

Interactive comment on “Geochemical perspectives from a new Aerosol chemical mass closure” by B. Guinot et al.

Anonymous Referee #2

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Review of the manuscript submitted to ACP

Geochemical perspectives from a new aerosol chemical mass closure

B. Guinot, H. Cachier, and K. Oikonomou

I think that this manuscript suggest a simplified procedure to aerosol mass closure that is interesting. To compare with traditional mass closure with trace element measurements, the authors could have try to use their procedure in databases when traditional method was used. There is a large number of studies with OC, EC, BC, ion and elements were measured at the same time, and these data sets are available. Instead, the authors have used their own data sets that do not have silicon or iron to check soil dust impact, for instance. I think that the authors have overstated their evaluation of

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uncertainties, being very optimistic in claiming 1% uncertainty in ionic concentrations. The authors do not even mention errors in sampling (flow rate and volume integrators) that could easily be larger than 5%, or errors in filter blank subtraction that could add another 5% easily for certain species. The estimated uncertainties are too optimistic. If the authors had run 2 SFU in parallel and compared ionic concentrations, they would observe that 5-15% uncertainties is more realistic.

Soil dust composition is not constant even for the same site. Urban traffic resuspended dust has different composition as long range transported dust. The mineralogical content of dust also varies with wind direction and transport characteristics. This could be especially true for Beijing. I think this issue must be discussed in the text.

I also think the title is not appropriate for the manuscript. I do not think that this provides a geochemical perspective. It is only a simplified approach to aerosol mass closure.

But there is nothing really wrong with the manuscript, with possible exception of the optimistic errors estimates. I recommend that ACP publish the manuscript after a revision taking into account the specific points discussed below.

Specific points:

In the abstract : “The coarse mode soluble calcium is shown to display a correlation (regression coefficient f , y axis intercept b) with the missing mass”. YES, of course because soluble calcium is a tracer for soil dust that has low organic contribution. The issue of soluble to insoluble calcium in soil dust particles is not appropriately discussed in the manuscript.

In the abstract: “ Using the f and k coefficients obtained from the data set, the mass closure is reached for each individual sample with an undetermined fraction less than 10%”. What about water content? It is supposed to be less than 10% or it is imbedded in the calculated factor?

In methods: You do not mention the use of Apiezon coated filters in the SFU: “stack

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filter unit (SFU) samplers working at $1\text{m}^3\text{h}^{-1}$ with two filters of different pore size used as follows: Nuclepore $8.0\ \mu\text{m}$ filters collect particles with aerodynamic diameters higher than $2\ \mu\text{m}$, while downstream $0.4\ \mu\text{m}$ filters collect particles smaller than $2\ \mu\text{m}$. Have coated filters been used? If not, which are the consequences? In dry conditions the differences could be very significant.

You conditioned the filters at 30% RH. But this do not mean you do not have significant amounts of water in the aerosol: “RH is set to refer to a dry aerosol in order to minimize uncertainties due to water adsorption-desorption by the filter media and aerosol particles (MacMurry, 2000)”.

Be careful on reporting uncertainties: It is impossible to have atmospheric ion concentrations uncertainties within 2% if your flow rate and volume for each filter has errors larger than 5%. I think you were too optimistic in the phrase: “Averaged uncertainty on the major ions results is of the order of 10 ppb, which represents less than 2% of uncertainty in atmospheric ion concentrations for most of our field campaigns.”

Another TOO optimistic phrase: “We eventually get rid of both positive and negative artifacts by heating samples at $60\text{ }^\circ\text{C}$ for 15 min prior to analysis, as similarly reported by Malm et al. (2005).” Are you sure you get rid of ALL organic aerosol sampling artifacts? I do not think so.

Figure 1 shows a comparison of TSP versus TEOM. What TSP (Total Suspended Particulate) is this? Measured by HighVol? Which size range? Or is just gravimetric mass from SFU, that is NOT TSP at all. I guess this is gravimetric PM₁₀ measurements, not TSP. This needs clarification.

Figure 3 - I think there is a mistake here. The legend mentions: “(c) Mass closure of the coarse mode: Consistency between the actual weighed mass and the reconstructed mass.”. The plot actually is Calcium versus missing mass. Figure 4d is a very good fit. Maybe too good?

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Page 12033 line 20: What about the non-soluble Calcium? How do you account for it? Your phrase bellow just recall on the subject, but do not offer a justification for the consideration that all calcium measured is only the soluble fraction: “It must be recalled that calcium is assumed to be totally dissolved as Ca²⁺ during the extraction phase and may be obtained by ion chromatography”.

The quality of the plots is very poor. They must be very significantly improved. Some are almost impossible to read.

I think is too optimistic your error evaluation: “The major uncertainties are brought by the gravimetric measurements and the carbon measurements as both may influence the aerosol mass from 1% to 2%”. I think that variability in blank values and analytical uncertainties make 1% precision impossible to achieve. What data supports such strong statement?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 12021, 2006.

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