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Comment

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

### **Anonymous Referee #1**

Received and published: 1 September 2009

The paper reports on the possibility to measure the condensational sink and the ion sink using the total current as measured by the ELPI. The ELPI uses diffusion charging, and the authors show that the process of diffusion of ions to the particle surface as well as the condensation of vapor on the particles is proportional to the charge that is deposited on the particles. All three processes are a function of diffusion and surface area of the particles. Calculation of the condensational sink and the ion sink is explained in the theoretical part of the paper.

The measurement of the total current by the ELPI is a by-product of standard ELPI operation and thus, the basic idea of the paper is very useful because it introduces a

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simple but straightforward way to measure both sink processes.

In order to calibrate the measurement the authors use the measured size distribution of a DMPS and calculate the particle surface area and calculated the condensation sink and ion sink from these measurements. A linear relationship is found, and calibration factors are derived.

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.

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.

Technical comments

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

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

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specific to H<sub>2</sub>SO<sub>4</sub>

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

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

Page 15873, line 9: Replace "As the molar mass and as the mobility in temperature T<sub>0</sub> = 293.15 K we use.." by "For the molar mass and the mobility at T<sub>0</sub> = 293.15 K we use ..."

Is M = 109 g/mol HSO<sub>4</sub>(H<sub>2</sub>O)<sub>n</sub>, a hydrated HSO<sub>4</sub><sup>+</sup> 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.

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

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

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

Page 15874, line 14: I suggest: "If the intention is to determine a quantity A(d), the instrument is said. . ." Page 15874, line 21: "...can be seen in Fig. 2, . . ."

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 mode, x-axis only < 1 μm). I suggest to skip this sentence.

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

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?

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:  $P_n(d)eQ = S(d)$  )

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

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

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