

Interactive comment on “Size-resolved aerosol composition at an urban and a rural site in the Po Valley in summertime: implications for secondary aerosol formation” by S. Sandrini et al.

Anonymous Referee #1

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General comments

The manuscript presents size-segregated aerosol measurements made with a Berner Impactor in summer 2012 at a rural and an urban site each in the Po Valley, Italy. Sampled particles were chemically analyzed for inorganic and organic species, and Principal Component Analysis was done for the differentiation of aerosol components. This is a well written paper that presents a careful analysis of an interesting dataset. I thus recommend publication after the comments below have been addressed. Quite a few comments are formulated as questions, which are meant as encouragement for the authors to add the answer to the manuscript in the form of additional explanations.

Specific comments

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Abstract: Mention which method you used to do the chemical analysis in the abstract.

P. 2, l. 20: Specify quasi-ultrafine

P. 3, l. 1-2: This statement is not entirely correct. The sampling efficiency can be close to unity for particles with a diameter up to $2.5\ \mu\text{m}$ when using a high-pressure aerodynamic lens (<http://www.atmos-meas-tech.net/6/3271/2013/amt-6-3271-2013-discussion.html>).

P. 3, l. 24: Say in a sentence or two what the Lenschow perspective is, so that the reader gets a basic idea without having to consult the reference.

P. 3, l. 37 – p. 4, l. 6: How is the predominating wind direction in the Po Valley (and how was it during the measurement campaign? Was SPC mainly downwind of BO, or were both sites downwind of Milano? Were the air masses ever coming from the East (sea)? Please add a few sentences here.

P. 4, l. 18 – 30: Is there any information on the sampling efficiency (e. g. bouncing of particles from the impactor)? And is there a large influence from gaseous organic compounds being deposited on the aluminum foils? I imagine the mass of semi- or low volatile gaseous material deposited during a 12 hr sampling period could add substantial signal to the CO₂ signal. Please elaborate.

P. 6, l. 22 – 27: During the periods with the two most prevalent clusters (1 and 5) – were the meteorological/ boundary layer conditions such that the measurement stations were heavily affected by these air masses? Inversion layers and/or low boundary layer conditions would make very local air masses more important. Please elaborate and add data if possible.

P. 7, l. 1 – 2: High PM mass loadings go together with shorter trajectory lengths, a more regional/local aerosol, and a higher fraction of the smaller particles. Based on this shift in size distribution you make the assumption that there is a larger contribution from secondary components. Couldn't there also be a larger contribution from

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primary emissions (e. g. soot or primary organic particles from traffic or e. g. biomass burning)?

P. 7, l. 19 – 22: The partitioning of ammonium nitrate (AN) into the gas phase shows a strong temperature dependence (as you mention a few times in the manuscript). If the T difference between day and nights were significant, this could be a reason for the increased AN contributions at night. Mention this already here.

P. 7, l. 34 – 35: Ammonia concentrations could be higher at the rural site due to agricultural emissions, leading to higher AN formation. You mention this on p. 10, l. 35 – 37. I would add a similar statement here.

P. 8, l. 33 – p. 9, l. 4: Can you say anything about boundary layer heights for the two stagnant periods? It is rather surprising to me that the shorter period, when air masses would have less time to accumulate, showed higher concentrations. How do the ALWC for the 2 periods compare? RH conditions seem to have been different for the two periods (also compare paragraph p. 10, l. 28 – 39).

P. 9, l. 23 – 25: This statement here is very vague and without much empirical evidence. I suggest removing it from here.

P. 10, l. 5: The numbers are reported for 25°C. What were the night time temperatures, and the respective RHD?

P. 10, l. 18 -21: The authors show in Figure 8 the correlation of e. g. nitrate and ALWC, however discuss it as “relationship between nitrate or sulfate and RH”. As a correlation of especially nitrate and RH is much less meaningful (both are inversely related to temperature and thus higher at night, which probably drives the correlation) than nitrate and ALWC, to which the authors seem to agree, at least according to Fig. 8, I suggest revising that paragraph accordingly.

P. 11, l. 25 – 30: With WSOA also of predominantly secondary origin, the common regional nature of WSOC and SO₄ can be a reason for their correlated temporal pat-

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terns.

P. 13, l. 12: There is quite a large fraction that remains unexplained with the choice of 6 principal components. Please add a short paragraph (here or in the supplementary section) on how this unexplained fraction depends on the number of principal components.

P. 13, l. 26 – 31: RC1 is not only related to photochemical production, but is also of regional nature (see previous comment). This is an important statement to make as it has implications not only for science, but also for air quality policy makers – regional aerosol is much more difficult to control due to the large area of formation. This is only mentioned in “Discussions and conclusions”, but I suggest adding it here already.

P. 14, l. 1 – 6: Is there a correlation of this component with wind directions from the sea? If you have such data it would make a stronger statement for the identification of this component.

Technical comments

P. 2, l. 19 : WINC?

P. 3, l. 27: “such as” gives the impression of the authors having tried several approaches, which, however, are not mentioned. Please revise.

P. 4., l. 21: TOC-VCPh?

P. 8, l. 11: “Figs” instead of “Figg”

P. 14, l. 19: Should be “at minimum”

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1036, 2016.

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