Atmos. Chem. Phys. Discuss., 14, C3839–C3842, 2014
www.atmos-chem-phys-discuss.net/14/C3839/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "How sensitive is the recovery of stratospheric ozone to changes in concentrations of very short lived bromocarbons?" *by* X. Yang et al.

Anonymous Referee #3

Received and published: 19 June 2014

This paper reports simulations of atmospheric ozone using the UM-UKCA model for 3 levels of stratospheric bromine loading (10, 15, and 23 ppt) and 2 levels of stratospheric chlorine loading (0.8 and 3 ppb). From these calculations the conclusion emerges that "changes in the concentration of ozone will be dominated by the recovery of anthropogenic chlorine" (page 9730, lines 21 and 22) but should there be a "5 ppt increase in the inorganic bromine from VSLS, the depletion of stratospheric ozone can reach up to 10% in the annual mean in the lowermost stratosphere of the SH polar region and 4 to 6 % in the NH" (page 9738, lines 3 to 5) with the exact amount of ozone depletion depending on chlorine loading.

I am "on the fence" regarding the appropriateness of this paper for ACP. On the positive side, the paper is succinct, clear, and likely correct given the assumptions that went into the calculations. On the downside, the paper constitutes the so-called "least publishable unit": so much more could have been done to model the effect of VSLS on ozone and the results in the submitted paper do not scratch below the surface. Also, most of the effect of VSLS halocarbons is found in the troposphere, yet this paper does not (apparently) consider the chemistry of iodine. To me, the lack of treatment of iodine coupled with the dominant effects being modeled low down is decisive . . . I suggest this paper not proceed until a more complete look at the effects of VSLS on ozone, including a treatment of iodine, is added.

Major Comments:

1. There is so much more that could be done to study the effect of VSLS on ozone. Here, the model is run for year 2000 conditions for GHGs, SST, etc. Only halogen levels are perturbed for other years. However it is well known the dominant catalytic cycle for halogen loss of ozone in the lowermost stratosphere is rate limited by the reaction BrO+HO2 (e.g., Figure 5 of Salawitch et al., GL, 2005). The levels of HO2 are controlled mainly by ambient H2O in the LMS and to a small degree by overhead ozone and tropospheric CH4. By restricting the analysis to conditions for year 2000, the potential effect of BrO+HO2 on ozone is not examined. At a minimum, this should be acknowledged. Other issues not touched by the design of the calculations in this paper are the sensitivity of ozone to future volcanic eruptions (here VSLS bromocarbons should matter a lot; many of the co-authors have written on this subject) and possible geo-engineering of climate via stratospheric sulfate injections. Tilmes et al., ACP, 2012 certainly have shown that in one model at least, VSLS bromocarbons and chlorocarbons matter a lot should society dare take this course of action. Would be nice to see if similar results are found in the UM-UKCA model.

Finally, a new emergent issue is the possible role of VSLS chlorocarbons. Atmospheric levels are growing and the source seems to be anthropogenic. The coupling of VSLS

bromocarbons from the ocean with VSLS chlorocarbons from industry must be quantified. My concern with allowing this paper through to ACP in something close to the present form is important issues such as H2O in the LMS and potential synergy with VSLS Bry; volcanoes and/or geo-eng, and coupling of VSLS Cly and Bry are all "swept under the rug". I suggest a revision (or resubmission) focused on most if not all of these topics.

2. Most of the modeled the impact of VSLS halocarbons on atmospheric ozone is found below the tropopause. Yet, iodine is apparently not in the model: there is no mention of the word iodine in the paper. Saiz-Lopez et al., Estimating the climate significance of halogen-driven ozone loss in the tropical marine troposphere, ACP, 2012 should be cited. This paper suggests that without representation of the chemistry of iodine, not much faith can be placed in a calculation of the effect of VSLS bromocarbons on ozone, particularly at low altitudes.

Minor comments:

1. The first paragraph of the introduction is misleading and incorrect. The paper states "first investigations focused on the role of chlorine but subsequent research showed that coupled chlorine-bromine reactions made a substantial contribution to the polar ozone loss", with a reference to a 1998 paper written by Chipperfield and Pyle. Of course McElroy et al. (Nature, 1986) first suggested a role for BrO & ClO in the context of the ozone hole and the early field research in Antarctica following Farman's 1985 paper focused on the role of coupled chlorine-bromine reactions: e.g., Salawitch et al., Chemistry of OClO in the Antarctic stratosphere - implications for bromine, Planet. Space Sci, 1988. The first PP of this paper reads like a strange, revisionist history which I am sure was not intended.

2. I consider Ko et al., Ozone depletion potential of CH3Br, JGR, 1998 to be the precursors of a lot of work on VSL bromocarbons and suggest this paper be reference in the second PP of the introduction.

3. The color scale chosen for Figure 1 is not optimal. Why have red if this color does not appear on the plot? Either revised the color bar so it stops at 2%, or else change the numbers associated with the colors so that red is used.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9729, 2014.