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Interactive comment on "Quantification of organic carbon sampling artifacts in US non-urban and urban networks" *by* J. C. Chow et al.

Anonymous Referee #2

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This referee is more than delighted with this very timely publication, because the EU has started a norm-commission on measuring OC (and EC) in PM. This measurement is obligatory for EU-countries, as stated in the new Air Quality guideline of 2008, which actually requires measurement of OC (and other PM2.5 species) as of now at regional sites.

However, for appropriate measurement of OC in Europe information on sampling artifacts is lacking and this publication is crucial in proposing a scientific guideline for the sampling in the CEN-commission TC264-WG35 responsible for the scientific basis of a guideline. Therefore the information presented by Chow et al. in this manuscript of the experience in evaluating data of the rather dense US-networks is therefore of utmost importance for the EU and the European measuring community.

The referee himself is more than happy to notice that "average" IMPROVE bQF and QBQ are comparable, as stated in the publication. In his own study he was only able to investigate bQF only and assumed that the values were comparable to QBQ values and thus for the positive absorption artifact. He is delighted that (at least in the US) this seems to be the case. The reason for his delight is that he made the assumption that bQF because he already used it to estimate the positive artifact of passive VOC adsorption. This study seems to warrant this inference.

It should be mentioned that his bQF were in the field for even a more extended time than in the IMPROVE network (namely 14 days instead of 14 days). In this respect this likes to refer to a large set of, yet unpublished, data of Dr. Ralph Lump, Karlsruhe, who made a study on the accumulation of OC on bQF as a function of time in the field. He noticed an accumulation of VOC blank values, but with the most significant amount accruing in the first few weeks.

While having said this admiring words about the study this referee has (especially) some difficulty with the wording of summary of this work, meaning the title and the abstract.

TITLE This referee would strongly suggest / request for the sake of simplicity to call the title:

Quantification of sampling artifacts of organic carbon in PM in US networks

ABSTRACT

In his opinion, the abstract does not fully convey the thrust of the conclusions of the work.

This specifically applies to the following passages:

"Lower STN/CSN flow rates and larger filter deposit areas result in 9–20% of the areal density (μg/cm2) compared to IMPROVE areal deposits".

This passage should be explained in more understandable language. What, I think, is meant is:

1) The field blank of STN is not representative for a real filter exposure because field blank is only exposed for 12 minutes. 2) It should be stated that the areal density of collected PM/carbon is much higher in IMPROVE samples thus reducing the relative contribution of the blanks to a much lower proportion than in the STN sampling. This is due to a higher sampling rate combined with a much lower filter surface area loaded. This is also in the abstract the crux of the message for the sampling community in Europe, see below.

General comment with respect to this: In Europe the, obligatory, reference sampling occurs with a sampling rate of 34 l/min with the Klein Filter Geraet. It might thus be that this sampling will also become the standard in determining the composition of PM2.5. Sampling occurs on a standard 47 mm quartz filter, quite often Whatman QMA is used.

In this respect the areal density (loading) and face velocity of the KFG are more similar to that used in the IMPROVE than in STN. This makes the experience with the IMPROVE sampling of more interest.

A second passage with comments from my side is:

The sentence “STN/CSN bQF values are 11–34% lower than linear regression intercepts derived from collocated IMPROVE-STN/CSN data pairs.”

It should be mentioned here that in the main text of the manuscript this concerns the uncorrected / raw data. The non-zero intercept is indicative of a much higher blank in STN, expressed as mass concentration as compared in collocated IMPROVE samples. Again this is highly indicative if not proof that higher face velocities are to be preferred in networks in which only a single filter can be used for daily sampling for OC analysis. He challenges the authors to agree with this conclusion or have a serious rebuttal to this conjecture.

Summarising, This referee likes the above changes to be seriously considered by the authors before submitting a revised version.

As for the main text, it seems sometimes a little hard for a European-based audience to locate the various measuring locations and to appreciate the possible implications of measuring at those sites for the OC-artifacts. Vice versa, this would apply for data from Europe to be interpreted by US-scientists. Still the fact hat the artifacts might be site dependent makes it more complicated to appreciate the findings in this manuscript. In this respect it is not clear whether the artifacts are higher in the urban or in the non-urban networks. A clear indication is not provided in this manuscript.

As for nomenclature: it is extremely difficult to simplify the issues brought forward in this manuscript. Still the use of abbreviations like btQF is understandable but hardly different at first sight form the abbreviation bQF. This referee, though not very comfortable with the abbreviations for the various types of blanks and acronyms does not have himself a better alternative for these. Still it might be worthwhile to come to an international agreement on describing such blanks in a more definite and acceptable way.

Summarising, the a manuscript provides a wealth of information on a very complicated issue, namely the blank values of filters used for collection of aerosol/PM carbon and this investigation more than warrants publication.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27359, 2009.