Atmos. Chem. Phys. Discuss., 10, C13104–C13106, 2011
www.atmos-chem-phys-discuss.net/10/C13104/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deposition of dinitrogen pentoxide, N_2O_5, to the snowpack at high latitudes" *by* D. M. Huff et al.

Anonymous Referee #2

Received and published: 1 February 2011

This paper presents the first observation based estimates of the N2O5 deposition velocity to snowpacks at high latitudes. N2O5 is well known to be an important nighttime reservoir of NOx that can react on surfaces and thus potentially be a sink for NOx. Surface deposition is thus a likely candidate for N2O5 removal, although its importance to the total NOx budget is difficult to assess given the poor vertical mixing of the nocturnal atmosphere. This issue is especially problematic for high latitudes. Nonetheless, the authors took care in their experimental design, data analysis, and presentation to arrive at a useful data set. I recommend publication in ACP upon their consideration of a few points below.

My general comments concern issues not unknown to the authors, and so I will not belabor them. The gradient method is subject to significant uncertainties, especially

under stable conditions. The authors attempt to filter the data for periods when the required assumptions are valid, but it appears that leads to the rejection of $\sim$75% of the collected data. Obviously, just because the gradient method cannot be used doesn't mean there wasn't deposition. The authors might wish to discuss the overall representativeness of their measurements in this regard.

I am under the impression that the obtained deposition velocity largely represents a limitation by aerodynamic transfer from the measurement height to the surface or molecular diffusion across some laminar layer, and not a limitation due to actual N2O5 loss once in contact with the surface. Is this true, or do measurements of N2O5 reactivity on ice surfaces suggest a limitation in this regard?

Other minor comments

Abstract

First sentence is missing "the": "Dinitrogen pentoxide, N2O5, is an important intermediate in the nighttime oxidation of NOx that can react on surfaces."

Line 4-5, Technically, the flux is derived (not observed) from measurements of the N2O5 gradient.

Line 10 – 11: . . .chemical removal of N2O5 that is located within the first few meters?

Introduction Pg 25331, line 15. I think a concluding statement about these different model predictions for the high latitudes might be relevant. Do they predict the same NOx loss by N2O5 in high latitudes as does Dentner and Crutzen?

Pg 25331, line 16. It is interesting that most observations of N2O5 atmospheric chemistry have been conducted in mid-latitude regions during summer, when it presumably is least important in a relative sense. Perhaps such an introductory statement to this paragraph would be useful.

Pg 25332, line 16. It doesn't seem possible to know that the deposition was due to

heterogeneous hydrolysis and not some other reactive loss process.

Methods Pg 25335, line 17: How often was the inlet position switched? Did it spend 15 minutes at one height and than move rapidly to the lower height for 15 minutes? If so, was there an attempt to interpolate both heights to a uniform time base so as to possibly capture some variations that may have occurred over the 30 minute period? If no such attempt was made, perhaps a statement on typical point-to-point differences between two measurements at the same height and how that variation may affect the derived flux would be useful.

Pg 25337, line 10: Introduce Richardson Number index Ri here

Pg 25338, line 9 – 24: I think this discussion should be greatly reduced and streamlined.

Results Pg 25339, line 27: Does the uncertainty quoted here reflect only the standard deviation of the derived values? What was the total number of determinations that went into the average? It seems like you might be underestimating the confidence in the mean value if the quoted range is just the standard deviation.

Discussion

Pg 25340, line 19, I think this first sentence can be cut

It is not clear from the discussion of Apodaca, et al just how big the discrepancy was in required N2O5 loss rate in that paper, and whether the derived deposition velocity from this work is really capable of closing that gap quantitatively.

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