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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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GENERAL COMMENTS

This manuscript adds to our understanding of the effects of both short- and long-lived bromine source gases on stratospheric ozone. The consideration of bromine on the magnitude of ozone trends and the estimate of the bromine efficiency for ozone depletion (compared to chlorine) are useful additions and updates to our current understanding. The authors are to be commended for the timeliness of this work with respect to the upcoming scientific ozone assessment, particularly regarding the calculation of the bromine efficiency. The paper deserves publication after several additional points are addressed.

SPECIFIC COMMENTS

p. 6499, line 4 - This summary of the Salawitch work suggests to me that it found a large effect on the trend, while the Feng et al. work found a large effect only during times of elevated aerosols. However, the Salawitch figure showing the ozone trends with different amounts of additional stratospheric Bry suggests a small change to the overall trend, with a large effect during the elevated aerosol periods.

p. 6500, lines 1-5 - It might also be useful to point out that the global alpha values in the past have also been calculated by 2-D models. The only 3-D model study that I am aware of is the Chipperfield and Pyle work (1998) for the Arctic.

p. 6500, lines 25-26 - Why is the direct forcing of the ODSs (CFCs, HCFCs, etc.) not included in the calculations. Because of the rapid change in these gases over the time period considered, it seems that they should be included. If it turns out that they are not important this should be stated, along with the reason.

page 6501, line 25 - 6502, line 7 - First, you state that some studies indicate that there 'must be a contribution ... of the order of 5-6 pptv' based on observations. Then you stated that the exact contribution in the 'real atmosphere' is unclear. This intent of the combination of these sentences is unclear to me. I would suggest rewording this paragraph and including references to some of the studies that suggest a different amount of additional Bry from short-lived bromocarbons. Also, it would be helpful to provide the range encompassed by the majority of the studies so the reader can have a feeling of how good of an approximation the 5-6 pptv is.

page 6507, lines 17-19 - Due to the coupling of ClO and BrO the insensitivity to pulse increases of 10% to 50% seem surprising. Perhaps it would help to quantify the difference between calculations using a 10% and 50% pulse (e.g., less than x% for the value of alpha).

page 6508, lines 10-16 - You suggest a possible reason for the decrease in the alpha

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profile in response to enhanced bromine. I infer that you have not dianosed this with the model. If you could do so, it would be helpful. Whether you can or cannot, at least please do give a plausible suggest for the increase in the alpha profile during other seasons.

page 6509, line 9 - Please state the reason for the decrease in modelled ClO.

page 6509, lines 11-22 - I have several comments that relate to this section. First, you need to be careful in applying the WMO fractional release factor of 1.12 to the AER BEF (bromine efficiency factor) calculation (Ko et al, 1998). The current AER model suggests a large fractional release factor, which would lead to an alpha value lower than the 52 as taken from the 1998 Ko et al work, and more in agreement with the Daniel et al. 45. More important is the large increase in the AER alpha estimate, which I believe should be more emphasized.

You show column alpha results for the Arctic that appear to be substantially higher than the results of Chipperfield and Pyle (1998). You should comment on this comparison as well and state whether the discrepancies are understood.

I believe that a statement emphasizing the large model-dependent range among the alpha values (even for recent calculations) would be useful.

Finally, from the northern and southern hemisphere partitioning of ozone trends between chlorine and bromine, one can infer an average bromine efficiency factor over the 1980-2005 period. If I have done the calculation correctly, I obtain values of 117 and 77 for the two hemisphere when assuming 45% and 35% of the ozone trend is due to bromine. For 1998 conditions, you calculate a global value of 73. Can you provide information that can reconcile these values. Although the values are not terrible different, particularly when one considers the greater depletion in the southern hemisphere, I would have expected the values to be a little closer. I feel a little discussion on this matter would be appropriate.

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page 6521, figure 8 - why is there such a kink in the alpha profile near 27 km for the standard model run?

page 6522 - additional figure? - It would be useful to show your ozone loss profile for the additional pulse of CFC-11 used in the alpha calculation. This would further help to compare with the Daniel et al. work.

TECHNICAL CORRECTION

page 6509, line 15 - Wuebbels should be Wuebbles.

page 6514, fig 1 - I would suggest removing the lines separating the minor CFCs and HCFCs since there is no information regarding which region corresponds to which gas. Remove the '(?)' from the lower panel. You state that the contribution in the real atmosphere is uncertain; your addition to the model is not uncertain. Please also list the HCFCs and minor CFCs that you considered parenthetically, for example.

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