Atmos. Chem. Phys. Discuss., 12, C12354–C12357, 2013
www.atmos-chem-phys-discuss.net/12/C12354/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A case study of the highly time-resolved evolution of aerosol chemical and optical properties in urban Shanghai, China" *by* Y. Huang et al.

R. Moffet (Referee)

rmoffet@pacific.edu

Received and published: 7 February 2013

General: This manuscript presents results from several measurements of aerosol chemistry and optical properties during an intensive sampling period in Shanghai. Overall the manuscript is well written and should be published with some improvements I mention below. The authors have done a nice job documenting the cluster analysis and particle types. There is also an excellent quantitative comparison between bulk NH4, SO4 and NO3 concentrations and ATOFMS peak area data. ATOFMS data has often been referred to as "non-quantitative", but this paper clearly shows that there is utility in the ATOFMS peak areas. The one area that can be improved the most is

the discussion on the chemical effect on optical properties. Much of the discussion is speculative and relies on empirical correlations – the strength of which may or may not depend on the reasons stated by the authors. The authors should have enough data to conclusively determine whether or not there is a chemical effect on optical properties through a closure study.

Detailed: Abstract, Line 20: I suggest moving the sentence starting with "The comparison" after the sentence directly following it.

Abstract, last sentence: It is stated that the mass extinction efficiency is controlled by the "chemical components". This is a very general sentence that does not have very much impact because everything is composed of "chemical components".

P. 31957, line 5: It is stated: "the radiative forcing of aerosols is largely determined by their chemical components. . .". This sentence has two problems: 1) aerosols are not the ones being forced, so "forcing of aerosols" is awkward – I suggest using the term direct aerosol radiative forcing instead. 2) The direct effect is more strongly controlled by absolute particle concentration and size rather than particle chemical composition. Chemical composition plays a more secondary role due to modification of hygroscopic properties and refractive index (especially in the case of soot). I suggest the authors re-word their introduction to address these comments.

P. 31961, line 11: Please describe the NOx measurements in more detail. This measurement is discussed briefly in Li et. al. 2011a, but I still think more detail regarding the NO2 correction would improve the quality of the paper.

P. 3165, line 11: Please clearly define "biomass burning". Is it possible to distinguish residential/agricultural biomass burning from coal fly ash? There is a great deal of "biomass burning" particles contributing to the overall number concentrations, so I think this is an extremely important issue to discuss from an emissions standpoint.

P. 31966, line 7: Suggest replacing the term "roller coaster" with a more formal term.

P. 31966, line 17: Regarding the metal containing particles: I would think if it was a local point source, there would be spikes in the time series. Instead, I think the more constant concentration is indicative of regional sources. To put it another way, it seems as if there is more than one source in the region leading to a constant baseline of these particles. Perhaps this is just a terminology issue and "regional source" is a better term.

Also, the metal particles are representing an appreciable fraction of the total particles and it would be nice to see a bit more discussion/analysis on the possible sub-types making up this particle class.

P. 31967, Line 13: What technique was used to correlate K+ with Cl-?

P. 31968, Line 22: The authors have shown an excellent correlation of particle acidity derived from bulk measurement with that derived from ATOFMS data. I think the authors should highlight this in the conclusions and abstract. It would be interesting to see if this correlation holds for different matrices. How strong is the correlation? It would be good if the authors could document the correlation by performing a sort of least squares fit and documenting the linear relationship.

P. 31969, Line 22: What are the uncertainties on the SSA? I would expect they go up for low aerosol loadings. I suggest the authors add error bars to their optical property measurements.

How did the authors correct for NO2 absorption? Please discuss the possible errors and QA procedures in the experimental section.

So the reader can unequivocally rule out NO2 as a possible interference, please show the NO2 time series – this can be placed in the supplementary section.

P. 31971, Line 12: The discussion of the effect aerosol chemical composition on optical properties is largely speculative. The best way to definitively conclude there is indeed a chemical effect is through a closure study whereby one calculates the absorption and scattering using measurements of aerosol size distributions and chemical

composition and then comparing to the bulk optical measurements. The authors seem to have enough data to do this. Much of the discussion that follows does not decouple the interrelated effects of size, concentration and chemical composition on optical properties.

P. 31972, Line 10: What values were measured by Chow et al.? These should be provided in the text to facilitate the discussion.

P. 31973, Line 13: What are the units and uncertainties associated with the scattering efficiencies quoted here? Was scattering efficiency properly defined in this paper? Table 3: This table could be improved by including statistics of PM concentrations during the different periods rather than just correlation coefficients.

Figure 1: it is not clear why there are three panels in this figure. Each panel should be labeled.

Figure 5. perhaps color code the text identifying the periods in panels b and c according to the dot color in panel a. This would greatly assist interpretation.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 31955, 2012.