

***Interactive comment on* “Factors of air ion balance in a coniferous forest according to measurements in Hyytiälä, Finland” by H. Tammet et al.**

H. Tammet et al.

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We would like to thank the editor and the referee for their interest in the article and for their many helpful suggestions. The suggestions are considered in a revised manuscript, which is prepared for final publication in ACP. The critical comments and responses are listed below.

Referee: A major weakness of the paper is the very short time interval of measurements. Only one night of measurement of 16.5 hours is presented. Out of these 16.5 hours 3.5 hours are classified as "disturbed" and the data is not discussed. Why were the measurements not repeated or continued? It would add a lot of credibility if more data were collected.

Authors: A longer measurement campaign would be very useful for analysis of the

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time variations of the ionization rate. Unfortunately, the two instruments were suitably available only for a short time. Thus the experimental part of the research should not be considered as a study of variations of the ionization rate in the forest but as an example showing the performance of the new mathematical model. An additional aim of the measurements was to show that the ionization rate varies considerably with height. As it is, the measurements were enough to accomplish these two tasks. The investigation of the height variation of air ionization is complicated and we hope that our limited results point out the necessity of special studies on the vertical profiles.

Referee: It cannot be judged in how far instrumental problems may have influenced also the other time periods. A measurement period of more than a week would usually be expected as a minimum requirement for such ground based measurements. This is especially important because the BSMA is not a widely used, well described and characterized instrument. It would be interesting to see in how far the measurements vary with time and in how far the presented measurements are typical. Potential instrumental problems could also have been identified in a longer or repeated measurement.

Authors: Unfortunately, there was no published analysis of the performance of the BSMA when the manuscript was written. Now a special paper about the BSMA is available and the corresponding reference (Tammet, 2006) has been added into the revised paper. The long-time measurements by means of single BSMA are described and analyzed in another paper (Hirsikko et al., 2005), and the reference is included in the revised paper.

Referee: It should also be discussed in how far not only the vertical difference of 12 m is responsible for the different results at the two sites, but also the horizontal difference of 50 m may explain at least part of the observed differences (local inhomogeneities of radon sources and beta radiation from ground etc.). Test measurements should also be performed at the tower at 2 m height.

Authors: The variation of the ionization rate at ground level was actually measured dur-

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ing the experiments, but not explained in the initial manuscript. We found a difference of about 5% in the cluster ion concentrations when the horizontal distance was about 150 m. The corresponding explanation has been added into the revised manuscript.

Referee: As the authors state at the very end of the paper, "the presented results are based on short-term measurements and are not verified with simultaneous measurements using different methods. Thus the conclusions have a provisional character..." Therefore the most interesting conclusions remain speculative.

Authors: We thank the referee for pointing out essential problems that require attention in an extended experimental study of air ionization in a forest. We will consider these recommendations in future research. The measurements described in the present paper have only a provisional character.

Referee: A comprehensive description and characterization of the BSMA should be given because the cited reference (Tammet, 2004) is not easily accessible.

Authors: The new publication referred in the revised manuscript (Tammet, 2006) includes a detailed description of BSMA. This paper is easily accessible via ScienceDirect.

Referee: The choice of the rough estimate for the needle density of 100 m⁻² (p3150, l 14) for both sites seems arbitrary. In how far does this choice influence the derived results? In how far do the conclusions, e.g. regarding the differences in ionization rate with height depend on this choice? In how far would an assumption of e.g. 180 m⁻² at 14 m height and of 50 m⁻² at 2 m height change the essential conclusions?

Authors: The data about the average density of needles originated from the referred papers by Rannik et al. As such, there is no account of the local characteristics of the forest close to the measuring points and the values must be considered as a rough estimate. The effect of the error in the estimate of the needle density is described in Table 3 in the present paper. According to this table the relative errors are suppressed

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about 5 times when transferred from the estimate of the needle density to the estimate of the ionization rate. Thus we have been satisfied with the rough estimates of the needle density and believe that the errors in the forest parameters cannot change the conclusions of the paper.

