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# *Interactive comment on* "Uncertainty analysis of projections of ozone-depleting substances: mixing ratios, EESC, ODPs, and GWPs" *by* G. J. M. Velders and J. S. Daniel

**Anonymous Referee #2**

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Overall this paper is well written and to the point concerning the future levels for EESC and various ODSs. I know it has been common practice in this part of the community to discuss fractional release as if it were a constant, but I also have experienced mis-understanding due to this practice outside the community. I encourage the authors to take a few sentences to explain what they are doing with fractional release – particularly the appropriate altitude range. This will help set up a better presentation of the empirical ODP formula which is attempting to account for different altitudes of maximum destruction for different ODSs.

I also have a bunch of minor comments; I think addressing these comments would

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improve clarity.

Abstract:

I understand that the abstract is supposed to include some conclusions. On the other hand, the part 'comparable to a hypothetical scenario' onward is pretty obtuse unless the reader is already completely in on the problem. Might be better to state in words how the upper ranges were calculated. ( I understand it having read the whole paper, I encourage you to make the abstract more physical rather than stating the result The latter end of the range corresponds to X which is the Same as' – this is a result you get from a sensitivity test, not an inherent attribute of the system (which is how it seems when reading).

28019 L 9 (and elsewhere) The way you talk about 'fractional release' always disturbs me, because it comes across as a single quantity for all locations when of course it is a function location (lat/alt – in the extreme view it is '1' in the upper stratosphere)) and can change if the circulation changes in a way to alter the age spectrum (e.g., Li et al., 2012). Same is true of mean age, of course. The values you use must for a specific altitude? There is a really nice theoretical paper (Hall, 2000) that shows how the age spectrum and fractional release are related through trajectories.

28037 Don't gain that much perspective comparing with the radiative forcing due to N2O (or other non-CO2 gases) without some perspective concerning radiative forcing due to CO2.

28037 That semi-empirical formula was always a stretch. ODPs were invented when people thought nearly all loss would be in the upper stratosphere, so the 'f' factor would have been 1. Need to be clear that this factor is comparing lower stratosphere – specifically accounting for lower atmosphere destruction of specific compounds which changes their 'potential for O3 depletion' relative to $CFCl_3$ that has to find it's way to the mid stratosphere to be photolyzed.

Minor and grammar:

Abstract: uncertainty is much larger than the two year change – do you mean the uncertainty was always larger than 2 years? Or did the old 'return' not have uncertainty.

28022 4 you say that Fsurf for CH3Br is different from all other ODSs, and you say what it accounts for, but where do the numbers come from? I note you talk about this later (28029) by saying WHERE the numbers come from (Yvon-Lewis (2009)). I think you should skip the numbers in the introduction and state them here, and I wish you could add one sentence that at least alluded to methodology or data set or rationale for the values.

28022 L 15 In the same paragraph you say that global mixing ratios and lifetimes are known more accurately than are emissions (bottom up) but then use bottom up to calculate the bank. This does seem circular. I think it is just fine to do – but I am not so sure about claiming top down is more accurate. The uncertainty has to be somewhere.

28022 L 9 – omit 'commas' the top-down approach is taken because

28026 L 15 decreasing to about 20% larger around 1980 and smaller towards 2010. What does that mean – do you mean that the instantaneous lifetimes is 20% larger than the steady state lifetime and the difference decreases between 1980 and 2010?

28027 L 6 I should think that the lifetime from a CTM (using assimilated fields with all the known warts in their circulation) would be different from lifetime calculated using a CCM

28018 L1 There is some 'order' issue here with OH and the next paragraph.

280030 L 20 You start out with percent differences all 16% or below, then call other CFC differences 'relatively large' when two are nearly 50%. Just say 'other differences'

28036 L 5 I agree that 1980 for return is arbitrary, and that models show loss prior to 1980, but I don't think that WMO 2011 made that conclusion about observations.

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28037 ODPs are indices that provide a measure to compare the ability – that is a really long lead in! How about OPS are indices used to compare I also think since you said 'compare' you can omit 'relative to'.

28043 Just curious – where future mixing ratio depends on uncertainty in the back – how long do we have to wait to for that uncertainty to decrease?

28037 L1 and 28045 L26 Is expected to decreases to

28039 L 21 violating a comma rule 28040 L 9 " " " "

28041 L 25 's' should be 'a'

28043 L 19 types of applications in which the ODS is used.

28043 L 26 delete 'even'. 28045 L 3 'based on a baseline'

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28017, 2013.