on e.g. emission inventories and source-apportionment studies), as well as to PM generating indoor activities during the study period. To use potassium as an example, I would assume that wood combustion is relatively common in California, and so are wildfires, which should lead to e.g. high concentrations; indoors cooking and smoking are known sources of potassium.

Specific comments

-Page 4935, line 10: cut-off sizes are given in a misleading way, it should be explained that there is also the lower limit (e.g. for accumulation mode 0.25 um). -page 4937, determination of air exchange rate: authors should explain how their took into account changes in outdoor CO (Abt et al. used sulfur hexafluoride), and which were the dominant indoor sources of CO -page 4938, determination of infiltration factor: I/O ratios <1 do not guarantee that indoor sources are not active – was there no information on indoor activities during the measurements available? -table 1: information on mass and total trace element concentrations in different size fractions during the two seasons should be given, and used in the interpretation of the results -page 4940, lines 15-16: should read "outdoor-indoor" instead of "indoor-indoor"? How were the deductions concerning outdoor origin made in practice? -page 4941, variability in I/O ratios, S and R: information on the ventilation systems of the main buildings (including efficiency classes of the filters), would help interpreting the variability -page 4941, high I/O ratio of Zn: is smoking a plausible explanation in the current study? Was smoking

allowed and if yes, how frequent were smoking events? -page 4944, line 1: Zn and Fe are also closely associated with non-exhaust traffic emissions -page 4944, line 17: I'm not convinced that based on similar indoor and outdoor EF profiles one can judge that most of the trace elements were of outdoor origin -page 4945, elements as tracers of outdoor PM: it should be stated more clearly that the use of tracers for PM of ambient origin is dependent on size distribution. Thus, maybe the apparent overestimation of infiltration factor using S in the present study was not just due to presence of semi-volatiles, but also due to differences in the size-distribution of PM2.5 between locations? -page 4946, lines 7-17: how do the rather similar I/O, R and intercept for S and Ni lead to conclude that using S (but not Ni, implicitly) as a surrogate for PM2.5 infiltration leads to overestimation? Further, shouldn't sulfur levels in quasi-ultrafine and accumulation mode PM be summed in order to estimate infiltration for PM2.5? -page 4948, outdoor PM concentrations as surrogates of indoor-infiltrated PM in time-series studies: when the analyses in the present paper have not covered any longitudinal aspects, in minimum presented longitudinal indoor-outdoor correlations for elements, conclusions cannot be drawn about the use of outdoor measurements in longitudinal epidemiological studies

---

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4931, 2009.