Atmos. Chem. Phys. Discuss., 12, C1729–C1731, 2012
www.atmos-chem-phys-discuss.net/12/C1729/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of atmosphere-biosphere exchange of total and speciated peroxynitrates: nitrogen fluxes and biogenic sources of peroxynitrates" *by* K.-E. Min et al.

Anonymous Referee #2

Received and published: 22 April 2012

I found this paper very interesting, giving a new piece of information in the controversial role of the vegetation in the NOx deposition. The paper reports quite rare concomitant measurements of vertical flux and concentration gradient within the canopy of total_PNs and individual PNs (APNs = PAN+PPN+MPAN). The important finding of the paper is that the downward flux of total PNs is slower compared to that of APN, which implies a production of total_PNs inside the canopy. A consequence of this result is that vegetation is not a simple sink of NOx through deposition but the total_PNs production can contribute to the removal of NOx.

The paper is well written and the subject is fully appropriate for publication in ACP. I recommend publication after the authors have addressed the comments below.

My main criticism regards the fact that even if the biggest differences between total_PNs and APN (denoted in the paper XPN) arise mainly during nighttime, the Authors did not point out this result in terms of analysis developed to explain the reasons for the observed XPN. For example: 1) page 6216 (lines 24-25) to derive the XPN flux is used the average daytime concentration of NO2, 2) page 6217 (lines 5-6) the derived HO2 is compared with the mean daytime HO2. These analysis need more description considering that during daytime XPN is much lower than during nighttime.

Why the reactions of sesquiterpenes with NO3 are not considered to explain the observed XPN during nighttime?

The intercomparison between total_PNs and APNs measurements (Page 6211 and Fig. 2) is limited to daytime observations. Since the differences between total_PNs and APNs concentrations arise mainly during nighttime, it would be worth seeing the intercomparison between the TD_LIF and TD_CIMS for nighttime observations as well. A similar comment for the altitude chosen to make the instruments intercomparison: since XPN is different for each height, why did the authors decide to intercompare the two systems only using observations at 18 m. A description of the agreement between the two instruments at different heights would be worth.

Minor comments:

Page 6214, lines 17-22: Are there observations and/or reference that support the hypothesis that the contribution of N2O5 to the XPN is negligible in the observational site?

Page 6218, lines 16-20: To reconcile the results of this work with those of a previous campaign (Farmer et al., 2006) one of the hypothesis is that the biogenic source of PN precursors at the BEARPEX site have decreased over the last decade. Are there any

evidences that this Authors' speculation is plausible?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 6205, 2012.