

Interactive comment on “Direct N₂O₅ reactivity measurements at a polluted coastal site” by T. P. Riedel et al.

Anonymous Referee #2

Received and published: 28 January 2012

General Comments:

This paper presents new measurements of N₂O₅ reactivity and uptake coefficients at a coastal site in southern California. These are the first measurements using this apparatus, previously described by the same research group, that incorporate an explicit comparison to a recently derived parameterization of the dependence of N₂O₅ reaction probability on nitrate and chloride content of aerosol. Thus, the study serves as an important first-test for this parameterization, especially since it was done at a coastal, presumably chloride-rich, site. The writing is clear and the methodology is sound. The conclusions are well-reasoned and will add to the growing understanding of this important nighttime reaction.

I recommend publication subject to consideration of the following, minor comments. In

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particular, the authors may want to clarify the degree to which the nitrate and organic aerosol content correlate, since their study shows clear evidence for a nitrate effect, but since it also requires additional suppression of the reaction probability to bring model and measurement into agreement.

Specific Comments:

Page 31913, line 9: A 1999 reference may not be completely appropriate for projections of future NO_x trends from 2012

Page 31915, equation (2): Equation not clear. What quantities are in the ratio? This should be expressed as rate coefficients rather than references to the reactions themselves, I believe.

Page 31916, line 13: Suggest replacing “elusive” with “not clear from the available data.”

Page 31918, line 10-12: Up to 50% error in the gamma values based on the hygroscopic growth factors of the particles. What was the method used to calculate the hygroscopic growth? The AIM model referred to later?

Page 31919, top: Was there any primary organic (POA) observed? Too small to be included? Also, there is no real use made of the organic categories later in the paper. Would it be appropriate to sum, rather than differentiate them?

Page 31919, bottom: Is there any observable loss of particulate, and surface area, during transit through the halocarbon wax coated (i.e., non-metallic) flow reactor?

Page 31921, line 11: Wind rose in Figure 1 – the strongest winds were from the SSE and NNW, so transport from San Diego and L.A. is event driven, but the overall wind pattern shows no particular preferred direction - i.e., it appears that there is plenty of light wind data with no particular direction. Fairly typical for nighttime, surface-level winds. Also, the legend is difficult to read – suggest increasing font size.

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Page 31923 – 31924: Discussion of parameterized vs. observed reaction probabilities. Two comments. First, the parameterization does not include organics, which could explain the lower observed reaction probabilities. This point is treated in the discussion that follows, but could be referenced here as well for clarity. Second, the comparison is for averaged quantities – 24 hour averages of data taken at approximately 1 hour resolution (but then filtered, so fewer than 24 points per daily averaged point). Is there any possibility that the separate averaging of quantities used to derive the parameterization differs from the averaged reaction probabilities due to non-linear dependencies? The authors could add a comment on any such effect.

Page 31924, line 23-25: What is the correlation between nitrate and organic? Could a co-variance explain the part of the variability not captured by the variation in nitrate alone? A simple correlation plot might be useful to clarify this point, which the authors do raise in the following paragraphs. Inclusion of the additional plot is at the author's discretion. The authors may also wish to discuss the reliability of the calculated water content from the AIM model. Presumably, a reduction in the calculated water content could bring the observations and the parameterization into agreement. Could the dependencies in figure 5 be used to fit the aerosol water content, for example?

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

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