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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric ozone change and related climate impacts over 1850–2100 as modelled by the ACCMIP ensemble" *by* F. Iglesias-Suarez et al.

Anonymous Referee #1

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The manuscript presents, what could be considered, an overdue analysis of the stratospheric component of the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP) model simulations. While much analysis has appeared in the literature on the tropospheric changes in the ACCMIP models, if the chemistry-climate modelling community is to move towards a comprehensive modelling capability covering both the troposphere and stratosphere we cannot forget about either one of these domains. This analysis is quite welcome.

My one significant concern is the loose way in which the authors have broadly applied the term 'GHG concentrations', referring to the degree of radiative forcing or global

warming, to explain certain differences in the response of ozone. The term 'GHG concentrations' appears in numerous places through Section 3.2 and is used as a general term to distinguish between the changes projected in RCP2.6 and RCP8.5, the two RCP scenarios investigated here. Yet it is critical when assessing the response of ozone to keep in mind that RCP8.5 is not just a scenario with a large increase in tropospheric radiative forcing by 2100, but it is also the only RCP scenario with a large future increase in methane. The problem is first apparent in the Abstract, Page 25176, Lines 21-24 (and discussed further in the body of the article on Page 25193, lines 16-22) with the following:

'Future TCO changes in the tropics are mainly determined by the upper stratospheric ozone sensitivity to GHG concentrations, due to a large compensation between tropospheric and lower stratospheric column ozone changes in the two RCP scenarios.'

The finding of the 'large compensation between tropospheric and lower stratospheric column ozone changes' is largely a result of the particular scenarios that have been investigated. The RCP2.6 scenario has weaker global warming and, one assumes, a weaker increase in tropical upwelling associated with the acceleration of the Brewer-Dobson circulation (BDC) along with a correspondingly weaker decrease in lower stratospheric ozone. The RCP8.5 scenario would have a much larger climate change signal which results in a much larger decrease in lower stratospheric ozone due to the increase in tropical upwelling, as can be seen in Figure 5(i). But the compensating increase in tropospheric ozone in RCP8.5 is most certainly largely due to the increase in methane specified for RCP8.5 at 2100 and less the result of an increase in GHG concentrations in general. If one were to investigate RCP6, would one find that future TCO changes are determined by the upper stratospheric sensitivity to GHG concentrations due to a cancelling of changes between the lower stratosphere and the troposphere? As written, the statement is correct – for the two particular RCP scenarios analyzed here – but more care must be taken that the proper caveats are applied on more general statements.

I have a related concern about the discussion of mid-latitude ozone changes discussed in Lines 25-29 of Page 25193, where the response of ozone in the lower stratosphere is '...positively correlated to GHG concentrations'. By only analyzing RCP2.6 and RCP8.5 it is not possible to separate the effects of a general climate change-associated response (here, an increase in transport of ozone from the tropics to mid-latitudes due to an acceleration of the BDC) from the photochemical effects associated with the increased methane particular to RCP8.5. Randeniya et al. (2002), Fleming et al. (2011) and Reader et al. (2013) have all shown the importance of methane to ozone in the extra-tropical lower stratosphere and it is likely that part of the lower stratospheric response of ozone is also due to the increase in methane and not purely a result of climate change. I would urge the authors to be careful about generalizing a response to 'GHG concentrations' when only analysing two RCP scenarios.

Other minor concerns are given below.

Page 25178, Lines 19-22. In the discussion of the effects of CO2-cooling on upper stratospheric ozone you should not ignore the straight-forward effects of temperature on oxygen chemistry. A large fraction of the response of upper stratospheric ozone to cooling has been attributed to the temperature dependence of O+O2+M -> O3 +M.

Page 25179, Line 13. Plural 'columns' in 'Recent past stratospheric columns ozone...'

Page 25181, Line 2. The authors introduce the idea that the SAM trend is not solely the result of ozone depletion by stating 'as it opposes the effect of increasing GHG concentrations.' without introducing the idea and referencing work that suggests the SAM is affected by both ozone trends and GHGs. A complete discussion of SAM trends does require mentioning the effect of GHGs.

Page 25184, Lines 17-20. It is stated that two models (CESM-CAM superfast and MIROC-CHEM) submitted time-slice simulations yet introduced ODSs into these simulations as an emission. Since in 1980 the atmospheric abundance of ODSs was very far from being in steady-state with emissions, evidenced by how rapidly the tropo-

spheric concentration of these species was increasing, how did these models ensure that the atmospheric concentration of ODSs is realistic for 1980 conditions? Since the authors of the present manuscript are not responsible for how these simulations were setup, perhaps a fairer question is to ask if the halogen loading in these simulations is realistic for 1980 conditions.

