Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Delayed Recovery of mid-latitude lower stratospheric Halogen Loading" *by* Andreas Engel et al.

Anonymous Referee #4

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This manuscript has built nicely on Ostermoller et al. (2017). The concept developed there is used to derive a relationship between previously calculated fractional release (FRF) values that assumed an age spectrum representative of an inert tracer to FRF values that are independent of tropospheric source gas trends. More importantly, this work quantifies the importance of using an age spectrum that accounts for chemical loss when calculating equivalent effective stratospheric chlorine (EESC). This improved approach effectively leads to older air in the EESC calculation, particularly for the mid-latitude stratosphere. This, in turn, implies lower EESC values in 1980; this 1980 level has been important because it has typically been taken as a value of significance in the return of stratospheric chlorine/bromine to natural levels. The proposed EESC revision (i.e., older air) also leads to higher EESC values for any given time when source gases

are declining. These changes combine to lead to a substantial delay in the time when mid-latitude EESC is projected to return to 1980 levels. As expected, the effects are smaller for polar EESC, since the difference in the average age for the dissociated ODSs and an inert tracer are much reduced.

I have a few general comments here, and some more specific ones below. Assuming these comments can be dealt with sufficiently, I find this manuscript to be valuable and I believe that it should offer an important improvement on work that came before it.

It would be useful to describe whether EESC from the new formalism is distinct enough from EESC using the old one so that past work that used EESC should have identified a shortcoming in the previous approach. Looking at Figure 3, I would be particularly interested in previous work that compared measurements or model calculations over a time range that spanned both before and after the EESC peak in the late 1990's, since the differences should be most apparent over such a period. If the two approaches are not distinct enough to be apparent in previous work, this would be worth stating here, so the reader knows the main impact is on the "recovery" date, and that it doesn't affect the validity of previous results.

The only other comment I particularly want to highlight here relates to the sensitivity study of the width of the transport distribution function. Please see my comment below for page 14, lines 2-3. I would find this most useful if you explored the impact of a change in width of the age distribution relevant to an inert tracer, with that impact propagating to the halocarbons depending on their chemical loss; however, unless I am mistaken, it doesn't seem like this is what is done.

Specific comments:

Page 1, Line 1 At some point, relatively early in the manuscript, you should make clear what you are not implying by this title, otherwise it could be considered misleading. As currently written, it could be taken to suggest that there has been more ODS emission than expected or that dynamics may change in an unexpected way to alter halogen

loading in the future. An alternative that may be preferable would be to change to a title more focused on the delay in EESC recovery.

1, 16 1980 is not the year of stratospheric ozone depletion onset, but it is often used as a benchmark to measure significant progress towards recovery

3,16 I suggest clarifying what 'this purpose' refers to at the end of this sentence

5,1-3 It is not clear to me that this sensitivity study addresses the entire phase space of possibilities in your assumed relationship between age and loss. Additional justification is needed to show that the simple relationship you are basing your calculations on are sufficiently appropriate.

6, 2 It is not clear to me that having the loss described as an exponential term with the lifetime depending on transit time is helpful in the formulation. It is really of an arbitrary mathematical form since the lifetime (denominator) varies with the location endpoint. It would seem more straightforward to skip straight to the factor $(1-f(t'))$, but I leave this decision to the authors.

10, 19 Somewhere you should discuss the impact of using the Plumb age estimates from an old 2-D model given the advancements in our ability to calculate circulation metrics over the last 20 years and the general superiority of 3-D models at making these calculations today

14, 2-3 I am having trouble understanding exactly what is being done here. Are the factors changed for both G and $G\#subN$? It would not be possible to have gamma be 0 for $G\#subN$ and be 0.7 for G, would it? But if both gamma factors are 0, it would seem that this approach would collapse to the old result since the mean release age would be the same as the mean age of an inert species. And if that is the case, I would have expected a larger impact on return times (i.e., they should be close to the VD (2014) values). Perhaps it would help if you had a figure (like Figure 1) showing what the G curves look like as gamma goes to 0 and for it equal to 2. It looks like you may be

using the $\Gamma\#$ values from Table 1; however, this doesn't seem appropriate if you want to examine the impact of a changing shape in the overall transport distribution function. In fact, I'm unclear physically what is going on here, so clarification would be very helpful.

14, 25-29 Please describe how the destruction vs. age relationship is determined for these perturbations

15, 15 Perhaps broaden this statement, if you think it is accurate to something like "This approach more accurately represents the amount of Cly and Bry in the stratosphere from tropospheric source gas concentrations, and should be adopted to estimate..." This would seem to be more consistent with the title, but do please refer to my earlier concerns of such a broadening.

Minor comments:

1, 10 Do you mean 'adopted' here? While it is adapted through your work, that doesn't seem to be the intent here

1, 18 Change 'assumed' to 'estimated'

1, 27 Change to 'winter and springtime'

2,1 Replace 'effectiveness' with 'extent' or something similar; otherwise it could sound like the destruction per Cl molecule is what you are referring to here

2,2 Reference EESC

9, 1 Change 'was' to 'way'

9, 3 $1-f(t)$ doesn't seem to be appropriately named as the 'loss term'; I understand why you called it the 'chemical loss term' back in eq. (6), but now in isolation is seems confusing that it is a 'loss term' that is really equal to one minus the fractional loss

12, 5 Change 'compares' to 'compare'

12, 27 Change 'on the new' to 'of the new' at the end of the line

13, 12 Add space in 'STRATcampaign'

13, 23-24 At first, this sentence seemed to suggest that you were doing another calculation from the Ostemoller results, but in fact, I believe you are summarizing the 2060 vs 2058 discussed at the top of this page. This could be clarified.

14, 18 Change to 'independent of'

15, 5 I don't understand the use of the word 'respective' here

15, 8 This seems to not be an appropriate use of 'Therefore'. From what is stated here, the following sentence doesn't seem to logically follow the previous statement(s)

15, 16/17 I suggest changing to present tense

15, 20 Change to '...perturbed values of stratospheric chlorine and bromine...'

15, 21-23 You should probably also point out that CO2 is expected to accelerate column ozone recovery across much of the globe (see, e.g., Butler et al., 2016 and many others)

15, 33 It could be useful to say what G# is here, so people who didn't read the main text will know what it is.

16, 10 Make 'distributions' singular.

16, 17 Change 'calculation' to plural.

Table 1 For CFC-113 and CH3Br add another significant figure '0' to the end of the time-independent FRF. Same comment for various species in Table 2 in last 2 columns

21, 2 Perhaps add 'the' between 'In' and 'case'