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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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General Comments:

The paper presents a novel approach to mass closure which could be used when no elemental analysis is available. The paper is well written in English but some errors are present in the text (see technical corrections), methods and assumptions are clearly described and an evaluation of uncertainties is also given.

A major criticism about this work relies on its "self-consistency". To be proposed as a valuable and robust alternative method, the comparability of its results should be proved against those given by more traditional approaches for mineral dust assessement.

According to the referee's opinion the title is a little bit confounding (especially the

words “geochemical perspectives”) and it could be simply changed in “A new approach for aerosol chemical mass closure”.

Specific Comments:

In table 1 the worst results are for Florence monitoring station: have any explanation for this? As regards to Florence results, in page 12039 lines 17-19 the contribution from biomass burning is intended to be wood burning for domestic heating? If it is the case, differences between the wintertime vs. summertime results would help in the identification of the real cause of differences between Paris and Florence. On the other hand, if you mean biofuels instead of biomass burning you should specify it.

Page 12026 from line 27: The “burning procedure” that you cite, apart from differences in temperatures, is what is usually called “pre-firing” to obtain low blank values? How the pre-firing of the blank filter can influence and minimize sampling artifacts? Moreover, how can you get rid of negative artifacts heating the samples prior to analysis? Please specify.

Page 12030 and Figures 1a, 1b: the problem due to heated inlets for TEOM is well known and here it is not clear the relevance of this observation. Both the text and the figures could be eliminated.

Page 12039 line 1: what the authors mean exactly with the sentence “photochemistry giving a more important abundance in summer than in winter for nitrates” ? Generally, low temperatures and low mixing layer heights favour nitrate particulate phase during winter periods.

Page 12040 paragraph 5.3: why these results should give consistency to your protocol? Please clarify what you mean exactly. In table 4 the choice of different stages appear to be a little bit “subjective”

Page 12030 lines 10-12 and page 12031 lines 28-29: why in Paris the unbalanced Ca2+ is only considered as carbonate? How can you exclude the presence in the

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coarse mode of compounds similar to those found in Beijing? Finally, it is not always clear if you consider the carbonate a soluble or insoluble species.

Page 12040 line 8: the ratio is BC/CO or BC/OC?

Technical corrections:

All references reported in the text should be carefully checked as they present a lot of errors (many differences in years between references in the text and those in the "reference" section; moreover, Guinot et al. 2006 is not reported).

Page 12025 line 1: SFU is "stacked filter unit"

Figure 3a: the caption left/right should be changed in top/bottom

Page 12027, line 17: the Birch & Cary protocol is generally referred to as NIOSH 5040 and not NIOSH 5054, please check it.

Figure legends and axes are difficult to read.

Figure 3b and 3c are identical despite the description in the caption !

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