

Interactive comment on “Measuring condensation sink and ion sink of atmospheric aerosols with the electrical low pressure impactor (ELPI)” by H. Kuuluvainen et al.

H. Kuuluvainen et al.

annele.virtanen@tut.fi

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We thank the anonymous referee for constructive comments and suggestions. Our responses to all the specific and technical comments are listed below.

Specific comments

However, to my opinion there are two major problems with this paper:

1) It fits better to the scope of Atmospheric Measurement Techniques (AMT) than to ACP. I would have suggested submitting the paper to AMT instead to ACP.

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In our opinion the article would fit well into ACP as a technical note.

2) A new method requires laboratory calibration and validation. A field calibration, i.e. the test and calibration of the method using a rather arbitrarily chosen 10-day data set from a field campaign is not sufficient. To my opinion the method needs to be tested, validated and calibrated in the laboratory, with well defined particles of known size, composition and concentration. Under laboratory conditions, the range of values for the particle number concentration and the particle surface area can be controlled and the method can be tested also for extreme values. Uncertain data points (e.g. several outliers in Fig 5) can be repeated and validated. I therefore suggest that the authors add laboratory data using generated particles with several different sizes and concentrations in order to put their calibrations on a more solid basis. Another important question is the possible time dependence of the inferred calibration factors. It is necessary to test whether the calibration factors remain constant over a longer time period or after relocation of the instrument. Also, the influence of variation in pressure, temperature and relative humidity needs to be determined.

At first, we made laboratory measurements and validation using generated NaCl particles with different particle concentrations and sizes. Variation of concentration had no effect on calibration, and particle size affected it as the response function predicts.

Unfortunately, we did not have continuous field measurement data over a longer time period. However, we checked other shorter periods and the values of calibration factors differed at maximum 10 % from the values we got in May. However, as we say in the article, “The period of May was chosen because there were a number of changes in the size distribution of particles as well as in temperature, and these ten days represent well the round-year average.”

Considering the effect of temperature, pressure and air humidity variation on the measurements, our measurement setup was incompletely described. We added a paragraph to the section “Measurements” and described, how these environmental factors

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stayed stable in the measurements. The sample air was dried and all the measuring instruments were at room temperature.

In calculating the sinks we have used measured outdoor air temperature, which also affects ion mobility properties. However, it has only a minimal effect on calibration factor values and no unambiguous change is seen in calibration factor values as a function of temperature. Pressure is kept invariable in the calculations, but natural air pressure variation has even smaller effect on the response functions of the sinks than temperature.

Technical comments

Page 15869, line 11 "intimately", is his the right word here?

The word "intimately" was changed to "closely".

Page 15871, lines 6-8 Move the sentence "In this paper..." to line 13, after "... Polling et al. (2000).", because up to equation (5) the statements are still general and not specific to H₂SO₄

Corrected as suggested.

Page 15871, line 14: "... of sulfuric acid." Remove "the"

Corrected.

Page 15872, line 12 & Fig 1: Fig 1 shows the attachment rate factors, not the ion sink factor. The size dependence of the ion sink factor is not known without the size distribution

We suppose that the referee meant here "charge distribution" instead of "size distribution". The problem is that the ion sink factor (defined by eq. 6) is not known without the charge distribution. In this context the calculation of ion sink was incompletely explained and it included also an error. We had used the Boltzmann equilibrium charge

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distribution which was too inaccurate for the smallest particle sizes. We therefore made new calculations using the Fuchs equilibrium charge distribution, which also takes into account the mobility difference between positive and negative ions. Therefore, the values of ion sink calibration factors changed a little.

Page 15873, line 9: Replace "As the molar mass and as the mobility in temperature $T_0 = 293.15$ K we use..." by "For the molar mass and the mobility at $T_0 = 293.15$ K we use..."

Corrected as suggested.

Is $M = 109$ g/mol $\text{HSO}_4(\text{H}_2\text{O})_+$, a hydrated HSO_4+ ion? Are there no higher hydrates? Please justify this statement.

Page 15873, line 12: "Based on the theoretical definition of ion sink, its proportionality to the diffusion charging of aerosol particles seems to be evident."

This is not evident to the reader if no definition or equation describing diffusion charging is given. The ions do not have the same m/z : 28 and 32 for air ions in the diffusion charger, 109 m/z is the atmosphere. It is obvious that both processes depend on diffusion and surface, but for such a statement the theory of diffusion charging needs to be briefly explained here.

Ion sink in atmosphere is usually employed in studying the balance between small ions and fine aerosol particles and in the calculation of ion production rate (e.g. Hörrak et al., 2008). The ion properties relevant in this context are those of the aged ambient air ions. The ion properties are known to have a distribution of different mobility values, depending on the chemical composition of air, including tracer amounts of various vapours (e.g. Eisele and Tanner, 1990). Like Hörrak et al., we approximate the ion distribution by characteristic ion properties. Following Adachi (1989) and Vohra (1969) we have chosen the mean mobility for positive ions to be $1.40 \text{ cm}^2/\text{Vs}$, and mass 109 amu. It is apparent that this is only one fixed value, and does not account for varia-

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tions e.g. in the humidity or other tracer vapour compounds, but it does contain the assumption that the ions are already reacted ions and not primary ions such as N_2^+ or O_2^+ . Including a temperature variation according to the Langevin rule (Eq. 8) in the estimation is consistent, though. We have modified the text after the Eq. 8 to explain this in the article.

Referring to the last point of the referee, it is indeed not evident, the sentence was badly formulated. However, we feel this is somewhat beside the point: it can be experimentally verified whether the instruments measures ion sink or not. We change the statement to: "Based on the idea and the definition of ion sink, it can be expected that diffusion charging process, where ions produced by corona discharge attach on aerosol particles, is somehow proportional to ion sink."

Page 15873, line 14: "Although"? Better "also" or simply "Using the so-called active. . ."

Corrected.

Page 15873, line 16: ". . . this quantity is correlated. . ."

Original expression "this quantity correlated" was changed to "this quantity correlates".

Page 15874, line 9: "The manufacturer. . ."

Corrected.

Page 15874, line 14: I suggest: "If the intention is to determine a quantity $A(d)$, the instrument is said. . ."

Corrected as suggested.

Page 15874, line 21: ". . . can be seen in Fig. 2, . . ."

Corrected as suggested.

Page 15875, line 6: "All the well-known modes of atmospheric aerosols are detected." Such a sentence is not good. Fig 3 does not support this general statement (no coarse

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mode, x-axis only $< 1 \mu\text{m}$). I suggest to skip this sentence.

The sentence removed.

Page 15875, line 16: "The ELPI. . ."

Corrected.

Page 15877: The different currents calculated/measured deserve more discussion. Would this be simply a result of an underestimation of one of the ELPI parameters (e.g., the number sensitivity function as given by DEKATI) or are there other measurement uncertainties that might be a function of time or of ambient conditions?

The difference between measured and calculated current is the same as between the calibration factors. This difference and the reasons for that are discussed about in the section "Summary and conclusions". The laboratory measurements made after these referee comments support the ELPI sensitivity function and its usage.

Figures

Figure 2: It is not reasonable to plot K_{PNeQ} since this corresponds to $K(d) = S_n(d)/S_n(d) = 1$ (see Equ 9: $P_n(d)eQ = S(d)$)

K_{PneQ} was removed from the figure.

Figure 3: Why is the average size distribution called "artificial"? I understand this is the mean size distribution during this time period?

The caption was changed and the expression "artificial" was removed.

Figure 7: Please add the particle surface area as a function of time

Done.

References

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