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Atmospheric
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Interactive comment on “Measurements of N_2O_5, NO_2, and O_3 east of the San Francisco Bay” *by* E. C. Wood et al.

E. C. Wood et al.

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We thank Referee # 2 for the comments.

The quantitative interpretation of the data is the weaker part of the manuscript. In their calculation of the N2O5 pseudo steady state the authors assume that loss processes of NO3 can be ignored. It would help to expand the manuscript in this section to provide a more quantitative description of the uncertainty introduced by this and other assumptions in the pseudo steady state calculation.

We discuss the effect of NO3 losses by adding text to the revision describing the ef-

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fect of 10 ppt of NO, which would lead to an error in the observed N2O5 lifetime of 15%.

In addition, the authors should considered recent publications showing that vertical transport of N2O5 can not be ignored for pseudo steady state calculations of N2O5 (Geyer and Stutz, JGR 2004).

We have added some text to the revision discussing the vertical structure of the atmosphere. As we note in response to referee #1, unlike the situations consider by Geyer and Stutz these measurements are not made in location that is in the midst of the NO source. As we noted in the original text (although without the explicit comparison to the Stutz and Geyer results that we now include) these measurements were made several km (many lifetimes with respect to the NO+NO3 and NO+O3 reactions) from NO sources. Thus we expect that the steady state gradients calculated by Geyer and Stutz are not directly applicable to analysis of these observations.

The authors conclude that N2O5 loss is a more important NOx sink than the reaction of NO2 with OH during the day in winter. While I do not disagree with this general conclusion, I would suggest to balance this statement with the fact that N2O5 was only observed on a few nights during the experiment. On several nights N2O5 appears to be unimportant and the daytime loss of NOx will dominate.

As mentioned in the text, [N2O5] was below the detection limit on foggy nights, which implies that N2O5 hydrolysis was rapid and thus even more important than the nights during which [N2O5] was observed. We have added text to the revision to make this conclusion more explicit.

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In addition, the authors should take the altitude dependence of N2O5 concentrations into account, and consider that the boundary layer is typically higher during the day than at night. The higher daytime boundary layer will increase the significance of the OH + NO2 reaction as a NOx loss process, since it occurs in a much larger volume than the nocturnal N2O5 loss.

We have added text in the discussion of the revision contrasting the NOx losses in summer and winter, noting both that OH changes and that the chemistry of the nocturnal and residual layer is therefore quite different in the two seasons. During the winter, the lifetime of NOx with respect to OH is long enough that NOx persists into the night both in the nocturnal boundary layer and in the residual layer. In both regions our expectation is that the lifetime with respect to losses by N2O5 hydrolysis is short compared to the length of the night. Thus in winter, the differences in boundary layer height between day and night effect the chemistry quite differently than in summer, when the OH concentration is 10 times higher and NOx is removed within a few hours of emission during the day.

Technical comments: I am unclear on the meaning of the unit ppbvv. It would help to briefly introduce this unit in the manuscript.

This was a typo and has been corrected in the revision.

I would suggest using the term “pseudo steady state” instead of “steady state” since a true steady state is rarely achieved in the atmosphere.

We have made the suggested change to the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6645, 2004.

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