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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimates of Ozone Return Dates from Chemistry-Climate Model Initiative Simulations" *by* Sandip Dhomse et al.

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We thank the reviewer#2 very much for his/her insightful comments. These are repeated below in italics, followed by our responses after the '»'.

This paper details ozone return dates from the CCMI model intercomparison. As such, the new estimates of the return dates are the primary new finding of the paper and most of the other results discussed are previously known results. The paper is clearly written and outlining the latest results on ozone return dates is an important task for the upcoming ozone assessment. I have a number of comments but most are relatively minor and thus I recommend publishing the paper after the comments are adequately addressed.

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As stated above most of the results outside of the specific details that are derived from the models simulations were already known. It would be good if you could highlight the new results from this paper in the abstract and conclusion and state more clearly the results that support earlier work.

» We appreciate the comment but this is not a general point that was raised by the other reviewers. While dealing with the other review comments we have rewritten the abstract and conclusions to add content and clarify some points. However, based on the other review comments, we think that the overall main results of the paper are clear. The paper does present up-to-date, state-of-the-art estimates of return dates in support of the WMO/UNEP Ozone Assessment and also (especially with the revisions) discusses the uncertainties in the concept and numerical value of return dates.

It is important to state more prominently that since the models all use mixing ratio boundary conditions an important source of uncertainly has not been included. Thus, the uncertainty given are likely underestimated.

» This is stated in the Introduction, which now includes the following sentence: “This shortening of the ozone recovery was also found by Morgenstern et al. (2018) for the models represented in this study, although it is important to note that the use of surface mixing ratios in studies largely removes the feedback between circulation changes and ODS return dates.”

The resolution of the figures is poor, making them hard to read. This should be fixed in the final versions.

» Yes, it will be. The figures appeared fine in our submitted pdf but the conversion to the ACP pdf (adding the journal information on each page) seemed to degrade the figures.

Page 2, lines 16-18: Are your later return dates within the uncertainly estimates given in the 2014 Ozone Assessment? This would be interesting to add.

» The uncertainty range from our and previous studies (e.g. WMO (2014)) are given in Table 3. It is difficult to compare uncertainty ranges concisely in the abstract so we have added some text indicating that the previous (WMO (2014)) best estimates are often outside the uncertainty ranges that we quote.

Page 2, line 38: Change “also changes ozone return” to “also increases the ozone return” so the reader knows the direction of the change.

» OK. We have changed the word to’lengthens’.

Page 3, line 5: I think you mean tropospheric chlorine and bromine peaked in 1993 and 1997, although I’ll leave it to you to double check.

» Yes, corrected, thank you.

Page 3, line 16: The statement “therefore requires” is unjustified from what comes before. I’m not saying it is does not “require” 3D models, but just that you have not presented evidence that would support the claim.

» OK. We have added some words about the possible changes to polar vortex dynamics to the end of the previous paragraph to better motivate the need for 3-D models (as opposed to just 2-D).

Page 3, lines 47 to end of paragraph: The “faster removal of ODSs” is largely uncaptured by the models here since they have fixed mixing ratio BCs. I say “largely uncaptured” since while the ODS loss term in the stratosphere is affected, it does not affect the surface mixing ratio as it should. This needs to be made clearer since this discussion is likely to mislead the uninformed reader.

» OK, we have added some text to note this.

Related to this effect it would be useful to have a figure of the CFC-11 lifetime (or some other long lived tracer) as a function of time from all the models.

» Unfortunately the information needed for this comparison was not saved by the CCMI

models. However, as the reviewer points out, the use of vmr boundary conditions constrains the models to more similar chlorine distributions more than would be the case for differeing lifetimes. The SPARC lifetime assessment did compile this information from a subset of models used here. The comparison of Cly shown in this paper does depend on the modelled halocarbon lifetimes.

Page 7, line 17: Using the period 1980-1984 is a bit unfortunate since there is significant ozone loss during this period, especially in the Antarctic region. Depending on how you have done the calculation this will bias your return dates to earlier values. Since you have model data starting much earlier I would suggest using 1978-1982 instead, or at least discussing the sensitivity of your results are to this choice. I suspect you made this choice due the availability of SBUV data but it should be possible to derive an adjustment for the data for your figures.

» We don't think that this will bias the return dates. The mean value for the period 1980-84 is used to estimate the adjustment, but the adjusted model values still vary during this period. Also, 1980 is the specific year chosen for the reference date. The fact that ozone loss may occur more strongly later in this period should not matter. In any case, satellite observations are not available for 1978.

Page 7, line 28: I don't see any shading. Is this in reference to figure 1?

» Yes. The shading is very pale and was affected by the conversion to the ACPD pdf. This will be made clearer in the final figure versions.

Page 9, lines 10-35: It is curious that nearly all of the model simulations are below the data. Any idea why? Seems worthy of mention and speculation.

» We have updated the BSVertOzone dataset,which was revised before the submission of Hassler et al. (2018). The lower stratospheric comparisons are improved. We have added some sentences to point out the remaining upper stratospheric difference and to say that we do not think differences in this region will greatly affect column ozone

return dates.

Page 10, lines 1-9: The variation shown on figure 7 at 5 hPa is worrisome, a fact that should be highlighted in the paper. At 5 hPa most of the organic chlorine should be liberated (as can be seen by the similarity of the left and right panels) and thus both the plots should be close to the surface values with a 2-4 year lag to account for the age of air. The peak should be close to the peak in total chlorine in the surface concentrations and the values during the falloff should be very close to the surface values (since a 2-4 year shift is a small change). Thus, the models that are outliers on this plot are evidently not conserving chlorine and their results throughout the paper should be in question. This needs to be stated.

