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Interactive comment on “Three-dimensional model evaluation of the Ozone Depletion Potentials for n-propyl bromide, trichloroethylene and perchloroethylene” by D. J. Wuebbles et al.

Anonymous Referee #3

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The authors have calculated ODPs for n-propyl bromide and trichloroethylene and perchloroethylene. The authors correctly point out that a good 3-D chemical transport model is needed to accurately quantify these values. Also, noting the importance of emissions location, they have specified emissions to be between 30-60N with a comparative case for nPB run with emissions over 60S-70N latitude. The study is important because of the growing interest in replacing longer-lived ozone-depleting compounds with short-lived substances. If it can be successfully shown that short-lived chlorine- and bromine-containing compounds have extremely low ODPs, this could have important policy implications.

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The authors have written clearly and their approach seems appropriate. My biggest concern involves the use of the MOZART-3 model. If the authors wish to convince us that the ODPs of these compounds are truly as low as the MOZART model calculates, they need to provide additional explanation. For example, for nPB, the authors should be very clear whether they feel their values are preferable to previously published values, or do they simply complement the previous values and provide more confidence in our estimated range. Also, there is almost no discussion of uncertainties associated with the ODPs or of the convection scheme, which is critical to the accurate calculation of ODPs for very short-lived compounds. I will include some other specific comments regarding this below. If the comments are appropriately addressed, I believe the manuscript is appropriate for ACP and should be published.

17890 line 12 – the Bridgeman and Olsen results might not be higher – right? They estimate lower and higher values depending on the time and location of emissions. Consider rephrasing.

17891 line 6 – insert ‘the’ before ‘ODP’

17891 lines 6-21 Why don’t some of these CTM models lead to values just as valid as the ones in this manuscript?

17892 lines 6-9 Are these ODP values calculated from the Wuebbles et al. (2001) work? I assume so, but please clarify. Again, it is not clear that the current values, if calculated for SE Asian emissions, would be lower than this 0.03.

17893 line 26 You state the importance of convection, but include no discussion of the accuracy of the convection scheme employed by your model.

17894 lines 16-19 Why are CTMs necessarily better than 2-D models for long-lived gases. It seems that as long as the model accurately calculates the ozone field and the lifetime, or more specifically the spatial distribution of destruction, for the source gas, the ODP should be accurate. I completely agree for the short-lived gases though.

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17894 lines 21-23 This is strongly dependent on the tropospheric lifetime, isn't it? Please clarify.

17895 line 1 In the conclusions of this reference, there are several problems discussed regarding the MOZART model when driven by the WACCM1b winds. As one example, there are significant differences between the observed and calculated age of air. While the problems are fewer than when using the ECMWF or the EXP471 winds, the issues raised could still be pertinent to this study. It would be particularly helpful if the authors could address the magnitudes of the errors in the calculated ODPs both from the met data used and the convection scheme.

17895 Line 9 In the Pan et al. paper, however, there is some discussion about the problem of insufficient vertical resolution near the tropopause. So again, a discussion of the uncertainty in the calculated ODPs is necessary.

17896 lines 10-14 Please clarify what is done with the degradation products. You make it clear that they release all chlorine in one time step. But what happens to this chlorine after that? It assume you then apply a rainout scheme. Please provide a clear explanation at this location, early in the text.

17896 lines 26-29 Please include the length of time the model had to be run. Also, it would be useful to include the actual equation used to convert the calculated, steady-state ozone change and the source gas emissions to an ODP.

17896 line 28 Is such a small ozone change sufficient to have the ozone change be significantly larger than the noise?

17897 line 7 Consider inserting 'An' before 'ODP'.

17897 lines 7-12 This would seem to fit better in the previous section

17897 lines 18-19 I do not understand 'as scaled ...'. The caption just says 'percent'. Is that not correct?

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178967 line 23 Please clarify that this is the excess mixing ratio

17898 line 2 I would recommend more explanation regarding the lack of transport across the tropopause south of 20N latitude. Does the higher latitude transport dominant because the cross-tropopause transport is occurring in the NH summertime, or is this an indication of too much diffusion of the tropospheric mixing ratios into the stratosphere?

17898 line 9 Does this ODP value include both tropospheric and stratospheric ozone depletion? Please clarify.

17898 line 26 Same as comment for line 2 of this page.

17898 line 26 Could this apparent increase at higher latitudes at the tropopause be caused simply by your tropopause definition?

17898 lines 27-28 This argument seems illogical. Most of the mass could be entering through the tropics with a small additional amount augmenting the extratropical burden.

17899 line 6 Clarify what you mean by the “BrAc correction” the first time. Perhaps, just say ‘correction for its revised lifetime’.

17899 lines 6-10 Can you provide more information why?

17900 line 21 This number is more than 10 years old; please provide a more current number or at least an estimate of how much growth/decline has likely been experienced

17901 lines 10-11 But if HCl is rained out, the Cl would not make it to Cly, which you also rain out I believe. Therefore, including HCl may have little effect.

17901 lines 12-14 They are maximum values only for this effect. If convection is underestimated, for example, values could be substantially higher.

Figures – please consider including in the caption (at least of Fig 1) that the white line is the tropopause. I realized you already define it in the text.

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The J.G. Levine 2007 JGR paper seems highly relevant to this work. If the authors agree, I would suggest that it be appropriately referenced.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17889, 2010.

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