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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of HONO budgets for two measurement heights at a field station within the boreal forest (SMEAR II – HUMPPA-COPEC 2010)" *by* R. Oswald et al.

R. Oswald et al.

robert.oswald@mpic.de

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General comments

"R. Oswald et al., 2014 has reported a background HONO measurement at both 1 m above ground surface and 2m above forest canopy. HONO budget features low contribution of other processes except for photolysis of HONO and unknown sources in background HONO chemistry. The gradient and diurnal profile of HONO may infer something we don't know yet. Overall, the introduction is in detail and accurate. The dataset is valuable and the analysis is reasonable. Thus I recommend publication in

ACP although several weakness that will be discussed in specific comments."

We thank referee #1 for the positive evaluation of our paper and his valuable comments.
Specific Comments

"1. In line 26 page 7828 and in line 25 in page 7837, the author mentioned drying ground surface would be a source of HONO and the change of temperature of soil surface in the morning. It was inferred from early morning to noontime with the temperature increasing, drying soil surface would be a HONO source. In fact, not only soil surface, leaf surface may also be a source due to drying of dew in the morning, see He Y et al., 2006. Both higher NO concentration in the early morning in figure 1 and smaller ratio of Lphot/Punknown in the morning than the noontime, see line 4 page 7840 and figure 5 indicate HONO and NO emission in the morning. Intensively increasing of HONO concentration in figure 1 after "the short and strong rain (line 15 page 7833)" before sun rise and when NOx concentration were still low also infers a emission source. Soil emission experiment reported in this manuscript may use disturbed soil and hence cannot represent the reactivity of the nitrous acid rich soil surface due to deposition in the night. Failure to account for this term potentially jeopardizes the reliability of correlation analysis in Figure 6, especially for the data point from the morning."

The referee raises an interesting point: There are potentially other processes of HONO reemission that follow the temperature increase in the mooring than just from equilibrium between soil nitrite and HONO or microbiological formation in soil. We added the two processes of dew evaporation (He et al., 2006) and reemission of deposited HONO (VandenBoer et al., 2013) to the text. The referee mentioned reasonable arguments for a NOx independent HONO source. Regarding the correlation in Fig. 6 there seems to be no systematic deviation from the correlation for low J(NO2) values = morning and evening. Furthermore, as low J(NO2) values represent morning and evening the values should split in these two regimes, which is not evident from Fig. 6.

"2. In line 27 and 28 page 7835, the author cited Wong et al., 2013:" the contribution of vertical transport to surface loss of HONO was estimated to be about 50 to 60%", yet no mention of how to calculate this term in equation (8) and no such term in Fig.5."

Wong et al. (2013) used a modelling approach to infer these values. From our measurements we were not able to perform a proper estimate of this contribution. Furthermore, none of the budget studies based on field measurements published so far has taken this term into account. A detailed discussion of the possible contribution of the vertical transport without further investigation and measurements is very speculative. Therefore, we prefer not to include more details about this topic in the present manuscript.

"3. In line 25 page 7840, the author mentioned:" the wild fire pollution plumes transported from Russia". The author should point out which data point came from this particular event in figure 1 and figure 8, also what is the intention and implication to involve the data point in HONO chemistry analysis in a forest. I would also suggest to point out other special events if any in figure 1 and also in result section."

We added the classifications of "normal boreal", "stressed boreal" and "transported pollution (Russian wildfire) according to Nölscher et al. (2012) to Figs. 1 and 8.

"4. In line 7 page 7839, Zhou et al., 2003, 2011 didn't intend to recommend an enhancement factor of 43. Thus it is improper to rule out absorbed HNO3 as a HONO precursor with the data the author presented."

We did not rule out HNO3 photolysis as a source of HONO (see also p 7841, L19 ff.). Nevertheless, such a strong enhancement (here a required factor of about 400) is very unlikely as it would substantially shift the NOx cycling by reducing the lifetime of deposited HNO3 to 1/400 of its lifetime in the gas phase. For a better understanding, we modified line 7 on page 7839.

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