» The models do show a variation in Cly at 5 hPa, but it is not possible from this plot alone to conclude that they do not conserve mass. The differences could be due to incorrect scenarios of the long-lived halocarbons or differences in the treatment of short-lived sources. Nevertheless, we agree that we should not be seeing models with chlorine a lot different to the prescribed scenarios. We have added the sentences: "Moreover, in the upper. . ...of around 2-4 years".

Page 10, line 40: Title should probably be "Sensitivity of ozone return to GHG concentrations and climate change"

» Section 4.5 deals with the impact of the GHGs CH4 and N2O. Section 4.4 is about the effect of climate in general and therefore to distinguish it from 4.5 we would like to keep the title as it is.

Page 10, line 47: The tropospheric impact of CH4 has been pointed out in many papers before Morgenstern et al. 2018, so why chose this reference.

» We have added additional references to the earlier papers of Shindell et al (2009) and Eyring et al (2013b).

Page 11, line 4-6: Actually, the effect of GHG is as comparable in the Antarctic to the

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other regions. It is just harder to see because the scale of the chlorine depletion is so much larger. From you graph, I estimate a 20, 30, 5, and 8 DU change between RCP45 and RCP85 scenarios at 2100 in the 1st four panels. Thus, the Antarctic appears to be second largest instead of “small”. This makes sense since the effect of the GHG is primarily above the ozone hole and thus should be similar. The main complication to this is the increased importance of Cl+CH4 in the ozone hole.

» We agree that the absolute variation is similar (or larger) in the Antarctic (Oct) relative to the other regions/periods but we wish to compare the relative variations. We have modified text to include: “The relative change is smaller in the Antarctic, where recovery is largely determined by Cly loading, but larger in all other regions. However, the absolute changes between, for example, the Antarctic (October) and Arctic (March) are similar”.

Page 11, lines 14-17: You state what is on figure 12 but say nothing of what it tells the reader. Either discuss or remove.

» We understand the reviewer’s point for the first mention of Figure 12. Figure 12 (along with Table 4) is meant to be a summary figure of the discussion surrounding "Figure 11". We have added additional information on how this figure relates to the MMM1S return date for SEN-C2-RCP45 and SEN-C2-RCP85 relative to REF-C2 (Figure 4). This figure is also discussed several times in sections 4.4 (Sensitivity of ozone return to climate change) and 4.5 (Sensitivity of ozone return to methane and nitrous oxide)

Page 12, line 3: Change “chemically inert” to “chemically inert in the troposphere and stratosphere” since CO2 is broken down in the mesosphere and above.

» OK, we have added ‘below about 60km’.

Page 12, line 10-11: Change “most important” to “most important for dynamical changes” or something similar.

» OK, we have added ‘for dynamical processes’.

Page 12, line 48-49: As above I disagree with this statement. It seems comparable to me looking at your plots if one adjusts to the greatly different scales. If you plotted the difference between the scenarios it would be clear.

» The statement above was for Figure 13, not Figure 11. Here eight models were shown with identical y-axis ranges. To make the sentence clearer, we have modified the sentence to read: "In summary, when one examines the relative impact on the ozone return date across the eight models from the four SEN-C2 scenarios, there is not a consistent pattern. Therefore, the result suggests that the Antarctic region is not sensitive to the perturbations presented in this work."

Page 13, lines 39-44: The fact that the global value for the return date is seemingly inconsistent with the different latitude regions implies that it is poorly constrained and you can conclude little to nothing about the effect of N2O changes.

» The effect of N2O on ozone is complicated, but we can still draw conclusions. We have changed the text in question to read:

In this comparison, one would expect that SEN-C2-fN2O with 1960 abundances of N2O would bring forward the SCO recovery date. This is certainly true for the near-global (annual) average comparison, where the MMM1S SEN-C2-fN2O SCO recovery date is shortened by ∼20 years relative to the REF-C2 case. This is mostly due to a shortening of the return date in the tropics; at mid-high latitudes there is little change. As mentioned above the future rise in N2O can lead to significant increases in lower-stratospheric ozone, particularly for regions where the loss rate of ozone due to halogens exceeds that due to NOx prior to the perturbation of N2O. The effect of N2O on ozone varies as a function of latitude and altitude (Wang et al., 2014), complicating the sensitivity to the ozone return date to variations in N2O (Morgenstern et al., 2018).

The new text now includes a citation to the Wang et al. (2014) paper, pointed out by reviewer #1, which had not originally been cited.

Page 14, line 39: Again, it is not the weakest but only less evident.

» OK, we have changed to ‘least evident’.

Page 15, line 7-9: You need to point out there are still serious issues with the chemical and/or transport schemes in some models.

» OK, text has been added.

Page 15, line 19-41: The points made in these final two paragraphs are important and should be made in the abstract as well.

»OK. Some of the points were already in the abstract (e.g. impact of N2O and CH4, also starting with text ‘As noted by previous studies. . .’, and comments on how to use models in future assessments). The points that were not mentioned relate to uncertainties in scenarios and models. We have added sentences on this to the abstract.

Page 28: You should mention the shaded regions in the caption.

» OK, text has been added.

Page 30: Add the abbreviations SCO and TCO to the caption in the correct places to help the reader who doesn’t read the paper.

» OK, done.

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