Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-87-AC3, 2018

Atmospheric Chemistry and Physics Discussions

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

Sandip Dhomse et al.

s.s.dhomse@eeds.ac.uk

Received and published: 5 May 2018

We thank the reviewers very much for their comments. These are repeated below in italics, followed by our responses after the '»'.

Anonymous Referee #1

This work presents a detailed analysis of the ozone return dates from Chemistry-Climate Model Initiative (CCMI) simulations. The authors concluded that there exist strong regional differences in the future trend of total column ozone and its return date. The paper is well written and the results obtained in this study are useful for the research community. I would recommend publication with minor revision. The specific comments are listed below.

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Discussion paper

Page 6, L15: Is a 10-point boxcar smoothing necessary? And whether this smoothing has an impact on the estimates of the return dates?

» We perform the smoothing to remove interannual variability – our aim is to determine a robust estimate of the return date which reflects the average behaviour of the atmosphere rather than on short-term dynamical variability. This point is now made in new text in the Introduction on what we are aiming for with a return date.

Page 7, L27: I cannot see the shading in Fig.1. If have, the shaded region is hardly detectable.

» The shading is there in the original figure but is very pale. It is within about 25 DU of the MMM line. Unfortunately it almost completely disappears in the ACPD pdf conversion. We will aim to improve this in the final figures.

Page 7, L33: 'of the adjusted models' is misleading. It should be adjusted time series.

» OK. We have changed this to 'forecasts provided by the adjusted models'.

Page 8, L2: This conclusion may be right for zonal mean TCO, but sea-ice loss may affect zonally asymmetric TCO trends (Zhang et al., 2018). The authors should give some comments about why there is significant difference between REC-C1 and REFC1SD during the 1990s in the Arctic for the adjusted time series (Fig.2d), which is not seen in the unadjusted series (Fig.2b). Zhang, J., Stratospheric ozone loss over the Eurasian continent induced by the polar vortex shift, 2018, Nature Communications, 9(1):206

» OK. We have added a reference to the Zhang et al. paper. The Arctic REF-C1 v REF-C1SD difference in the 1990s is likely related to the series of cold Arctic winters in this period which resulted in large column ozone loss. The mean of the free-running REF-C1 simulations do not capture this. We have added some text on this.

Page 8, Line 5: The Antarctic and global ozone recovery rate before 2047 in REF-C2 is nearly the same as that in SEN-C2-fGHG (Fig.3). But this feature is not seen for

the other four latitudes. It is understandable that GHG has a little impact on Antarctic ozone; however, it is strange that GHG doesn't affect global mean ozone significantly before 2050. Does the tropical ozone loss cancel the extratropical ozone recovery?

» Please note that the polar regions are not included in the 'near global' mean plotted in Figure 3. In terms of the other regions, yes there is some cancellation of mid-latitudes (fGHG later) and the tropics (fGHG earlier). Please also note that the stratospheric column values are given in Table 4 and this shows similar behaviour, indicating that it is a stratospheric effect.

Page 8, L20-22: Since the decline of the tropical ozone column is mainly due to transport, there should be a corresponding increase in the mid-high latitude TCO. Is it justified that the decline of the global TCO after about 2080 is mostly due to the decline of the tropical TCO?

» The reviewer raises an important point (see also his/her comment below on SCO). Again, please note that the 'global' referred to near-global, i.e. excluding the polar regions where ozone continues to increase through 2100. Figure 9 shows the partial tropospheric ozone column. This does also show a decrease after 2080, by around 2-3 DU at low-mid latitudes, which is similar to the TCO decline. Moreover, the tropospheric decrease is larger in the NH than the SH, similar to the TCO. Therefore, we have clarified the text on this point (and on the SCO below).

Page 8, L35-40: It is interesting that the return dates in this study are all later than those detected from CCMval-1,2 and CMIP5 simulations. Do authors have any comments on this result? Is it resulted from the methodology used in this work?

» We have added a more detailed discussion on this. Factors include the update WMO halocarbon scenario and different climate forcings.

Page 9 L10: The authors should provide more information of the vertical pressure of BSVertOzone. Did you interpolate the observed ozone profile onto the vertical pressure

of CCMs and integrate the modeled and observed partial ozone column using the data at the same pressure levels?

» We have added detail to the paper: “BSVertOzone spans 70 pressure levels that are approximately 1km apart (878.4hPa to 0.046hPa). For the calculation of the partial columns, ozone was interpolated to the exact boundaries of the partial columns from the two closest BSVertOzone pressure levels, and then ozone was integrated between the determined levels. The boundaries for the partial columns were defined as follows: tropospheric column (surface-tropopause), lower stratospheric column (tropopause - 10hPa), upper stratospheric column (10hPa and above; for BSVertOzone this means up to 0.046hPa). The tropopause pressure was defined as 100hPa in the tropics (20S-20N), 150hPa in the mid-latitudes (20-60N/S), and 200hPa in the polar regions (60-90N/S). CCM partial columns were integrated between the same partial column boundaries, but directly from the CCM pressure levels. No additional interpolation of CCM ozone profiles or BSVertOzone profiles was performed.”

Page 9, L13-14: This sentence is hard to understand. Please rephrase.

» We have rewritten this.

Page 9 L36-38 The sentence is fragile. Please rephrase.

» We have rephrased this.

Page 9, L46: isÂż>its

» Corrected.

Page 10 L12 notably to climate Âż>notably by climate change.

» We have modified the text, and split into two sentences.

Page 11, L38-39: Wang et al (2014) pointed out that the effects of N2O increases on the stratospheric ozone are altitude dependent and GHG dependent. Wang W. et al (2014): Stratospheric ozone depletion from future nitrous oxide increase. ACP. 14,

12967-12982, 2014, doi:10.5194/acp-14-12967-2014.

» OK. We have added a new sentence with citation to Wang et al and Revell et al, after the citation to an early Crutzen paper.

Page 12, L20-L39: I suggested that the author could move the discussion regarding GHG to the Section 4.4.

» We understand the suggestion, but have decided to leave the text in place, because Section 4.4 focuses on the effect of climate change on ozone for a single GHG scenario, whereas Section 4.5 explores the sensitivity of future O3 to how CH4 and N2O evolve.

Page 12, L45: Do you mean the red line and black dots for the three models in Fig.13? I don't think they are accurate, but the GEOSCCM and SOCOL simulations are better.

» Apologies. The text was not clear or accurate (and was left over from a previous version of the figure). In fact, many models perform well. The text has been updated.

Page 13 L28,32: SOC->SCO

» Corrected.

Page 13, L36-39: The authors argued (P8, L20-22) that a decrease in tropical ozone column contributes a decline in the global TCO after 2080. Here, the authors suggested that the dynamical transport process has no significant impact on the return date of global TCO. Which argument is correct?

» P8 referred to total column ozone (TCO). This page discusses the stratosphere only (SCO), the difference being the tropospheric column (see Figure 9). We have clarified the earlier TCO discussion to mention the tropospheric contribution.

Page 14 L25: but->by

» Corrected.

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