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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Bromine measurements in ozone depleted air over the Arctic Ocean" *by* J. A. Neuman et al.

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We appreciate the helpful discussions and suggestions provided by the reviewers and have modified the manuscript according to their recommendations.

Referee #1 suggested several additional references. We have included the more recent work (Jones et al., [2009] and Mahajan et al., [2009]), and provided more frequent and explicit mention of two recent review papers (Steffen et al., [2008] and Simpson et al., [2007]) that discuss nearly all of the other suggested references.

In response to questions from Referee #1: The first paragraph of section 2.3 and the end of 2.4 has been changed to highlight the sensitivity tests that were performed on all of the instruments. We do not fully understand the small interferences that degrade

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the Cl2 and BrCl measurements from the WP-3D. New discussion in section 2.4 notes that the variability was associated with rapid changes in humidity during aircraft altitude profiles. Likely, the large changes in water mixing ratio caused slight changes in ion chemistry that altered the background levels. The technical comments regarding the errors in referencing refer to the originally submitted manuscript and have already been corrected in this APCD paper.

Referee #2 asks if the boundary layer mixing times were sufficient to allow an even distribution of BrO. We don't know the mixing times, and it could be variable with a dependence on the influence of open leads. However, with a HOBr photolytic lifetime of tens of minutes, and boundary layer mixing times of similar magnitude, we think an even distribution is plausible. The modeling of vertical transport shown in Figure 4 of Lehrer et al. [1994] provides some support for this assumption.

Referee #2 asks if there was a correlation between active bromine and particle surface area. We looked very closely for such a connection, but found none. We gave this considerable attention because we were concerned that the observed Br2 signals were caused by liberation of Br2 when bromide-containing particles were sampled. However, we found no indication for the conversion of particulate matter to gas phase bromine on the inlets. We hesitate to speculate on the importance of the mechanism, as we did not sample near the surface where it might be most important.

Lastly, Referee #2 notes that the last paragraph of section 3.4 needed clarification. We agree that the alternating discussion of a specific flight and campaign averages was confusing. Hence, all of the discussion of the 4/19 flight has been moved to the first paragraph of 3.4, and the last paragraph of 3.4 discusses only campaign averages.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3827, 2010.