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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new formulation of equivalent effective stratospheric chlorine (EESC)" *by* P. A. Newman et al.

Anonymous Referee #4

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GENERAL

This paper presents a new, improved formulation of EESC, a concept that has been widely used (especially in WMO ozone assessments) as a measure of the stratospheric halogen burden. Moreover, the paper presents a detailed analysis of the sensitivity of EESC on the various parameters that determine EESC. An important finding is that this new formulation of EESC implies different years for 'recovery' than stated in WMO (2007) and that these differences are driven by different fractional release values estimated in this paper for some CFCs.

An important feature of the new EESC presented here is that it is directly related to the stratospheric burden of Cl_y, Br_y, and F_y. However, no use is made of this feature in the

paper. I suggest that values of Cl_y, Br_y, and F_y are indeed calculated and compared with information available in the literature on these quantities. Such an exercise could be important to corroborate the accuracy of the EESC estimate proposed in this paper.

Further, both the fractional release value f and the (stratospheric) lifetime of halogen species are concepts of central importance to this paper. But both of these concepts and their use in the paper could be much better explained than in the present manuscript. For example it is not clear from the paper, how the fractional release values f listed in Table 1 have been calculated (see also detailed comments below).

In summary, this is a well written and organised paper addressing an issue of great interest to the ACP readership. I am sure it will be well recognised and cited. The suggested changes, at least in my view are important, but should not take too much time to implement.

DETAILED COMMENTS

The fractional release value f is one of the most important parameters for the calculation of EESC in this study. Nonetheless, it is not well explained in the paper, how the values used here are calculated. In the paper it is stated that:"Table 1 lists 16 different species used to estimate EESC in this study along with [...] and observationally derived fractional release values for 3– and 5.5-year mean ages (valid in the lower stratosphere)." It is not clear to me if these values are identical to those used in Newman et al. (2006). Please clarify. And which observations are they based on? The same as those used in Schauffler et al. (2003)? Further, it is not clear from reading the Newman et al. (2006) paper and its supplement how strongly the f values calculated there deviate from those in Schauffler et al. (2003) that are possibly based on the same set of observations. And what is the improvement in the Newman et al. (2006) values compared to Schauffler et al. (2003)? Finally, I think it is worth stating in the paper that the lifetimes listed in table 1 are not used for the calculation of f (if this is true).

top of page 3971: Here the term 'stratospheric lifetime' is introduced but not defined.

ACPD

7, S1187–S1190, 2007

Is this value supposed to be valid at all altitude throughout the stratosphere? Also, in Table 1, it is stated that stratospheric lifetimes are listed for species whose lifetimes are significantly different than the 'atmospheric lifetime'. Which would mean that, e.g. the CFC-115 lifetime of 1700 years is a 'stratospheric lifetime'. Naively, I would interprete the information given in Table 1 that the burden of CFC-115 and HCFC-22 has been reduced by a fraction of 2.718 after a stratospheric residence time of $1700\ 288$ years, respectively. I suggest to revise the discussion here to avoid such misinterpretations.

p. 3976, l. 8: the value for Br_y calculated here (9 ppt) is considerably lower that estimates of this quantity in WMO (2007). I suggest to comment on this discrepancy.

p. 3981, l . 9: I cannot agree that is is 'clear' from Farman et al. (1985) alone that ozone loss had occurred as early as 1975. If it would be so obvious, it would not have taken a decade for the Farman et al. (1985) paper to be written :-). After all this paper is only based on total ozone from one particular station. Perhaps other ozone loss measures, e.g., those put forward in recent papers by Huck et al. are quantities that can be considered to support this statement.

MINOR ISSUES

p. 3964, l 23: adjustment $\longrightarrow$ adjustments

p. 3966, l 4: insert 'projected' after 'on'

p. 3966, l 12: 'stopped increasing' $\longrightarrow$ in the troposphere or in the stratosphere?

p. 3967, l. 4: This definition of f is not consistent with the one in Table 1 (where f is absolute rather than relative to CFC-11). Clarify.

p. 3975, l. 12: Here it is stated that f is 'determined from mean age alone'. If this is true, than it should be straightforward to give the function $f_i = f(\tau_i)$ with τ_i the lifetime if species i.

ACPD

7, S1187–S1190, 2007

p. 3976, l. 1,2: I believe there are a few more recent studies on the value of α that might be helpful here.

p. 3981, l . 18: remove ‘rates’

p. 3982, l . 22: replace ‘the change’ by ‘an acceleration’.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 3963, 2007.

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7, S1187–S1190, 2007

Interactive Comment