

## ***Interactive comment on “Halogens and their role in polar boundary-layer ozone depletion” by W. R. Simpson et al.***

**Anonymous Referee #3**

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This is a review article on the topic of the relationship between halogens and ozone depletion in the boundary layer in polar regions. This subject has been of interest to the atmospheric chemistry community for about 20 years, and there remain considerable uncertainties that preclude a quantitative understanding of the phenomenon. Nevertheless, there has been a lot of research done on the subject and it is appropriate for a lengthy review article to be published on its different aspects. I put some general observations below.

I am particularly impressed by the thoroughness of Sections 1 and 2, on the chemistry, lab studies and field observations. But it strikes me as somewhat strange that the section on Models (1.4.2) is so very short. I can understand that the authors may be hesitant to focus on the result of only one specific set of models, but I think that there

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should be an extended discussion of the modeling results obtained so far, especially given the scope of the rest of the review.

There is a lack of emphasis on the details of the chemistry that might occur at the interface of ice that might be different from that which occurs in bulk solution. For example, there is a section on the importance of pH (3.1.2) but the importance of pH might be greatly diminished if the ideas of Finlayson-Pitts and co-workers are correct, i.e. that there is halide oxidation chemistry that occurs in the absence of protons as a reactant.

Wouldn't it make more sense to put section 3.1.7 immediately after the other sections on frost flowers?

I found the section on Meteorology and Boundary Layer Physics to be not on that topic! The first few paragraphs are very general and I recommend they be deleted. Why is there a section of HO<sub>x</sub> and NO<sub>x</sub> fluxes? I know that such chemistry might be going on at the same time as the halide oxidation, but this section seems to far removed from the main topic of the paper. The section of haloorganic fluxes is interesting but not related to the overall title above (i.e. Meteorology and BLP).

Finally, in the regions at which ODEs occur, there are genuine impacts on mercury and ozone. But, when one asks what the importance is beyond these regional effects, the case becomes quite weak, or at least unproven. If there is export of active bromine from these regions, then, yes, there will be effects on lower latitude free troposphere chemistry. But, aside from isolated instances, has this shown to occur to a significant degree? Will there really be effects on DMS in the non-polar marine boundary layer arising from polar processes - I doubt it. Don't get me wrong - I think that this is an important phenomenon for the regional polar environments and, perhaps even more importantly, it is important as a "test case" for similar chemistry that might occur at lower latitudes. But, I thought when reading Section 4 that the "effects" section might be toned down a little bit as the ODEs do remain highly episodic, localized events in

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space and time that may not play a very important role globally.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 4285, 2007.

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