Referee: The presentation quality needs to be improved. Numerous corrections are necessary especially concerning the use of articles. Various sentences need rewording. Please consider to consult a native speaker. A list of sentences and phrases to be corrected or reworded is given below.

Authors: The revised manuscript is thoroughly edited and corrected.

Referee: In general the paper is suitable for publication in ACP but it suffers from the limited amount of measurement data. The credibility of the statements and the usefulness of the model approach would be increased greatly if longer measurement periods were presented and discussed.

Authors: We will consider the recommendation in future research.

Referee: List of technical corrections: ...

Authors: All specific suggestions by referee are accepted and corrections are included into the revised paper by thankful authors.

Referee: The whole abstract should be revised to improve English language style and readability.

Authors: The abstract is fully rewritten in the revised paper.

Editor: 1. Abstract, line 9. The statement that "it solved the controversy of different estimates in the earlier study" is vague. It would be much more informative to state specifically what caused the incorrect estimates in the earlier study. For example, point out that neglect of the ion loss onto the canopy and use of incorrect mobility help to explain the low ionization rates derived in an earlier study.

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Authors: The abstract is reworded considering the comment.

Editor: 2. Abstract, line 13. It would help to give the size limits of "cluster ions" (e.g. $< x$ nm) and "aerosol particles" (e.g. $> x$ nm) when the sink apportionment is discussed.

Authors: The size limit of 1.6 nm is indicated in the revised abstract.

Editor: 3. p. 3138, line 1. Errors in the ionization rates from Laakso et al. are needed to show that the difference between measured and modeled rates is real.

Authors: Additional explanation is included into the revised manuscript. Unfortunately, the paper by Laakso et al. does not consist of error estimates for the values of 4.5 and 2.6 $\text{cm}^{-3} \text{s}^{-1}$.

Editor: 4. p. 3144, line 8. The statement that the needles are the main absorbers of ions and the trunks and branches are not important should be substantiated.

Authors: The text is reworded and a citation (Chen et al., 1997) is added.

Editor: 5. p. 3151, line 11. It is stated that the particle lifetime is long enough to assume that the particles are homogeneously distributed below 14 m. The size distribution is only measured at 2 m, and this is a very important assumption that is used in the derivation of ionization rates. The derived ionization rates are quite sensitive to the aerosol surface area, since loss to aerosol is the dominant ion sink. What are the expected uncertainties in this assumption? Are there previous measurements of the variation of the size distribution with height that support this assumption? If so they should be referenced and discussed.

Authors: Unfortunately, we do not know about the height variation of the size distribution in the forest. The residence time of air in the forest is measured in minutes. The aerosol sink of air ions is caused by the accumulation mode particles, which have a lifetime measured in days. Thus the difference in the particle concentration is expected to be below the uncertainty of measurement. This consideration is included in the revised manuscript.

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Editor: 6. p. 3156, line 5. The authors attribute special significance to the derived vertical gradient in ionization. This seems somewhat overstated considering that the measurements are sparse. How much horizontal variation is expected in the ionization rate at ground level?

Authors: The variation of the ionization rate at ground level was measured during the experiments, but not explained in the initial manuscript. We found a difference of about 5% in the cluster ion concentrations when the horizontal distance was about 150 m. The corresponding explanation has been added into the revised manuscript.

Editor: 7. p. 3158, line 21. "8 V m-2" should read "8 V m-1".

Authors: The revised text is corrected.

Editor: 8. The double negatives on p. 3158 line 27 and p. 3148, line 15 should be reworded to improve clarity. Suggested changes are: replace "cannot be expected to be very low" with "may be significant" and replace "does not contradict" with "is consistent with".

Authors: The revised text is corrected according to the suggestion.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3135, 2006.

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