

Response to the comments from three referees

We thank all three reviewers for their insightful and constructive comments. The time they spent on our manuscript is very much appreciated and has helped make the manuscript scientifically stronger and clearer.

Anonymous Referee #1

Paper summary: This paper generates a new scenario for the major ozone depleting substances (ODSs). The paper also generates uncertainties for these future projections of ODSs, along with new estimates of ODPs and GWPs. This is the first comprehensive analysis of uncertainties in ODS projections into the 21st century.

Recommendation: Publish with minor revisions. The paper is well written with a very thorough analysis of the uncertainties that would accompany future ODS levels.

Major Comments:

The formulation of the global mean mixing ratio includes a factor “that relates the mass emitted to the global mean mixing ratio”. This factor needs to be included because emissions are at the surface, while the loss is typically in the stratosphere. As the emissions of a compound begin at the surface, the loss will not begin until that compound reaches the stratosphere. In this manuscript, the factor (1.07) is a constant for ALL gases except CH₃Br (1.16).

Response: Both values are indeed not mentioned in WMO(2011), but are used in chapter 5 of that assessment as a way to relate the surface mixing ratio to the total average “box” mixing ratio in our 1-box model. This is required since the model is constrained to surface observations and yet the box mixing ratio average can be thought of as a total atmosphere average. The value of 1.07 is given in WMO (2007, chapter 8). This reference is added to the sentence. The larger value for CH₃Br is based on the fact that a larger fraction of CH₃Br is located in the troposphere relative to other ODSs. Using a molar fraction of troposphere versus whole atmosphere of 0.82 and assuming that 95% of the CH₃Br is located in the troposphere (based on Yvon-Lewis et al., 2009) gives a F_{surf} value of 1.16. I have added the following text to the footnote of Table 2 “The molar mass of the troposphere is about 0.82 times the molar mass of the whole atmosphere. Assuming that about 95% of the molar mass of CH₃Br is in the troposphere (based on Yvon-Lewis et al. (2009)) yields a surface factor of 1.16. The surface factor of 1.07 implies that 88% of an ODS is in the troposphere.”

The authors should also include a table of mixing ratios vs. year for this new scenario.

Response: Good suggestion. We added two files to the supplementary material with all the output data (production, bank, emissions, mixing ratios, EESC, radiative forcing, ODP- and GWP-weighted emissions; mean and percentiles).

Specific Comments (page, line):

Don't use CI for confidence limit. Everyone thinks CI means chlorine.

Response: Agreed. CI is replaced by ‘confidence interval’ everywhere.

There is a mix of methods for quoting uncertainty. Sometimes a range is cited, "... from 0.30 to 0.34 Wm⁻² (95% CI) ..." while sometimes a percentage, "The uncertainties (95% CI) in ODPs are about 30 to 35% ...". I would recommend that you try to homogenize this across the paper.

Response: We have changed the formulation in several places accordingly.

28019, 17 "models begun to have a slow enough stratospheric circulation". This is not correct. Model transport algorithms have advanced significantly over the last 20 years. This resulted in greatly improved ability to properly model age-of-air.

Response: The sentence has been changed to "Douglass et al. (2008) suggested that past estimates of the CFC-11 lifetime were too short and that longer lifetimes are found by models that more accurately simulate atmospheric circulation and the age-of-air in the stratosphere."

28021, 13 "using a fixed factor." What is this fixed factor? Drop this clause. Define later in the page after the factor appears in the equation.

Response: Agreed. The beginning of the paragraph is changed. A reference is included to Table 2 where the surface factor is better explained (as mentioned above).

28022, 24 It is unclear to me what is meant by the "bank emission factor (Ef)". Please clarify.

Response: The word 'bank' is removed here and later in the text. In the next sentence it is made clear how the emission factor is used "Future emissions are then estimated directly from future bank sizes by assuming the fractional rate of release from the banks (emission factor) for each ODS remains the same as it has been when averaged over the previous ten years (1999-2008)."

28022, 2-3, Where do the 1.07 and 1.16 values come from? I couldn't find it in WMO (2011).

Response: See above.

28022, 11 "Future emissions are estimated from a scenario of future production" Define what you mean by the scenario of future production.

Response: The following is added to the sentence "...following the maximum allowed production under the Montreal Protocol (in combination with an extrapolation of observed trends for a few years for HCFCs) ...".

