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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous HONO measurements in and above a forest canopy: influence of turbulent exchange on mixing ratio differences" *by* M. Sörgel et al.

Anonymous Referee #1

Received and published: 27 October 2010

1) Does the paper address relevant scientific questions within the scope of ACP? –Yes.

2) Does the paper present novel concepts, ideas, tools, or data? –Yes.

3) Are substantial conclusions reached? –Yes.

4) Are the scientific methods and assumptions valid and clearly outlined? –Yes.

5) Are the results sufficient to support the interpretations and conclusions? –Yes.

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? –Yes, but see the comments below.

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? –Yes.

8) Does the title clearly reflect the contents of the paper? –Yes.

9) Does the abstract provide a concise and complete summary? –Yes.

10) Is the overall presentation well structured and clear? –Yes.

11) Is the language fluent and precise? –Yes.

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? –Yes.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? –Yes, but see special comments below.

14) Are the number and quality of references appropriate? –Yes.

15) Is the amount and quality of supplementary material appropriate? –N/A.

General comments

This paper presents simultaneous measurements of HONO at two different heights within and above a forest canopy. The authors tried to combine the micrometeorological measurements to explain the diurnal variations of the HONO concentrations measured at the two heights, which seem strongly influenced by turbulent vertical transport and relative humidity. In the morning and during the mid-day, strong vertical exchange caused almost the same HONO concentrations at the two heights. When the turbulence was not strong (e.g., in the evening and at night), the difference of HONO concentrations became obvious and were caused by the different source/sink processes at the two heights. Different from a few other studies, the authors conclude that there is not a simple relationship between RH and HONO in their study at this site. In general the paper is well written and reports important results. I recommend it be published in ACP after revision and ask the authors to consider the following special comments in

their revision.

Special Comments

1. P.21111, L.22, should "(NO2(ads)+H(ads)->HONO(ads))" be "(NO2(ads)+H2O(ads)–>HONO(ads))"? Also in two places on the same page (L.19-20 and L. 26), I would suggest changing the phrase "not of atmospheric relevance" to "not of atmospheric importance" or something like that. These processes are certainly relevant to atmospheric HONO formation, but may not very important in the real atmosphere.

2. P. 21114-21115, it would be great to include a little more information regarding the site (major nearby cities and roads which might influence the measurements at the site) and the chemical and meteorological conditions during this study. Perhaps including a figure with time series of chemical/meteorological parameters (e.g., NO, NO2, O3, HONO, temperature, RH, wind speed and direction, J(NO2), boundary layer height, etc.) would be good.

3. P.21117-21118, Section 3.1, the large deviation in the intercomparison of two LOPAP instruments during the wet period is a little worrisome, despite the good agreement during the dry period. The authors gave a few possible reasons, but in my opinion none of them is convincing. What were HONO levels during the wet periods? It is better to include time series of both HONO measurements in Figure 1 (maybe add an upper panel) to see the absolute differences in HONO level.

4. Figure 1, x axis: "01-Sep", "02-Sep", and "03-Sep" should be "01-Oct", "02-Oct", and "03-Oct". Also, before the noon of Sep. 29, there was significant difference between two instruments which seems related to low visibility. After the evening of Oct. 2, the visibility decreased again. However, the relative difference was not very large compared to the wet periods before. Any explanations for this?

5. Figure 2, add NOx time series to show relatively constant NOx level.

6. In Figure 4, I am surprised to see the poor correlation between NO2 and HONO. Have the authors tried to look at the correlation during the dry periods only? I wonder if the data points from the wet periods are causing the scatter.

7. In Figure 5, briefly explain the meaning of C, Cs, Ds, Dc and Wa so that the figure is self-explained without going back to the text. Also on P.21124, more description is needed regarding how to get the red boxes and what measurements are included in the red boxes in Fig. 5. I only found one sentence: "As an indicator for the effectiveness of vertical mixing, the detection of coherent structures was used," which is not very clear to me and not sufficient.

8. In Figure 6, the relatively large HONO difference at two heights between $\sim$21:00 and midnight is kind of surprising to me. More discussion is needed in the text (P.21115). The authors attribute this to the "advection of HONO-enriched air masses above the forest canopy" or the release of adsorbed HONO from the wet canopy. These are not sound and need more evidence to explain the observations. Also, any explanation for the sudden increase (from $\sim$5-10 to $\sim$15-25) in the HONO lifetime ratio (red bars) at around 14:00?

9. P.21126, L.23-24, from Fig. 7, it seems to me that both HONO and RH (from the counter plot) follow similar trends at the two heights and I would expect similar correlation between RH and HONO concentration with in the canopy. Please verify the significant difference in the correlation at the two heights.

10. In Figure 7, a sudden increase in HONO concentration measured above canopy around 21:00 needs more explanation. The authors state: "This event is considered to be dominated by an air mass change and not by local HONO production or release". Any evident from wind direction switch and/or wind speed change?

11. In Reference, Sander, 1999 is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21109, 2010.