

Interactive comment on “Pseudo steady states of HONO measured in the nocturnal marine boundary layer: a conceptual model for HONO formation on aqueous surfaces” by P. Wojtal et al.

Anonymous Referee #2

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- 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, but see the comments below.
- 3) Are substantial conclusions reached? –Yes.
- 4) Are the scientific methods and assumptions valid and clearly outlined? –Yes, but see comments below.
- 5) Are the results sufficient to support the interpretations and conclusions? –Yes.
- 6) Is the description of experiments and calculations sufficiently complete and precise

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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, but see the comments below.
- 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, but see the comments below.
- 15) Is the amount and quality of supplementary material appropriate? –N/A.

General comments

This paper presents nighttime HONO measurements in a polluted marine boundary layer. The authors presented a conceptual model to further analyze the observed data to improve a better understand of HONO formation. My only concern is that the processes regarding HONO formation in the conceptual model is rather simple and as the authors have pointed out in Conclusions, more analysis and relevant measurements are needed in order to better understand the HONO formation in the atmosphere. In general I think the paper is well written and reports some reasonably important results. I would recommend it be published in ACP after revision and ask the authors to

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consider the following special comments in their revision.

Special Comments

1. In Introduction, in the literature review of HONO formation: NO₂ reduction on organic surface and the photolysis of surface-deposited HNO₃/nitrate (Zhou et al.) should be also included.
2. Detection limits (3 sigma) were 0.30 ppb for HONO, 0.45 ppb for SO₂, and 0.20 ppb for NO₂, determined by repetitive determination of a low concentration sample. HONO detection limit is not very significantly different from its nighttime levels (~0.5 ppbv), which causes a large uncertainty in the measurements and may affect some of the conclusions
3. Were there any measurements for marine boundary layer heights? This important information needs to be discussed somewhere if there was no direct measurement.
4. Section 3.6, 1st paragraph, add Yi et al. (Importance of dew in controlling the air-surface exchange of HONO in rural forested environments, *Geophys. Res. Lett.*, 33, L02813, 2006) about the dew evaporation to release HONO and other nitrogen species.
5. Fig.2, there was large variability in the observed HONO mixing ratios from day to day. It would be great to explain a little bit for the reasons.
6. Fig. 7 is relatively scatter, maybe should plot HONO/NO₂ vs. RH

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 10, 25153, 2010.