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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The authors described results from 14 field campaigns using a commercial Aerodyne Aerosol Mass Spectrometer (AMS) at 10 sites in Switzerland, Austria, Liechtenstein, Germany, and France. Non-refractory aerosol species, including organics, sulfate, nitrate, ammonium, and chloride, were measured using AMS instruments; in addition, complementary black carbon measurements were made during most studies. For most studies, factor analysis was utilized to separate the following organic components: low-volatility oxygenated, semi-volatile oxygenated, hydrocarbon-like, primary biomass burning, and local organic aerosol.

My major concern is that this "overview" reads as an archival data report, rather than

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a scientific discussion. While a worthwhile study, the context and overall findings of this overview are not clear. Who is the intended audience? What is the result of comparing all of the studies? What new was gained from the overview? These points are not clear and must be made so in a revised manuscript. In addition, care should be taken to making sure the manuscript flows and is organized well such that the main take-home points of each paragraph and section are clear. Much of the results and discussion section, for example, is difficult to read and does not flow well. In addition, the overview requires an added section discussing each of the sites and what aerosol sources and processes they are impacted by. Much of the manuscript only states the results of the data analysis, but it does not discuss the science. Overall, the manuscript requires a discussion of results and their implications.

Major revisions are suggested with particular attention to data interpretation and discussion. For an overview manuscript, overall trends and patterns should be discussed in detail. If the authors do not believe that trends and assumptions may be discerned from this study and presented to the scientific audience for future research, then the authors should wait until additional field campaigns are completed to present this data. Major and minor comments are noted below.

Major comments:

Abstract: Provide context for this work; why is it needed? What unique was gained from this overview?

1 Introduction: Currently, the introduction is lacking in background motivation for this overview; over half of the introduction concentrates on introducing the locations and methods of analysis, rather than providing scientific motivation for the study. In addition, no background is provided for previous measurements of aerosol chemical composition in the “greater Alpine region”; this should be discussed and provide motivation for why an overview of AMS measurements is needed/useful. While it is noted that previous AMS studies for this region have been published, no information is provided regarding

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their results. I suggest moving the discussion of factor analytical approaches to the methods section. Further, while it is noted that the Zhang et al. 2007 overview did not include most of the sites within this overview, it is not discussed why this overview is needed in particular; what does this overview provide beyond Zhang et al. 2007?

2 Methods: Is Section 2.5 needed, or could it be shortened and combined with Section 2.2 or provided as supplemental material? It appears that the factor analysis used is not new, and the discussion of it is tedious to read.

3 Results and discussion:

- Expand the discussion of NR-PM1 mass concentrations on page 24994 (lines 24-28). Provide values associated with “high” and “lower” concentrations in the text. Put these studies in the context of previous work and what would be expected. Provide more comparisons between studies.

Section 3.1: - Currently, the first paragraph is difficult to read and easily glean the important points. In addition, it should be expanded with greater discussion of each of the chemical components, suggesting possible sources for sites other than Roveredo. In addition, a discussion of absolute species concentrations (similar to that done for sulfate) would likely be of interest. The following paragraphs provide a nice discussion of the data; this type of discussion, describing the sites and data in detail, should be emulated throughout the manuscript.

- The paragraph spanning pages 24997-24998 seems to contradict that of the discussion spanning pages 24995-24996, where it seemed to state that the more aged, higher altitude aerosol was acidic. Please clarify these discussions so that they agree and are clearer.

Section 3.2: - The first paragraph provides a nice discussion and comparison with Zhang et al. 2007, particularly with respect to outlier situations.

- When separation of LV-OOA and SV-OOA is not possible, for the winter campaigns,

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do the organic mass spectra more resemble the SV-OOA mass spectral signature as one might expect, given the discussion on lines 16-20 on page 24999?

- What is the suggested reason for the "high" HOA contributions in Hohenpeissenberg? What is the suggested reason for the "low" P-BBOA fractions for the winter Swiss Plateau studies?

4 Conclusions: The first paragraph is a good start for a summary of overall findings. However, on page 25005 line 11, it is stated that "nearly homogeneous chemical composition. . .resulted from 13 campaigns. . ."; does this mean that the conclusion of the paper was that aerosol chemistry was similar/constant for all sites and seasons? Discuss this further as it is an important implication of the overview. Also, be careful with the word "homogeneous"; does this mean the aerosol is similar, or homogeneously mixed, or what? Further, on page 25006 line 1, it is stated that "large variation was observed" for the different inorganic aerosol fractions; this seems to contradict the above statement. Another main result of the overview appears to be the importance of biomass burning in the region; however, hasn't this already been concluded from AMS work presented in other manuscripts? At the end of the conclusions section, it is stated that additional field campaigns are necessary to validate trends and patterns. Little discussion in the conclusions is provided for any observed trends/patterns. However, the point of an overview should be to describe trends and resulting assumptions that can made for future work.

