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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 18 May 2009

This paper presents a very interesting analysis of a blowing snow event and temporally associated ODE along with SCIA BrO data in the Weddell Sea area. The paper hypothesizes that blowing snow events can increase the effective suspended "salty" reactive surface area, leading to ODEs under extreme wind speed conditions, along with those better known to occur under stable low wind conditions. I found this paper to be very well written, thoughtful, and interesting. The section that connects the ECMWF winds and BrO data to conclude in situ propagation during the wind event was quite convincing. The paper adds to the literature information on the elusive question of what environmental conditions are necessary for/trigger ODEs. I think it should be published after attention to some details (essentially editorial/clarifications) outlined below.

1. Line 77 – the paper should also note that sustaining the chain reaction also requires

radiation, which is likely to be reduced under blowing snow conditions. While I think this paper needs to be published, the qualitative analysis presented here will benefit greatly from an explicit photochemical model treatment with appropriately parameterized radiation and suspended surface area data. This might be worth noting in the Conclusions section, which add nothing to the paper as written.

2. Line 183 – it might be helpful to present the actual temperature gradient.

3. Beginning of Discussion – I think the paper should make some comments about what we know regarding the reliability of satellite BrO retrievals under such conditions, with respect to reliable interpretation of surface layer BrO in the presence of blowing snow grains in the surface layer. Some comment on this would seem essential, given that the paper in part connects surface observations of ozone with BrO signals that may well derive from somewhere aloft of the layer with enhanced snow grain surface area. Line 374 effectively states that there is a good probability of a disconnect between what SCIA sees and the surface level ODE.

4. Line 293 – the paper should be written to be consistent with or in recognition of the existing literature, specifically, that it is not only sufficient salinity that is believed to be necessary, but also sufficiently low pH. This comment applies also to line 402. How low is the pH of the salt associated with the blowing snow?

5. State the source/basis of equation I.

6. Is it really true that one can safely assume that N(psd) is invariant with wind speed? Is there a reference?

7. Line 356 – you should state in the paper that it is known that Br2 and BrCl can be produced within the firn (Foster et al., 2001).

8. Line 362 – it is really the suspended condensed phases.

9. Fig. 12d – state explicitly how the lines are calculated; to me it is unclear how this is done, e.g. for the low wind end. 10. Fig 13b – re interpretation of the Br- peak

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in this figure, it should be noted/stated that you don't really know what Br- is at the surface. That seems important given your basic assumption that there is no initiation in the subsurface snowpack.

11. As indicated above, the Conclusions simply summarize the content of the paper, which isn't necessary given how well written the paper is. I suggest you either delete the Conclusions, or insert some concluding thoughts.

12. Figure 5a should have the same scale as for 5b and 5c.

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

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