28027, 26 This 0.9 factor is not justified. Please add some text as to why such a number is appropriate.

Response: A correlation factor of 0.9 is assumed in the calculations. It follows the conclusion in chapter 3 of the SPARC report that dominant loss processes are of the ODSs are reactions with OH for hydrogen containing species and photolysis in the stratosphere for CFCs, CCl₄ and the halons. From Table 3.8 of the SPARC report it can be calculated that the uncertainty in the O₂ and O₃ photolysis rates is about 2/3 of the total uncertainty for the most relevant ODSs. This corresponds to a correlation coefficient in Eq. 3 of about 0.9. Table 3 of the manuscript shows that this exact value of the correlation coefficient is a very sensitive parameter for the EESC calculation. The following sentence is added to the text "This roughly corresponds with the analysis in SPARC (2013) of the different contributions to the uncertainty in the total ODS lifetime".

28036, 18 "ODSs do not only ..." to "ODSs not only ..."

Response: Corrected

28040, 18. Is this equation necessary?

Response: Yes. Although it is straightforward, it has never, to our knowledge, been written down in a paper. It is also important as reference for the uncertainties in R_{CO_2} and I_{CO_2} and to show that the uncertainty depends on the absolute value of the lifetime.

28041, 27 Don't use CI for confidence limit. Everyone thinks CI means chlorine.

Response: Agreed. CI is replaced by 'confidence interval' everywhere.

28041, 25. Error "... probably based on s statement in IPCC (1995) ..."

Response: Corrected

28041, 23. A longish paragraph that basically points out the IPCC doesn't do GWP uncertainty correctly. I would shorten this para considerably. You've done the estimate to a much better degree. Just say that.

Response: Agreed. The start of this paragraph is changed and just states what was used in IPCC(2007).

28042, 17. A very weak opening paragraph to the Conclusions. I suggest, "A new ODS scenario (with uncertainties) has been derived ..."

Response: Agreed. The start of the section has been changed accordingly.

28042, 9-10. I would rewrite uncertainties in the paper as: "The GWP weighted emissions (Fig. 9) peaked around 1988 at 9.7 GtCO₂-eq yr⁻¹ with a possible 8.1-11.8 GtCO₂-eq yr⁻¹ range"

Response: Agreed. Sentence is corrected.

28044, 20 There is also an important point here that the lifetimes of all of these gases change as these factors change.

Response: Agreed. The following sentence has been added, "This can be directly or through changes in the lifetimes of the ODSs".

28045, 21. A 100% error for the ODP? Maybe it would be better to quote the actual range. I doubt that we realistically believe that the ODP for any of these Br species could be zero.

Response: Agreed. The ranges in uncertainties are presented more precise.

28045, 22-25. The ODP values in the MP are numbers in a political document that were extracted from the WMO assessments. I would remove commentary on the MP ODPs, and more explicitly point to the ODPs in the older assessments.

Response: We agree that the MP numbers are in a political document, based on previous assessments. The MP numbers are used for the official reporting of production and consumption of ODSs by the parties. Although the MP numbers have not been adjusted after being initially agreed upon, we think it is useful to point out that for several ODSs the newly assessed values differ substantially from those currently used.

28045, 25. Weak sentence to open the paragraph. "Based on our new scenario, we have revised the ODS climate radiative forcing."

Response: Agreed. The sentence has been changed.

28045, 26. I would actually quote the total RF and put in brackets the range.

Response: Agreed. Sentence has been changed.

28051, Table 1. Please add the SPARC (2013) reference to the caption, since the table is largely adapted from SPARC (2013).

Response: Agreed. Caption is adjusted.

28052, Table 2. Please consider adding the “relative” uncertainty contributed by these individual factors.

Response: The table shows the inputs for the calculations. How much these contribute to the uncertainty of EESC is shown in Table 3, Figure 6 and other figures. Putting results in this table could confuse the readers.

28056, Table 4. (a) The fractional uncertainty values can be a bit misleading. For example, Halon-1201 is 96%, but I doubt we would reduce this ODP to near zero. (b) you might consider bolding values that are substantially changed.

Response: We decided not to do this because reader might think the values in bold are statistically different from those of WMO.

28057, Table 5. Bold values that are substantially changed, say more than 10%.

Response: We decided not to do this because reader might think the values in bold are statistically different from those of WMO.

