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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Measurement of atmospheric nitrous acid at Blodgett Forest during BEARPEX2007” *by* X. Ren et al.

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Comments to the paper by Ren et al., 2010.

In the paper by Ren et al. very interesting data on the contribution of HONO to the HOx production of the rural forested atmosphere is described. In contrast to all other studies in similar environment, the contribution was found to be quite low and some explanation is presented to explain the differences. In addition, a promising intercomparison of the aqueous sampling HPLC technique and the CIMS technique for the detection of HONO is presented.

I have some minor comments to the manuscript:

1) The acronym LOPAP should not be used here, since the used instrument is different to the LOPAP which we have developed in our group: a) sampling at pH = 0 by fast chemical reaction, whereas by solubility at pH=7 in the HPLC instrument, b) use of an external sampling unit in our LOPAP, whereas inlet lines are used here c) on-line correction of interferences in our LOPAP by the use of two channel in series, only one channel used here. In addition, there is a registered trade mark on the name LOPAP by the company which fabricates this instrument.

2) page 7387, line 3: The reference by Langridge is on NO2 reactions on self cleaning window glass, which may be too special here (not too much of these surfaces in the atmosphere yet...). May be add a more general paper by Ndour et al. on this topic (dust). In addition, since NO2 has much higher reactive uptake coefficients on organics (NO2+org = HONO) compared to bare surfaces (NO2+H2O=HONO+HNO3) a references to the first reaction could be also added (studies by Markus Ammann’s group).

3) page 7387, line 8: In Bröske et al., we explicitly excluded HONO formation on secondary organic particles, at least in the dark.

4) page 7388, section 2.1: How long was the inlet line for both HONO instruments? Sampled from 20 m altitude? HONO losses in a very long Teflon line, on which alkaline particles (see text) have been precipitated, may also explain the low HONO levels of both instruments. Any line tests done?

5) page7389, line 10: The references to Heland et al. and Kleffmann et al. do not fit here (different instrument).

6) page 7391, R3: Is the HFNO2- stable? Normally, an elimination of the very stable HF should be favoured. This should be temperature dependent. May be the strong periodic noise in Fig. 7 is caused by regular temperature variation of the instrument?

7) page 7396, interference tests: Was pure SO2 tested (there should be no interference...) or SO2+NO2 (known interference)?

8) page 7399, line 14: In Beine et al. no HONO formation was observed in Antarctica also under low snow pH, from which they question pure nitrate/HNO3 photolysis but postulated reactions similar to George et al., or Stemmler et al. on snow, which were recently confirmed in the lab.

9) page 7400, line 3: The study of Rohrer et al. is on a different reaction: a) artificial HONO formation in a Teflon chamber b) the photochemical production of HONO (=daytime chemistry) is studied. However, in the present study differences appear during night.

10) page 7400, differences cold/hot days: Was the wind speed (turbulence) different between the cold and hot days? Than the differences could be explained simply by differences in the BLH and the night-time formation of HONO on ground surfaces.

11) Intercomparison: There is a very strong periodic noise on the CIMS instrument (see Fig. 7) and the amplitude of the noise is almost similar to the HONO level. Any explanation? Considering this noise the agreement is excellent.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7383, 2010.