

1. It seems that most of my major criticisms on the paper have been rebutted by the authors. I agree to the points they have raised in that the nature of condensed-phase chemistry in and on the natural snow is highly uncertain at the moment and therefore any modeling attempts with hypothesized scenarios of such chemistry would suffer from large error bars. As such, it is perhaps not necessarily wrong if one rationalizes the focus only on the gas-phase chemistry as is done by the authors. But then it would be more appropriate if the authors set out the introduction of the paper more clearly by reflecting such a standpoint; as is currently composed in the introduction, the paper initially gives an impression that the role of nitrogen oxides deposition to the aerosols and the snow, namely, (R8-12) and (R13-14), is to be elaborated more than what is presented right now. The introduction should perhaps include a few criticisms/doubts/limitations on the existing models dealing with snow chemistry and a clearer and more vigorous statement as to what the authors actually do in this study.

We added several sentences at the end of the introduction in the new revision addressing the complication of simulating snow chemistry and therefore that we have focused on gas-phase chemistry only.

2. Taking their present approach generally granted, I still feel that the authors could have discussed some of the elements more quantitatively and vigorously. I can see that the model clearly shows the large impact of changing NO_x levels on gas-phase bromine chemistry (black vs. red lines in Figure 7A-B), which, however, does not reproduce full details of the BrO and HOBr variability observed in the field owing to inherent limitations in mimicking all the environmental parameters. Then, there could be other ways to articulate better the logical connection between the enhanced NO_x and suppressed BrO and HOBr observed at Barrow. If the problem associated with the uncertain condensed-phase chemistry is indeed alleviated by constraining the Br₂ and Cl₂ concentrations as measured in the field, one could also test a third scenario using the actual day-to-day variations of measured NO_x levels (in addition to two existing scenarios, namely, "high NO_x" and "low NO_x") in the box model and compare the results just like Figure 7A-B.

We performed a simulation where the gas-phase NO_x was constrained to the NO_x observations of the simulation period. The BrO and HOBr generally were over simulated compared to the observations because of over production of HO₂ in the model.

3. Also, can the relationship between BrO DSCD and NO₂ DSCD found from the BROMEX aircraft MAX-DOAS data and presented in Figure 8 be explained broadly and yet quantitatively by gas-phase mechanisms examined in the present model? For example, by assuming upper and lower bounds for the vertical dimensions of the air masses below the aircraft to infer the actual concentrations of BrO and NO₂, one can probably discuss the context of this observation in relation to the model runs in a more quantitative fashion.

Figure 8 has been altered and a discussion was added comparing the BROMEX data to the modeling results.

[Technical comments]

1. I still don't understand why the authors like to use the term "mole ratio" for referring to what we normally call "mixing ratio" or "mole fraction" in the atmospheric science community. As far as I can tell from search results on the occurrence of the term "mole ratio" in past ACP articles, the term "mole ratio" perhaps has never been used in lieu of "mixing ratio" and "mole fraction", see

http://www.atmos-chem-phys.net/full_text_search.html?q=mole%20ratio%22

Also, check with the index words "Concentration, gas and liquid solutions" in the Glossary of Atmospheric Chemistry Terms available online at

<http://media.iupac.org/reports/1990/6211calvert/glossary.html>

Since the authors seem to have no problem in using the term "mole fraction", I suggest using it instead of the "mole ratio" throughout the manuscript.

We changed the wording from mole ratio to mixing ratio, throughout the manuscript.

2. Equation (1) seems to remain a bit confusing. In the denominator, "k[Br][C3H6O]" should be corrected to "k[Br][C2H5CHO]" for consistency with the later occurrence of "k[BrO][C2H5CHO]" in the same equation, while "k[Br][C4H8O]" should be eliminated if C4H8O is indeed unused in the model as stated in the authors' response to my previous comments.

That was a typo, as both propanal and n-butanal are included in the model. The equation was edited and a note was added to the supplemental information.

3. In the revised Table S5, "C2H5CHO" (propanal) is listed as species measured by canister sampling and offline GC-MS along with light hydrocarbons (C2H2, C2H4, C2H6, etc.). I suppose this is wrong and the earlier version of Table S5 was correct ("C3H6", namely, propene was listed instead). Please correct. Now, I wonder if C2H5CHO was measured by any of the instruments listed in Table S5.

The following species were measured using the online GC/MS:

CH₃CHO, CH₃COCH₃, MEK, *n*-C₄H₁₀, *i*-C₄H₁₀, C₂H₅CHO

We have corrected Table S5.