Page 25187, Lines 20-26. I think there should be discussion of the complication of calculating 1980-2000 trends from timeslice experiments for 1980 and 2000 conditions. My motivation here is that in 1980 the tropospheric concentrations of many ODSs is rapidly increasing. Given the 3 to 5 year lag for transport into the stratosphere, running for 10 years with constant 1980 tropospheric concentrations will produce stratospheric halogen loading that is more like that found around 1985, assuming a few years additional simulation were discarded to allow the model to properly spin up. I imagine the effect on trends is not large, but there should be discussion of this consideration.

Page 25188, Lines 23-24. Here it is stated that Tier 1.4 of the BDBP ozone database is based on a regression model to the original observations. Are all terms of the regression conserved when deriving the trends that are used for the comparisons shown in Figure 2?

Page 25188, Line 28. It might help the reader transition from the previous introduction of Figure 2 to the reference to Figure 1b by stating that you are discussing total column ozone in the tropics.

Page 25189, Lines 10-13. The statement 'ACCMIP models fail to represent observed ozone depletion occurring in the lower and middle stratosphere region, which may be linked to a poor representation of the HOx and NOx catalytic loss cycles (e.g. Lary, 1997; Nedoluha et al., 2015)' seems to be a significant bit of speculation. One should certainly be suspicious of the HOx chemistry, as it is dominant in the lower stratosphere, but can one rule out problems with trends in transport? And the reference to Nedoluha et al. (2015) seems out of place as they discuss trends in ozone around 10

hPa, where the models are not doing too badly. Further, the Nedoluha et al. findings of NOx effects on ozone trends was explained as being due to trends, secular or transient it is not clear, in N2O transport where N2O is the source gas for reactive nitrogen.

Page 25190, Lines 4-5. The use of the term 'low biased' is not as clear as it should be in 'the ACCMIP multi-model mean is low biased compared to the BDBP data (Fig 2e).' The models generally underestimate the large negative trends in the BDBP data and the use of 'low bias' could mean that the models are more negative.

Page 25190, Lines 22-25. Here the authors state 'ACCMIP models show fairly good agreement with BDBP Tier 1.4 decadal trends at various altitude regions, except around 70–30 hPa, likely linked to NOx ozone loss chemistry associated to stronger temperature trends than observed (see Sect. 5).' Assuming that much of the reactive nitrogen is sequestered in PSCs or has been removed by sedimentation of PSCs during at least the early part of SON shown in Figure 1f, the authors should explain more completely their thinking behind how NOx ozone loss chemistry can explain the underestimated ozone loss.

Page 25198, Line15. I'll admit to always feeling on thin ice when discussing statistics, so if I am mistaken please accept my apologies. Why did the authors choose to use a paired sample Student's t-test when testing the significance of changes in the SAM index across the different experiments? My understanding is that a paired sample requires matched pairs within each of the populations being compared. Here, since the individual years in each of the experiments are completely independent isn't the independent samples t-test the appropriate one?

Page 25199, Lines 11-13. In discussing the large spread of model projected changes for RCP8.5 at 2100 the authors state that the spread is '...likely linked to sensitivity of ozone to future GHG emissions uncertainty (i.e. various direct and indirect processes affecting ozone amounts in the troposphere and the stratosphere).' It is a really minor point, but GHG emissions uncertainty is usually referenced in discussing the spread

across scenarios. Here, all models used the same RCP8.5 specified emissions and the spread in model responses arises as models respond differently to these large forcings. The text in the parentheses, I think is a good description of the cause but it is not the same as future GHG emissions uncertainty.

Page 25201, Lines 26-28. The effect of prescribing ozone or having interactive ozone and the role of zonal asymmetry was discussed in general terms on pages 25199-25200, but here a direct link between the different dynamical responses of the CHEM and NOCHEM models is attributed to the specification of zonally symmetric ozone in the NOCHEM models. It has been discussed earlier in the article that the SPARC/IGAC CMIP5 ozone database underestimated Antarctic ozone depletion and that the NOCHEM ACCMIP models show less ozone depletion than the CHEM models. Given the many different factors that may have affected the comparison of the CHEM and NOCHEM models, can the authors conclude that the use of zonally symmetric ozone is the cause of the differences they find?

References:

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Randeniya, L. K, P. F. Vohralik and I. C. Plumb, Stratospheric ozone depletion at northern mid latitudes in the 21st century: The importance of future concentrations of greenhouse gases nitrous oxide and methane, Geophys. Res. Lett., 29, 1051, doi:10.1029/2001GL014295, 2002.

Reader et al., Contributions to twentieth century total column ozone change from halocarbons, tropospheric ozone precursors, and climate change, Geophys. Res. Lett., 40, 6276-6281, doi:10.1002/2013GL057776, 2013.

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