

Interactive comment on “Characterization of aerosol chemical composition by aerosol mass spectrometry in Central Europe: an overview” by V. A. Lanz et al.

V. A. Lanz et al.

valentin.lanz@psi.ch

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We thank anonymous referee #2 for the helpful comments on our ACPD manuscript. Please find below our responses directly following the reviewer comments 1-11.

Comment 1: *One important piece of information that is missing from this paper is the mass spectra of the components determined for each study. The mass spectra gave valuable information about the chemical characteristics of the components. It is important that the authors show them and make relevant discussions. It is curious to know what they look like and how they compare among the sites of this study as well*

C10730

as vs. OA components published in other studies as deemed appropriate.

Response 1: The mass spectra of the organic components were/will be published separately for Zurich summer (Lanz et al., 2007) and winter (Lanz et al., 2008), Grenoble (Favez et al., 2010), Massongex (Perron et al., 2010), and the Rhine Valley (Weimer et al., 2010). Furthermore, in the supplementary material of the revised ACP version we will show the mass spectra of all OA components from all campaigns investigated in this study. In addition, they will be made available in the online mass spectral database maintained by the Jimenez-group, Boulder CO (as already mentioned on P. 25003 in the ACPD manuscript).

We additionally note that the OOA spectra reported here made part of a detailed discussion by Ng et al. (2009). We will add this reference to our revised paper (in Sect. 3.2.1). Moreover, we will also add a paragraph on the separation of OOA into SV-OOA and LV-OOA and the (dis-)similarities of these components as outlined in our response to anonymous referee 1 (AC C9815, Page C9825 – C9826).

Whilst the OOA were very similar to each other and to measured references spectra (typically showing coefficients of determination, R^2 's, between 0.90 and 1.00), the primary wood burning spectra were less similar (R^2 's between 0.80 and 0.90) to each other and external references (which can be downloaded from the mass spectral database; Ulbrich et al., 2009). This latter observation is not very surprising when we consider the widely differing mass spectra of primary wood burning OA resulting at different burning conditions (Weimer et al., 2008). On the other hand, Jimenez et al. (2009) pointed out that OA from a variety of sources will eventually converge to chemically similar, highly aged, and secondary OOA components. This will be highlighted in the revised paper and we will also incorporate our response to Comment 11 on profiles of OOA and P-BBOA (see below).

Concerning the inter-comparison of HOA-profiles we note those spectra were most similar and typically exhibited R^2 's from 0.95 to 1.00 to each other. However, in half

C10731

of the data cases the HOA-profiles were imposed and their approximate shapes were prescribed.

Lastly, we note that the OA component spectra can also be characterized by important tracer species (e.g., organic m/z 's 44, 57, and 60), which was done here as well and intensively discussed on P. 25002, L10 and P. 25003, L25 as well as on P. 25020. The abundance of the key fragments in average OA was discussed at those instances as well.

Comment 2: *In "2.4 Aerosol neutralization", only Takegawa et al. 2006 was cited for the using of ion balance data derived from AMS measurements to determine aerosol neutralization or acidity. I'd like to point out that this approach was first reported in a paper published in JGR in 2005 (Zhang et al., 2005) and was discussed and qualified in detail in a later paper by the same group of authors in EST in 2007 (Zhang et al., 2007). Original works deserve to be cited too.*

Response 2: Will be done.

Comment 3a: *Fig. 4, figure caption, how were the uncertainties determined? The error bars may be used to show the variability (e.g. 1 stdev) of the values too.*

Response 3a: The vertical bars represent the standard deviations of the mean ratios z ($z=x/y$), where x ="concentration of organic marker" divided by y ="total organics concentration". Both x and y need to be associated with uncertainties and the relative standard deviation of the mean ratio z (as mentioned in the captions of Figures 2 and 3), $\text{rel.std}(z)$, was calculated as a propagation $\text{rel.std}(z)=(\text{rel.std}(x)^2 + \text{rel.std}(y)^2)^{(1/2)}$, where $\text{rel.std}(z)=\text{std}(z)/z$. Please note that the standard deviation of the mean, std , can be related to the simple standard deviation, sd , via $\text{std}=\text{sd}/(\text{number of samples})^{(1/2)}$.

The horizontal bars represent an assumed $\pm 5\%$ absolute uncertainty for the OA components, which represents the retrievability of the PMF-AMS method and an approxi-

C10732

mation for different levels of uncertainties (e.g. number of factors or rotations) - instead, the standard deviations of the resulting OA components were often lower (Lanz et al., 2007, 2008, Ulbrich et al., 2009, Allan et al., 2010).

Comment 3b: *[Fig. 4 . . .] Also, it may be useful to color the data points based on the categories classified in Fig. 2.*

Response 3b: Will be adopted correspondingly in the revised paper.