Minor comments:

Page 24987, line 18 – Clarify what is meant by "low sites". Does this refer to altitude?

Page 24989, line 17 – "C" does not appear to be defined.

Figure 1 – Swiss border line not clear. I assume darker shades are higher altitude? This is not clear. I would suggest labeling the different countries on the map for individuals not as familiar with Europe. Label x and y as longitude and latitude. Show approximate

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locations of mobile studies.

Page 24989, line 26 – Provide altitude range for the elevated sites here as well.

Page 24990, lines 1-3 – The Swiss Plateau is discussed here; however, it would be helpful if these locations were also noted for reference in Figure 1.

Page 24990, line 14 – "on" should be "into"

Page 24990, lines 20-21 – Metals would also not be detected.

Page 24991, line 19 – Should say "...concentrations were...".

Page 24991, lines 19-23 – Can you comment on any uncertainties/differences introduced from using these different techniques?

Page 24991, lines 24-27 – Can you comment on uncertainties introduced from the different size cut-points?

Page 24992, line 14 – As worded, this is confusing since the AMS measures only positive ions. I believe the authors are trying to say that they assume ammonium is the only cation in the aerosol balancing sulfate, nitrate, and chloride.

Page 24992, line 15 – "neq" is not defined.

Page 24992, lines 21-22 – Should this say "...dimensions as samples..."?

Table 2 – Is it not clear why the mean mass concentrations are given as ranges for certain studies. Also, the label "BC (%NR-PM1)" is misleading since BC is not NR. The equation in the caption helps to clarify this; however, the phrase "fractions of NR-PM1" is still misleading. Also, I assume "STP-conversion" in the table actually lists the "conversion factor" rather than the converted data(?). I assume that the mean mass concentrations shown take the listed CE and conversion factors into account(?); make sure this is clear in the caption.

Page 24994, line 2 – Note NR-PM1 here.

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Page 24994, lines 7-10 – Does this refer to species other than sulfate, and thus, CE was based off of the sulfate comparisons alone? Make this statement clearer. Also, can you add a comment about the impact of different size cut measurements?

Page 24994, lines 13-17 – Add a short discussion about possible reasons for the changing CEs from study to study. Can this tell you something about the particles? Otherwise, lines 4-23 should be moved to the methods section presumably.

Page 24995, lines 1-8 – Move the BC discussion to Section 3.1 as it a discussion of chemical composition.

Page 24995, line 3 – Missing parentheses after "PM1".

Page 24995, line 3 – Perhaps list maximum BC fraction here as well?

Page 24995, line 5 – Provide actual values here in the text for the "low" BC fractions so that the reader can compare with the >15% value reported on line 3.

Page 24995, lines 6-8 – Without fractions listed in the above sentences it is difficult to use this discussion as a comparison.

Figure 2 – Mention that these are AMS measurements in the caption. Also, the division of the sites between the "Swiss Plateau" and "Alpine region" seems useful. Could this be done in the Tables and Figure 1 as well to make things clearer? It would also be useful to have a definition of these two areas (and what sites fall into them) somewhere before the introduction of this figure in the text.

Page 24996, lines 19-21 – Have other studies in this region, and/or others, observed this seasonal difference in chloride as well?

Page 24997, line 8 – By "homogeneous", do you mean "similar"? "Homogeneous aerosol composition" can have different meanings, so I would suggest making this clearer.

Page 24997, line 21 – Reference for faster photochemical degradation of PAHs? Is

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the degradation itself actually faster, or is there just more photochemical reactions occurring?

Page 25000, line 8 – ". . .due the. . ." should say ". . .due to the. . .".

Page 25000, line 22 – Clarify what is meant by "almost as uniform".

Page 25000, line 25 – ". . .as relevant. . ." should say ". . .as a relevant. . .".

Page 25000, last paragraph – This discussion seems misplaced within this section and doesn't allow the section to flow properly; consider reorganizing.

Page 25001 – Would it be possible to reorganize and combine these last two paragraphs with the earlier discussions of OOA and P-BBOA in this section? It may help the section flow better.

Figure 4 – Does "modeled" mean the result of factor analysis? This is not clear.

Page 25002, lines 18-21 – Is this saying that stronger correlations are observed for winter campaigns when high P-BBOA periods were excluded? This is not clear. Why should the relationship between m/z 44 and OOA fractions depend on OOA loading?

Figure 5 – What does "pre-Alpine" mean? I don't think this term had been used or defined yet.

Page 25003, line 29 – "is" should be "are"

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 24985, 2009.