

Interactive comment on “Chemical and climatic drivers of radiative forcing due to changes in stratospheric and tropospheric ozone over the 21st century” by Antara Banerjee et al.

Anonymous Referee #1

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I judge this to be well-written and original paper on an important issue, which represents a significant advance in understanding of the future drivers of ozone change in both the troposphere and stratosphere. I recommend acceptance after relatively minor modification.

My more important comments are indicated with a M

1:12 “tropospheric ozone precursor” – this is ambiguous, as it needs to be made clear this excludes methane (the ambiguity is emphasised by line 1:26 referring to methane as a tropospheric ozone precursor, and it also being a important result in this paper that methane is a stratospheric ozone precursor)

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1:14, 2:5, 11:1 and elsewhere: The paper would be helped if it could be made clear when (for example) increases due to strat-trop exchange are due to there being more ozone to transport, rather than more advection doing the transport. Perhaps a terminology could be proposed that distinguishes the two?

2:7-8 It is unclear (and indeed it may be unclear in Myhre et al.) whether the forcings on line 1:30 assign all the ODS forcing to stratospheric ozone and all the ozone pre-cursor forcing to tropospheric ozone. I feel that one important result in this paper is that there may be a need for some better terminology to capture these effects.

2:20 and in addition, the role of NO_x in forming nitrate aerosols (see e.g. Myhre et al)

M3:3 -3:16 I feel there needs to be more of a discussion about what is left out. It seems no aerosol forcing is included in the simulations (at least, it is not mentioned) and a more major issue that emerges later is that the authors have had to make a methodological choice – most notably the methane perturbations calculations are performed at present-day ODS concentrations, which might significantly impact the results. Although this is flagged later in the paper, I feel it is a major restriction that needs raising earlier, and returning to in the conclusions.

M4:1-2 “surface concentrations”. I struggled to understand this. If, in the ODS and CH₄ experiments, it is the surface concentrations that are perturbed, does this mean that the perturbation has then to propagate through the atmosphere by advection? If this is the case, given the age of air in the stratosphere is several years, a 10-year integration (line 4-13) is hardly long enough for the perturbation to impose itself (especially as the results seem to be averaged over this 10 year period). I feel sure I am misunderstanding here, and some improved clarity should help.

4:20 Stevenson et al. (2013) indicate that the ozone radiative forcing is significantly dependent on the spectral file used in the Edwards and Slingo code. Since this radiative forcing plays such an important role in this paper, it would be good practice, perhaps in the Supplementary, to be specific as to what spectral file is used here. There may be

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further details of version numbers in the UM-UKCA that could be usefully documented at the same time

Table 1: Somewhere it may be good to spell out what makes up the WMGHGs (again in the Supplementary?). Some/all of the ODS are part of this? And in ΔO_3^{pre} , is the biomass burning assumed to be non-anthropogenic, as that is the implication of the label.

6-1: Since only adjusted forcings are presented (which is perfectly fine) it may be worth a note that some of the adjusted LW forcing is due to the SW-driven temperature changes – so the separation between SW and LW is not always a completely clean one.

6-7: “all” – this does not seem to be the case for dCH_4 according to the table.

6-7: although not essential, adding the total column ozone change would be useful for this table.

6-14: Without going to the other paper, it is not clear what the equivalence is. Is it forcing equivalence, or stratospheric- temperature-change equivalence?

9-11: A minor point, but the “which is driven” part of this sentence might be better at the end of the sentence on line 7, where the ozone reduction is first mentioned (it would also shorten this long sentence).

10-21 This sentence implies that all halocarbons are ODS's (as otherwise what is the point of comparing them?). I might guess that a significant fraction of the 2000-2100 halocarbon forcing is from non-ODSs.

11:3 “0.03” – the table says 0.02

11:18-20 I was not sure what the logic of adding ODS and dO_3^{pre} (but excluding CH_4) was. What point was trying to be made?

11:32 (and 1: 16) A minor query about the “a third” – in the table it is (0.05/0.19) nearer

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a quarter, although the third may be consistent with the fraction prior to rounding.

M12:15-17 As noted above, this is a major caveat which I think requires more flagging earlier in the paper and in the conclusions. It might help the discussion if it could be stated clearly how different the chlorine loading is between 2000 and 2100.

14:16-21 It is worth adding that this estimate of the methane effect is without the climate-change induced component of the ozone change resulting from CH_4 increase (which I guess may be more like the $dCC4.5$ case, as methane won't strongly impact on upper stratospheric temperatures) and so the methane component could be even larger.

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