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Atmospheric Chemistry and Physics Discussions

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

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This manuscript presents a model of air ion dynamics that accounts for ion deposition to the forest canopy. One day of measurements of the aerosol size distribution and ion mobility spectra are analyzed with the model to derive the ionization rate at two levels in the canopy and the apportionment of the ion sinks. The results suggest that the ionization rate decreases with height and pine needles represent a significant loss for the ions. The results appear to reconcile some discrepancies in earlier related work. I agree with reviewer #1's thoughtful analysis. As pointed out by reviewer #1, the observational data is sparse and hence the conclusions are "provisional", to use the language of the authors. At times the English is difficult to follow. This work is a valuable contribution to improving the understanding of air ion dynamics in the boundary layer, and is worthy of publication, once the following comments have been addressed.

Specific comments:

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.

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.

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.

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.

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.

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?

7. p. 3158, line 21. “8 V m-2” should read “8 V m-1”.

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”.

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

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Interactive Comment