

Interactive comment on “Characterization of positive air ions in boreal forest air at the Hyytiälä SMEAR station” by U. Hörrak et al.

Anonymous Referee #1

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General comments

This paper reports measurements of positive ions and aerosol in Finland, and more specifically, attempts to close the ion balance between production and loss terms. The measurements carried out are of fundamentally good quality, though some of the techniques applied and conclusions drawn are not adequately justified given the uncertainties. The authors have adopted new and not always clear terminology to describe standard terms in ion-aerosol physics, and this makes the paper harder to understand than it needs to be.

The paper title is not really relevant to its contents and implies a detailed study of the chemical or electrical properties of positive ions, which has not been carried out

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here. A new title, perhaps including the words “ion balance” or “ion concentration” and mentioning aerosol would make the topic of the paper much clearer. The paper is also rather long and some aspects could be shortened and clarified, see specific comments below.

Specific comments

1. Nomenclature.

a. Ion naming. In my opinion the first author is the world expert in the classification of air ions by their electrical mobility. He has written several papers and a landmark thesis in which detailed classification of air ions by their mobility categories has been carried out. However, in the paper, atmospheric charged particles are referred to as cluster ions, small/intermediate/large ions, charged nanometer particles, aerosol ions, charged ultrafine aerosols, charged nanometer particles etc. Horrak has previously defined all these types as small, intermediate and large ions, but perhaps this broader collaboration of authors has led to the use of less rigorously defined categories. Returning to the Horrak terminology throughout, with just one name for each type of ion, would make the paper much easier to follow.

b. Charged aerosol nomenclature. The use of the term “ion sink” is only defined relatively late (p9475) as an attachment rate. In the abstract it is referred to ambiguously as a “loss”. Only after equation (3) on p 9473 it is possible for the reader to work out that it actually has units of rate. The authors appear to be unaware of the basic aerosol literature which already defines and discusses attachment rates at length. It is confusing for the reader to be presented with an alternative, poorly defined term when a perfectly good one already exists. Similarly the use of the word “overcharging” appears to be another re-definition. Though it is not defined in the text, it seems to refer to a non-equilibrium charge distribution; again, this is use of a confusing new term in place of an existing definition.

2. In Section 4.2, it is suggested that fog reduces the conductivity of atmospheric air

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by decreasing the ion concentration. Attachment of ions to fog droplets will occur, but a change in ion mobility could also modulate air conductivity. I am surprised this is not mentioned, since the Estonian group are known for their ion mobility instrumentation, and Horrak himself has discussed this effect in his own thesis (pp82-83). Perhaps the authors are aware of other work on this topic that they could refer to in order to exclude mobility effects.

3. The authors determine the ionisation rate q by closing the ion balance equation, i.e. by calculating ion loss due to self-recombination and attachment (nucleation is, quite reasonably, excluded on the basis of their data). The values of q calculated are lower than is usually measured. This discrepancy is then used as a basis on which to infer there must be an additional loss term. This approach requires q , and indeed all the other parameters of the balance equation, to be very well known in order to accurately estimate the residual and its error range. Errors in q are not stated, but on page 9485 the errors in air ion concentration are quoted as $\sim 10\%$. The ion uncertainty alone in eq (1) therefore implies a fractional error in q of $\sim 30\%$, which could account for the “pine needle effect” and hygroscopic aerosol growth that are invoked by the authors to increase q . To justify the introduction of a different mechanism it must be clearly shown that the discrepancy in the ion balance is greater than the possible errors in q , which need to be discussed as follows:

a. Errors in N (aerosol number concentration) measurements

b. Errors in beta: discussion of the attachment coefficient formulation chosen and its errors over other approaches (e.g. see Gunn, 1955; Fuchs, 1963, Marlow and Brock, 1975; Clement and Harrison, 1992).

c. Errors in alpha: The self-recombination coefficient α is also assumed to be constant throughout the paper, which is probably inappropriate for a detailed closure of the ion balance. An expression for α should be specified and the errors estimated.

4. The paper is rather long and this is partly because well-known facts are repeated, for

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instance demonstrating that the atmosphere always contains air ions, and explaining the recombination limit in clean air, are not necessary and referencing e.g. review papers would suffice. The discussion of precipitation (p9481) is qualitative and should be removed. Also, figures 3a, 3b and 4 do not all seem to be needed.

Technical corrections

Equation 4 uses radius whereas Figure 1 shows diameter.

Epsilon 0 is the permittivity of free space, not the electric constant

P9474 line 17 define “diameter concentration”

Figure 9 - it is impossible to resolve the shape of individual points

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9465, 2007.

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