

Interactive comment on “The contribution of anthropogenic bromine emissions to past stratospheric ozone trends: a modelling study” *by* **B.-M. Sinnhuber et al.**

R. Salawitch (Referee)

rjs@caesar.jpl.nasa.gov

Received and published: 19 September 2006

This paper reports calculations using a 2D photochemical model to assess the role of bromine emissions to past stratospheric ozone depletion and reports a value for the efficiency of chemical loss of bromine relative to chlorine, the so called alpha factor. The analysis shows that anthropogenic emissions of bromine are responsible for about half of the past ozone depletion. A value of 69 is found the alpha factor, significantly larger than the previous estimate used in the WMO/UNEP 2002 Ozone Assessment report. However, this higher value of alpha is consistent with other recent estimates. The paper is well written, reports calculations that are timely given their role in the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

WMO/UNEP 2006 Ozone Assessment, and is clearly suitable for publication in ACP. I offer the following suggestions, all "minor", for consideration upon revision. In all cases, the authors should decide whether a change to the paper is warranted; none of these suggestions are essential.

1. The fact that the ozone loss by bromine occurs via the combined BrO/ClO catalytic cycle (line 190) should be represented as well in the abstract.

2. The paper reports calculations using JPL-05 preliminary, which was the kinetics data set used for the 2006 WMO/UNEP Ozone Assessment. As such, this is perfectly fine (although it is regrettable the assessment community had to rely on a preliminary set of kinetic parameters). Recently, however, JPL 2006 has been released. If the authors have time to complete a run using JPL 2006, and can add few sentences noting how these results compare to the JPL-05 preliminary case, this would be helpful to the community. I realize, of course, I am suggesting a huge amount of work for "a few sentences". This is entirely up to the authors as to whether these additional calculations can be conducted within the time frame available. Am suggesting simply because such information would be so valuable.

3. The paper considers the effects of very short lived bromocarbons on stratospheric ozone trends, keeping the level of VSL bromine constant over time. All other previous efforts to quantify the role of VSL bromocarbons have made the same assumption. In the meantime, Worton et al. (ACP, 6, 2847, 2006) have reported that while brominated dihalomethanes have remained nearly constant over time, the brominated trihalomethanes (CHBr₃, CHCl₂Br₂, and CHClBr₂) have increased by about 20% from 1950 to 1990 (see Table 5 of Worton et al. for precise values). It is speculated that anthropogenic sources are responsible (see also discussion in Section 2.2.4.1 of the final draft, Chapter 2, for the 2006 WMO/UNEP Ozone Assessment report, which some of the paper authors have helped prepare). This raises the possibility of an additional calculation, considering the effect on ozone of increasing VSL bromine over time, based on the Worton et al. (2006) measurements. Again, the authors should decide if such

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

a calculation can be carried out in the time available. I think the results would be quite interesting.

4. The discussion on lines 104 to 115 about VSL Bry is overall fine, but it does tend to minimize slightly some of the uncertainty of the exact contribution of VSL bromocarbons to Bry. The Sioris et al. paper is now out (JGR, 111, D14301, doi:10.1029/2005JD006479, 2006) and should probably be referenced. While it is true that the central value for VSL Bry considering many studies is probably "of the order 5 to 6 pptv", there is considerable uncertainty still associated with this estimate. For instance, the Sinnhuber et al. (2002, 2005) studies reported a smaller value of 3 ppt and Sioris et al. (2006) reports a higher value of 8.4 ppt. I think a slight re-write of this section, adding a bit more detail (no need to be exhaustive!), would improve the paper because there is still considerable uncertainty in the value of VSL Bry.

5. I like section 3.2 and think it is an important result for the ozone assessment community. However, I have never understood why alpha is calculated assuming bromine is released in the stratosphere in exactly the same manner as chlorine is released from CFC-11. We know that bromine supplied to the stratosphere, even from CH₃Br and halons, is released more rapidly than chlorine is released from its reservoirs.

I am not suggesting any change to the paper. Indeed, Sinnhuber et al. must calculate alpha in the same manner as Daniel et al. 1999, for proper comparisons, and for use in the assessment

Since these reviews are posted publically, thought I'd take this opportunity to criticize the overall approach the community has devised to find this single, "magic number".

6. Bobrowsky (lines 179 and 331) should be Bobrowski

7. Wuebbels (line 289 and 329) should be Wuebbles

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6497, 2006.