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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.

Anonymous Referee #1

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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 bellow

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

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

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

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 REF-C1SD 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

Page 8, Line 5: The Antarctic and global ozone recovery rate before 2047 in REF-C2 is nearly the same as that in SEN-C-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?

P8, 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?

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?

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?

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

Page 9ïijŇL36-38ïijŽ The sentence is fragile. Please rephrase.

Page 9, L46: is»>its

Page 10ïijŇ L12ïijŽnotably to climate »>notably by climate change.

Page 11, L38-39: Wang et al (2017) 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

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

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.

Page 13 L28,32: SOC->SCO

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?

Page 14 L25: but->by

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