

Interactive comment on “Deposition of dinitrogen pentoxide, N₂O₅, to the snowpack at high latitudes” by D. M. Huff et al.

D. M. Huff et al.

wrsimpson@alaska.edu

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We thanks the Anonymous Referee #3 in comment C13104 for the helpful comments on the manuscript and are addressing these in the revised version. Essentially all of the minor comments were taken into consideration in our revised version. In this reply to the reviewer, we will discuss the major comments that were brought up. We are also adding this discussion material to the revised manuscript.

Major Point 1: The reviewer asks about the representativeness of the data because the use of the gradient method required rejection of much of the collected data. We agree with the reviewer that this is an issue and are discussing it further in the revised version of the manuscript. However, this work represents the first work on deposition of N₂O₅ to snowpack, and thus represents a starting point to the observations and discussion

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of the role of the snowpack on NO_x chemistry. One of the major probable impacts of the data selection is that more significantly stable data periods needed to be rejected. It is likely that during those times the vertical mixing is more strongly hindered and thus the vertical extent of the surface deposition is shorter than the periods where we could use the gradient method. Thus, the statements of near-surface measurements being impacted by N₂O₅ deposition would be enhanced during these more stable periods. Additionally, the overall (e.g. column-averaged) chemical removal of N₂O₅ due to surface deposition would probably be diminished due the poor vertical mixing preventing contact of N₂O₅-containing air from contacting the surface. We also note that other non-gradient methods (e.g. eddy covariance) should be used in the future to quantify the deposition of N₂O₅ under a wider variety of conditions.

Major Point 2: The reviewer asks about the relative role of aerodynamic transfer from the atmosphere to "contact" with the snow surface versus chemical limitations on reaction of N₂O₅ on the ice surface. This is an important point that we discuss further in the manuscript. There are measurements of N₂O₅ reactive uptake on ice and impure ice (e.g. Hanson 1997, Hanson and Ravishankara, 1991), which show values of a few percent. From the relatively high value of this reactive uptake, we expect that the chemical process is probably not the major control on the deposition velocity of N₂O₅, but rather that the aerodynamic control is stronger. In addition, we note that our result for the N₂O₅ deposition velocity is similar to that of nitric acid, which presumably attaches to ice readily would be in agreement with both species being limited primarily by aerodynamic transfer.

We thank the reviewer for these helpful comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25329, 2010.

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