28060, Fig. 3. Drop the thick black lines that “outline” the colors. They tend to obscure the “possible” values. Same with 4, 5, 7, 8, and 9. Leave the thick black line that shows the mean.

Response: Good point. I have removed the outlines for the most likely range to improve the clarity of the figure.

Anonymous Referee #2

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 improve clarity.

Response: It is a very good suggestion to make this point clear. We completely agree with the reviewer that these values are often presented incorrectly as single fixed values. We have added additional text near the beginning of Section 2 to try to clarify this and set the stage for the following discussions. We have added this text to address this general comment as well as two of the more specific comments below.

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).

Response: We understand the last part of the abstract is difficult to understand without some prior knowledge. Discussing details of the uncertainty analysis would make the abstract too technical, though.

To improve the readability the last part of the abstract has been changed to “The primary contribution to these ranges comes from the uncertainty in the lifetimes, with smaller contributions from uncertainties in other modeled parameters. The earlier years of the return estimates derived by the uncertainty analysis, i.e. 2039 for mid-latitudes and 2061 for Antarctic spring, are comparable to a hypothetical scenario in which emissions of ODSs cease in 2014. The later end of the range, i.e. 2064 for mid-latitudes and 2105 for Antarctic spring, can also be obtained by a scenario with an additional emission of about 7 MtCFC-11-eq in 2015, which ...”

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.

Response: See response to referee #2’s general comment.

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.

Response: Agreed. The following has been added here “The uncertainty is small compared with the change in forcing from CO₂ of 0.7-1.8 W m² from 2009 to 2050 from the four RCP scenarios (Meinshausen et al., 2011).”

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 CFC13 that has to find it’s way to the mid stratosphere to be photolyzed.

Response: See response to referee #2’s general comment.

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.

Response: Uncertainties were never reported to the year of return for EESC levels in WMO assessment using the same formulation as done here. We show here that the change in lifetimes gives a change of two years in the return year, which is much smaller than the uncertainty in this parameter. We have altered the relevant sentence to “... is much larger than the shift due to this change in lifetimes.”

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.

Response: You are right. How the numbers are derived and their references are added to Table 2 and we now refer to this table at these two places. Please also see the response to the first major comment from referee #1.

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.

Response: A bank derived solely from cumulative production and emissions has a large uncertainty since it is the result of the difference between two large numbers (see section 2.1). For the year 2008 bank data is available from TEAP, derived by a bottom-up analysis. This bank, which does not depend of the lifetime of the ODS, is considered to have a smaller uncertainty (see Table 2) than one derived only as the difference between cumulative production and emission. Therefore the banks of 2008 are used as a constraint in the scenario. In the projections, no other information is available and the change in the banks are calculated as the difference between production and emission. We have substantially altered this section in an attempt to better clarify our reasoning.

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

Response: Corrected

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?

Response: That is correct. We have changed the sentence to better express this.

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

Response: Agreed. In this sentence we simply state that the different methods used to calculate lifetimes yield different results. The lifetimes from the different methods have been compared and combined in SPARC (2013) to provide uncertainty ranges in lifetimes.

28028 L1 There is some 'order' issue here with OH and the next paragraph.

Response: You are correct. The OH statement is removed here.

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'

Response: Agreed.

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.

Response: You are correct. In WMO(2011) it is stated that the observations do not show a discernible trend from 1964 to 1980. The sentence is changed to "...since model calculations show that ozone depletion must have occurred before 1980".

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'.

Response: Good suggestion. Sentence is changed accordingly.

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?

Response: This depends on the lifetime of the species and also somewhat on the application the species is used in and the corresponding emission factor from the bank. CFC-11, for example, is released slowly by about 4% per year from the bank and has a lifetime of about 50 years. The uncertainty in the bank of 2008 grows to about 7 ppt in a few decades and stays at that level for the remainder of the century. For HCFC-22, lifetime 12 years and emission factor of 16%, the bank uncertainty decreases to almost zero after a few decades.

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'

Response: All of the above are corrected

Anonymous Referee #3

This is a very timely paper on the influence of different parameters to the time of ozone recovery, ODP, and GWP and the uncertainty of these figures. Especially the uncertainty analysis adds to the scientific understanding of these issues. I suggest that the manuscript should be published with minor changes shown below.

28018: L. 5-10: Why do you only specify the time for the mid-latitude and not also for the Antarctic stratosphere?

