

Interactive comment on “Diagnosing the radiative and chemical contributions to future changes in tropical column ozone with the UM-UKCA chemistry-climate model” by James Keeble et al.

Anonymous Referee #2

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The manuscript presents an investigation of the model-projected evolution of ozone in the tropics (10S to 10N) over the period 1960 to 2100. A transient simulation covering the whole period and following the specified reference scenario for the Chemistry-Climate Model Initiative (CCMI) model intercomparison project, the REF-C2 simulation, is augmented with time-slice simulations for year 2000 and 2100 conditions run under different levels of ozone depleting substances. The influence of changing greenhouse gases (GHGs) and ozone depleting substances (ODSs) on tropical total column ozone are investigated by splitting the total column into upper stratospheric, lower stratospheric and tropospheric components. Linear functions of the change in column ozone due to the effects of GHGs and ODSs are derived and these functions are used to

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reproduce the evolution of stratospheric ozone column in the full model simulation.

Of significance, the results add to a number of recent papers that underline the importance of ODS-driven changes in ozone on tropical upwelling in the lower stratosphere. The core of the methodology and results presented in the manuscript are, in my opinion, solid. My one significant concern is the way in which the effects of methane and nitrous oxide are treated. On page 13, Lines 1-4, the authors state:

‘In Section 4 we showed that future changes in tropical stratospheric column ozone are driven primarily by changes in: (i) the halogen-catalysed loss; (ii) the strength of tropical upwelling; and (iii) the upper stratospheric cooling induced by GHGs (mainly CO₂).’

I would argue that the authors have not, in fact, shown this in general. The inferred causes of changes in partial column ozone are derived from the set of timeslice experiments that only varied GHGs and ODSs. That these are then the only two factors that were found to be responsible for changes in ozone should naturally follow. On Page 5, Lines 9-11 the authors state ‘In this study we consider the radiative impact of a large number of GHG species (CO₂, CH₄, N₂O, CFCs) and assume that the dominant driver of chemical changes is changes to ODS loadings. In this way, the chemical impact of changing N₂O and CH₄ emissions is not considered here.’ Since the effects of changing N₂O and CH₄ are not considered it seems difficult to justify the conclusion (Page 16, Lines 21-23) that ‘Results from the simple model indicate stratospheric column ozone changes resulting from future CH₄ and N₂O emissions are of second order on the timescales considered here.’

While the parameterization of stratospheric column that is derived here is able to reproduce fairly well the evolution of stratospheric column in the transient simulation, the variation of methane is fairly small in RCP6. The parameterized stratospheric column also significantly overestimates the trend from 2020 to 2100, where the full model shows almost no change while the parameterization projects an increase on the order

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of 5 DU, which could be related to the steadily increasing concentration of nitrous oxide. While the authors have nicely constructed a set of experiments to quantitatively estimate the effects of ODSs and GHGs on tropical ozone, the absence of any methodical investigation of the effects of methane or nitrous oxide would, I believe, rule out making any statements on the importance of these species.

Somewhat related to this point, it would be very helpful to the reader if the authors would state what N₂O and CH₄ concentrations were used for the timeslice experiments. I assume all six of the timeslice experiments used the same specifications for N₂O and CH₄ but it would be helpful to know if this were so and what boundary conditions specifically were used.

Aside from that my other concerns are minor and are specified below.

Page 4, Lines 22-23: Do the two extra ensemble members that start in 1980 use chemical initial conditions from the original two members that were started in 1960? If not, how are the chemical tracers for these two simulations initialized?

Page 5, Lines 2-3: In Table 1 there seems to be an error in the specifications for TS4.5_ODS as that table says climate for RCP8.5 is used.

Page 7, Lines 18-20 states 'These results indicate that over the recent past upper stratospheric ozone depletion resulting from increased Cly concentrations has in part been offset by radiative cooling resulting from increased GHG concentrations, and that in the future both increased GHG concentrations and reduced stratospheric Cly will result in increases in upper stratospheric ozone concentrations.' A very applicable reference to earlier work on this point would be Shepherd and Jonsson, On the attribution of stratospheric ozone and temperature changes to changes in ozone-depleting substances and well-mixed greenhouse gases, *Atmos. Chem. Phys.*, 8, 1435-1444, 2008.

Page 8, Lines 8-10: 'As was seen for the upper stratosphere, the PCO₃_LS response to a given decrease in ODS is dependent on the GHG concentration, (+7 DU for

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TS2000_ODS - TS2000, +6 DU for TS4.5_ODS - TS4.5 and +4 DU for TS8.5_ODS - TS8.5).' Do you have any explanation for the variations in the response to ODSs across the GHG concentrations?

Page 9, Line 7. Here in reference to Figure 3 the amount of ODSs in the atmosphere is indicated by EESC. Traditionally Equivalent Effective Stratospheric Chlorine has been defined in a very particular way using tropospheric concentrations, age of air and release factors for the decomposition of the ODS compound. Given the way the trace of EESC on Figure 3 looks, I think you would want to refer to Equivalent Stratospheric Chlorine (ESC). Have a look at Eyring et al., Multi-model assessment of stratospheric ozone return dates and ozone recovery in CCMVal-2 models, *Atmos. Chem. Phys.*, 10, 9451-9472, 2010, for an example. You should also quote what value of alpha, the enhancement factor for bromine, you have used.

Page 12, Lines 12-13. The statement 'The largest rate of change for tropospheric column ozone occurs over the recent past (1960-2000) (Figure 2c), when increases in anthropogenic NO_x emissions (Lamarque et al., 2010) drive increases in ozone production.' A minor point, but I do not think you can rule out the increase in methane over 1960-2000 as contributing. Methane in 1850 was ~800 ppbv, in 1960 it was 1250 and in 2000 it was 1750 ppbv. About one-half of the total increase occurred between 1960 and 2000 and results from ACCMIP (e.g. Young et al., Pre-industrial to end 21st century projections of tropospheric ozone..., *Atmos. Chem. Phys.*, 13, 2063-2090, 2013) show that the methane increase does account for a good portion of the total increase between 1850 and 2000.

Page 14, Lines 6 and 7: I had trouble reading 'These scenarios include RCP4.5, RCP8.5, RCP6.0 using ODS fixed at 1960 values and RCP6.0 using CDE fixed at 1960 values.' It took a bit of rereading and looking at Figure 7 to understand that not all of RCP4.5, RCP8.5 and RCP6.0 were run using ODS fixed at 1960 values. Is it possible to reword a bit.

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Page 14 Lines 6 and 7: The RCP4.5 and 8.5 results from the parameterization could be compared with Figure 6 of Eyring et al., Long-term ozone changes and associated climate impacts in CMIP5 simulations, *J. Geophys. Res.*, 118, 5029-5060, 2013. They show that going towards 2100, it is actually RCP6 that has the lowest stratospheric column ozone while RCP8.5 is slightly higher. Not to beat on this point too much, but I think the different relative order shown by your parameterization may be due to ignoring the effects of CH₄. Of course, it is a different set of models compared with your parameterization derived from UM-UKCA and that cannot be ignored either.

Page 32 – Figure 6. I may have missed it, but I did not find any discussion of Figure 6 in the text.

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