Comment 4: *For the discussions on CE values, it is important that the authors cite more papers and expand the discussions on the use of variable CE throughout a given study or among different studies. The authors mention that Takegawa et al. 2009 applied CE up to 1. But it is important to note that Takegawa used CE = 1 for a study conducted near Beijing during summer, when RH tends to be high, aerosol was composed of high fraction of NH_4NO_3 , and particles were likely to be hydrated. This is consistent with another study conducted in Beijing during July 2006 (Sun et al., 2010), in which a variable CE was applied based on comparison between AMS vs. SMPS. Also, the dependence of CE on particle acidity was observed by Kleinman et al. (2006) and CE = 1 was found appropriate for strongly acidic particles. In addition, note that there are a large number of other studies justified the use of CE = 0.5. The review paper by Canagaratna et al. (2007) should be cited to help interested readers find more information about this subject.*

Response 4: We will declare that constant CEs were assumed for each study, but that the CE is in fact a function of different physical and chemical properties of the aerosols (see also AC C9815, Page C9833 – C9834). We will add a new column to Table 2 ("CE determination") and mention the ancillary measurements (e.g. " $\text{PM}_{2.5}\text{-SO}_4^{2-}$ ") or references from the literature (e.g., Kleinman et al., 2007, Canagaratna et al., 2007, Sun et al., 2010) that were considered to derive appropriate collection efficiencies. Furthermore, sections on the collection efficiency will be changed as requested by

C10733

anonymous referee #1 (see also AC C9815, Page C9833 – C9834).

Comment 5: *In table 2, it is useful to add notes next to the CE values indicating their sources, such ACPD as citations to articles on individual studies.*

Response 5: Please see comment/response above.

Comment 6: *Also in table 2, I don't understand why the STP-conversion column show fixed value for each study? Didn't ambient air change temperature during any study?*

Response 6: The STP-conversion factors are not the same for the sites presented here: they range from 1.10 (Grenoble) to 1.67 (Jungfrauoch) (Table 2) and mainly reflect decreasing ambient pressure (increasing altitude). The temperature in the measurement trailer was relatively constant and had in fact only a minor influence on the calculated conversion factors.

Comment 7: *Page 24995, line 16, the discussion is quite interesting. What's the fractional concentration of sulfate in PM1 in regional and aged background air of Roveredo? Was the wind speed low during this study period?*

Response 7: During the campaign in Roveredo, November-December 2005, the wind speed (at 10 m above ground level) was typically < 1 m/s and the air masses were overall rather stagnant. We did not perform AMS measurements outside Roveredo in parallel. Therefore, we can not determine the sulfate concentration in aged background directly. Valley venting may have occurred during the night (when SO₄²⁻ emissions from wood stoves need to be considered as well). Thus, even when filtering the in-situ data from Roveredo by meteorological conditions we still can not rule out local influences on the observed sulfate level. Nevertheless, we stated that sulfate was a rather regional pollutant (see also our response to anonymous referee #1): for the other sites investigated here sulfate contributions to total AMS-aerosol were about 10-20% NR-PM₁ or 2 μg m⁻³. This is clearly higher than the sulfate fractions and concentrations (3% and C10734

0.9 μg m⁻³, respectively) found for Roveredo, November-December 2005.

Comment 8: *In discussing the chemical compositions of PM1 (3.1), how about giving a table that lists all the numbers? The PAH results are interesting but only the values of a few sites are cited in the texts. It will be useful that the data from other sites can be found somewhere.*

Response 8: We will provide such a table in the supplementary material of the revised paper.

Comment 9: *"LV-OOA" and "SV-OOA" were first introduced by Jimenez et al. in a recent Science paper. It should be cited accordingly.*

Response 9: We note that this work was cited in the ACPD version on P. 25019 (Fig. 3, caption: "The low-volatility (LV-OOA) and semi-volatile (SV-OOA) fractions of OOA had previously been called OOA1 and OOA2, and were now changed into more descriptive terms (Jimenez et al., 2009)."). We will furthermore add on P. 24999, L7: "[...] (new nomenclature according to Jimenez et al. (2009) for the components formerly called OOA1 and OOA2, respectively) [...]."

Comment 10: *Line 8, p 25000, missing "to" after due.*

Response 10: Will be corrected.

Comment 11: *P 25001, if a portion of OOA is fresh wood combustion OA, how likely BBOA signatures, such as m/z 60 and 73 according to Alfara et al. (2007), are seen enhanced on the OOA spectrum?*

Response 11: Primary OOA-like emissions from wood burning did not show a marked increase in the m/z's 60 and 73 fraction of the spectra and were as low as 0.3% of OA in the study by Weimer et al. (2008). Depleted mass fragments 60 and 73 can

be explained by combustion conditions, at which levoglucosan is pyrolyzed. Such mass spectra from primary OOA-like wood burning are difficult to distinguish from mass spectra of secondarily formed OOA. However, as OOA was correlated well with secondary inorganics and rather poorly with tracers of primary combustion (CO , NO_x) it can be assumed that this type of interference (i.e., the misclassification of a primary OOA from wood burning as secondary OOA) was a minor issue here. In OOA spectra found for sites with a strong wood burning influence, m/z 's 60 typically was $<1\%$. The campaign in Roveredo, November-December 2005, represents an exception in this respect (discussed on P. 25019 of the ACPD manuscript), where m/z 60 was about 1.2% of OOA.

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C10736

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C10737

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C10738