Atmos. Chem. Phys. Discuss., 9, C2153–C2157, 2009
www.atmos-chem-phys-discuss.net/9/C2153/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

9, C2153–C2157, 2009

Interactive Comment

Interactive comment on "BrO, blizzards, and drivers of polar tropospheric ozone depletion events" *by* A. E. Jones et al.

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Received and published: 24 June 2009

1. The influence of blowing snow on radiation levels has been included in the text. Modified text reads: Radiation is also required for sustaining the chain reaction, and actinic flux is likely to be modified under conditions of blowing snow. Whether enhanced or attenuated will depend upon a number of factors including the depth within the blowing snow layer, the solar zenith angle, the snow density and the wavelength. An analogy is provided by a modelling study [Lee-Taylor and Madronich, 2002] that considered light propagation through snowpack. They calculated that for SZA less than ∼50o, some actinic flux enhancement would occur in the top 10 cms of the snowpack. This occurred as a result of enhancements to diffuse light from forward scattering that outweighed attenuation of direct radiation, such that actinic flux is enhanced. Deeper within the

snowpack, actinic flux was attenuated.

2. Temperature is measured by sensors placed at various intervals on the 32 m mast. On 8th/9th Oct the temperature gradient was zero. We do not feel that a figure to show this can really be justified, but the fact that the temperature gradient was zero is now more explicitly stated in the text:

Air temperature profiles from a 32 m mast show the atmosphere to be un-stratified prior to the event. With the vigorous shear driven mixing near the surface, this lack of stratification was maintained throughout the storm of 8th to 9th October, with zero temperature gradient observed between the mast sensors.

3. Paragraph 2 of the Discussion section has been modified to address for the Reviewers comments. The paragraph now reads: An obvious question at this stage is the capability of the satellite instrument to "see" into blowing snow. i.e. is it possible that it can measure BrO within blowing snow, or is it more likely that it is measuring BrO that is located above the blowing snow layer? Experience from ground-based observations suggest that a good signal can be achieved with long light-paths under blowing snow conditions. However, from satellites, it can be expected that the blowing snow will reflect a proportion of the photons, and the penetration into blowing snow of the remaining photons has not previously been assessed. The analogous snowpack modelling study of Lee-Taylor and Madronich [2002] outlined in Section 1. is also relevant here, as, assuming that BrO is present within the blowing snow, it is reasonable to expect to see at least part of it from the satellite, the reduced light penetration being partly offset by the enhanced light path. More quantitative statements would have to rely on radiative transfer calculations within blowing snow itself which go beyond the scope of this paper. We note, however, that the association between high BrO events and blowing snow is also apparent from earlier ground-based studies (Kreher et al., 1998; Frieβ et al., 2004). We therefore assume that this relationship exists, and use the observations presented in this paper to explore whether the role played by blowing snow is indirect (i.e. via aerosol) or whether there might be an alternative route for bromine

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production, namely that the blowing snow itself might be a direct halogen source, and that the additional step of aerosol production is not essential for the argument.

We also suggest in the Summary and Conclusions, that such modelling, of light propagation through/into blowing snow, would be timely.

4. The reviewer was right that we ought to have included issues of pH into our paper. We have now amended the text in both places as suggested, and raised the point that pH is also an important factor to consider.

5. This equation is, indeed, an extremely simplified b.l. height scheme, but based on two observations. a) The minimal b.l. depth, which occurs over non-sloping terrain, appears to be 10 m. b) scaling arguments show that b.l. depth is roughly proportional to friction velocity, which under minimal stratification, is related to wind speed. There exists no robust b.l. height parameterisation scheme at present, but this makes little difference to essence of the argument, as the bi-modal nature of the "ODE Strength" depends on a (near) linear behaviour with wind speed and an exponential behaviour with blowing snow + ventilation.

To ensure the readership is aware of this, additional text has been added referring to Eq. 1.

Original Text: Neff et al. (2008) suggest that hz is linearly related to surface stress, u* . We propose a simple function which describes the tendency for hz to increase with wind speed, assuming that u* is proportional to the 10-m wind speed, U10.

Additional Text: We note that the shallowest boundary layers observed at Halley (a flat homogenous terrain with similarities to a plane of sea-ice) with sodar or tower are $\sim$ 10 m. These occur under near calm conditions. When 10-m winds are of the order of 10 ms-1, the Halley sodar shows turbulence $\sim$ 60 m. A highly simplified parameterisation of hz would therefore be: (1). We do not suggest this is a suitable scheme for modeling purposes, but the function is sufficient to indicate the essence of the bi-modal nature

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of ozone depletion as a function of wind speed.

6. The data in Mann (2000) show no significant dependancy of the particle size distribution on wind speed. Note that the PSD is normalised and therefore only shows the relative number of particles. This implies that, at a given level, as wind speed increases, the number of small and large particles increase at the same rate. This is entirely plausible. We suggest that, given the wording: Mann (2000) describes NPSD(z, r) the concerned reader is directed to the correct reference.

7. Yes, this ought to have been explicitly stated and properly brought out in the submitted paper. We have now included the following statement in the Introduction: Certainly it is known that Br2 and BrCl can be produced within the snowpack, as demonstrated by field observations in the Arctic (Foster et al., 2001).

8. Actually it's not solely the condensed phase, as the equations also take into account the ground-based snowpack surface. What they don't consider is any interaction between firn ice and interstitial air. We have added a clarification at the point raised by the reviewer: We have thus now derived the equations necessary to describe the two key environmental parameters that influence the degree of contact between gaseous and condensed phases (i.e. the suspended condensed phase and ground-based snowpack surface) and the way in which they vary with wind speed.

9. Figure 12d is Figure 12b / figure 12 a. The dotted line is 12b x 12c / 12 a, as described around line 374 in the discussion. This is clarified in the modified figure caption: Figure 12. a) The variation of boundary layer height with wind speed; b) The variation of snow surface area within blowing snow according to wind speed; c) The enhancement function that accounts for the influence of ventilation according to wind speed. new part: Figure d) is the potential for boundary layer ozone depletion found by dividing the data from figure b by figure a (the dashed line, accounting for ventilation, is b.c/a). See discussion.

10. We have added a sentence in the Discussion: "The observations also open the

possibility of enhanced initiation processes within the snowpack." We were not intending to rule this out with our observations, but merely didn't get into this issue as it's hard to say anything conclusive about it from our observations.

11. We have changed the heading "Conclusions" to "Summary and conclusions" and amended this section in light of the reviewers comments to account for the need both for photochemical modelling of the blowing snow scenario, and also for light propagation into blowing snow to address uncertainties in satellite data under such conditions.

12. We have improved the scale for Figure 5a.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8903, 2009.

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