Response: Good point. I have added the numbers for the Antarctic to the abstract.

28019 L. 21ff: you already could refer to Laube et al., 2013 for an additional data set which provides new information on EESC which will be discussed in the paper.

Response: We would rather not refer to fractional release values in the introduction. The new values from Laube are not very important for the overall uncertainty analysis. Also, I think that fractional release values are too technical for the introduction and would distract the reader from the storyline of the paper.

28019 L.20: Montzka et al., 2011b should be Montzka and Reimann et al., 2011 (then also the 2011a and 2011b partition is no longer needed).

Response: Corrected

28021 L. 12: it is not clear what authors mean with the sentence: In the box model, average surface mixing ratios of ODSs are calculated from mean atmospheric mixing ratios using a fixed factor. Is this factor used for converting mean atmospheric mixing ratios to input concentrations into the box model?

Response: The factor is used to convert the mean atmospheric mixing ratio to the value at the surface as is mentioned a few lines down. The first sentences of this paragraph are changed to avoid the confusion. Also see the response to referee #1's first major comment.

28024: L. 21: be more precise:...emissive? Production (as production for feedstock is still allowed). Or mention the production is used for feedstock and did practically not decline in recent years.

Response: Text added to mention that feedstock production continues.

28025: L. 9. For me it is not logical that you speak of using the information production, mixing ratios, and lifetimes to calculate banks. Thus, one consequence of this should be that the bank size should have changed when the new SPARC lifetimes are used. However, here you speak of using the old information on banks?

Response: A bank derived solely from cumulative production and emissions has a large uncertainty since it is the result of the difference between two large numbers (see section 2.1). For the year 2008 bank data is available from TEAP, derived by a bottom-up analysis. This bank, which does not depend of the lifetime of the ODS, is considered to have a smaller uncertainty than one derived only as the difference between cumulative production and emission. Therefore the banks of 2008 are used as a constraint in the scenario. In the projections the change in the banks are calculated as the difference between production and emission. The text has been changed to better explain this.

28030: L20: For EESC, the most relevant differences in lifetimes between SPARC... It could be mentioned that the stratospheric lifetimes are meant here.

Response: The word 'stratospheric' is added to the next sentence.

28031: L2ff: I understand what the authors mean by increase, but it is a little bit misleading as overall the compounds will have declined by then, but only not as fast as expected with the old lifetime.

Response: Agreed. We changed the sentence using ‘higher mixing ratios’ instead.

28041: L23: Can the newest IPCC report already be cited?

Response: The values can be cited, but the details of the GWP calculations will appear in the Supporting Material which is not published yet. We therefore decided to keep using the radiative efficiencies from WMO(2011), but added the IPCC/AR5 values to the Table. In AR5 new AGWPs of CO₂ are used from Joos et al. (2013). We adopted those values also for our calculations. We also added a few sentences comparing our uncertainties in GWPs with those of AR5 (which are given for CFC-11 and CFC-12).

28042: L16ff: I don’t like the long discussion and conclusion section too much. It is rather long and the take-home messages are a little lost in the discussion. I suggest that the authors try to shorten this section and better separate the discussion from the conclusions or even clearly identified subsections for the different messages.

Response: We agree that the section is too long and have split the section in two (7. Discussion, 8. Conclusions).

28044: L29: ...we have shown that the uncertainty in the lifetime of the ODSs is the dominant term. This statement is too absolute. As for some substances also other factors get important. Maybe use ...“normally” is the dominant term?

Response: Agreed. This sentence should only refer to the uncertainty in total EESC. The sentence is changed accordingly.

28051: Table 1. The combination of the new SPARC stratospheric lifetime for CCl₄ and the oceanic lifetime of 94 years from Yvon-Lewis and Butler (2002) is different from the use as proposed in SPARC 2013, which uses a new value of 81 years, for which however no peer-reviewed reference is available. I am ok with authors using the “old” oceanic lifetime, but it should be stated in the caption of the table.

Response: We retain the value of 94 years as used in WMO (2011), because the value might be changed in the upcoming WMO assessment. This is already stated in footnote a of Table 1. A reference to Yvon-Lewis and Butler (2002) is added here.

28051: Table 2: Lifetimes though → lifetimes through

28064: in the caption colors are not red and orange, but light and dark blue

28064ff: in the caption for figures 7-9: specify that uncertainties are shown as relative uncertainties

Response: All of the above are corrected