

Interactive comment on “Emission location dependent ozone depletion potentials for very short-lived halogenated species” by I. Pisso et al.

Anonymous Referee #1

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In this manuscript, the authors develop a method to calculate ozone depletion potentials (ODP) for very short-lived species (VSLs), based on tropospheric and stratospheric trajectory calculations. The calculation of ODPs for VSLs is challenging, as they depend on the location and time of emission and on the details of tropospheric transport pathways. This new methodology is an important step forward to calculate ODPs for long-lived source gases and VSLs in a consistent framework. As an example the ODP for n-propyl bromide is calculated here, with the results giving larger ODPs for n-propyl bromide compared to other recent estimates. The paper is in general well written and I recommend publication in ACP after a few mostly minor revisions.

General comments

1. Although I understand that the calculation of the ODPs of n-propyl bromide serve
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mostly as an example for the methodology developed and that there are some simplifying assumptions used in the current study, the large differences in the calculated ODP between this study and other recent studies (e.g., Wuebbles et al., Three-dimensional model evaluation of the Ozone Depletion Potentials for n-propyl bromide, trichloroethylene and perchloroethylene, Atmos. Chem. Phys. Discuss., 10, 17889-17910, 2010) makes me slightly uneasy. In particular as these results may have political and economic consequences. Similarly, it is not fully satisfying that the calculated ODPs depend so much on the details of the calculation (differences by more than a factor of 2 for the ODPs for calculations with and without explicit treatment of convection). I feel that a bit more discussion is needed to put the ODP values into perspective.

2. The derivation of the equations is not always easy to follow and at some places a more detailed explanation would be helpful. In particular the explanation of ζ_{SG} and ζ_{PG} (p. 16282, l.10-13) is not particularly clear. Moreover, it was not clear to me how these two efficiency factors entered the calculation of the ODPs of n-PB in section 3. Are there explicit or implicit assumptions made with respect to these factors?

Similarly, it would be helpful to say more explicitly how the "fractions of the source and product gases that reach the stratosphere" are defined. Number of molecules that reach the stratosphere relative to the number of molecules emitted? Or fraction of mixing ratio in the stratosphere relative to the mixing ratio in the planetary boundary layer?

Specific comments

p. 16286, l.14: Which two factors are you referring to?

p. 16292, l.19: I have some problems with your phrase "The estimates of ODPs are, of course, dependent on the assumptions underlying the modelling methodology...". My understanding is that the ODP should be a number that is useful for decision makers and should not depend too much on the details of its calculation.

p. 16296, l.23: It does not seem to be straight forward how you could calculate "...how the ODPs change as tropical sea surface temperatures change in the future". Please specify or remove.

Technical corrections

Affiliations, p. 16277: As given here, P. H. Heynes is with UPMC and K. S. Law is with DAMTP. Is this correct?

p. 16285, l.5: This sentence is too long and should be split in two.

p. 16289, l.28: insert "be" between "might" and "expected"

Fig. 3: The title of Fig. 3 says this is "year average" and the caption says this is "in February".

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16277, 2010.