

## ***Interactive comment on “Direct N<sub>2</sub>O<sub>5</sub> reactivity measurements at a polluted coastal site” by T. P. Riedel et al.***

**T. P. Riedel et al.**

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Response to Anonymous Referee #3

We thank Referee #3 for reading the manuscript and providing helpful comments; below we provide responses to each comment individually.

General Comment by Referee #3: This is an interesting short observational paper describing N<sub>2</sub>O<sub>5</sub> reactivity in a polluted coastal area. The observations show a large range in the value of the heterogeneous uptake coefficient for N<sub>2</sub>O<sub>5</sub> ( $\gamma$ ) onto the ambient submicron aerosol particles, stressing the importance of better understanding the dependence of  $\gamma$  on composition and atmospheric conditions. The observations back up the laboratory observed "nitrate effect", which is a poisoning of the

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surface reactivity when large amounts of nitrate are present in the particles. At first thought, the concept that a coastal study would prove the nitrate effect exists in field data is surprising because one would have thought that chloride in the particles would provide a rapid sink for the hydrated intermediate. Thus, the finding that neglect of the chloride assists in bringing parameterizations for  $\gamma$  closer to the observations is surprising. However, it may be a reasonable explanation that the aerosol particles are not internally mixed and that the chloride is not in the proper sized particles to cause the full effect predicted by the parameterization. The authors then go on to discuss that the observed  $\gamma$  is still smaller than expected and speculate upon the possible role of organic matter in poisoning the surface. One should note that both of these effects (turning off the nitryl chloride channel and organic surface poisoning) lower  $\gamma$ . Thus, if one was to indicate that the organic effect were larger, one could possibly still allow the nitryl chloride channel to be active. Therefore, the question of the chloride effect and organic effects is not really disentangled by this analysis. As the authors indicate, further study is clearly indicated to resolve these important issues. In any case, the direct observation of the nitrate effect in field data is novel and an important finding.

Author responses follow each comment and are denoted with \*\*.

p31913 Line 24: Typo on Finlayson-Pitts et al. Also p31915 Line 14

\*\*These changes have been made.

p31914 line 7: This equation assumes no diffusion to particle limitation, and thus is appropriate for submicron aerosol. This might be mentioned as a part of one of these sentences.

\*\*We have added the following sentence: "Eq. 1 neglects any limitations resulting from gas-phase diffusion to the particle surfaces which are likely negligible under our measurement conditions ( $\gamma(\text{N}_2\text{O}_5) < 0.05$  and  $r_p < 2 \mu\text{m}$ )."

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p31918 line 21: Missing period before "Neglecting..."

\*\*The ACPD manuscript has dropped punctuation in some places. This is likely an oversight on our part after the typesetting. We will make sure to address these issues with the production staff.

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31911, 2011.

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