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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 #2

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Summary: The authors assess ozone and climate change in the ACCMIP, CMIP5, and CCMVal-2 multi-model ensembles, both in the stratosphere where ozone depletion is associated with considerable climate change in the Southern Hemisphere, and in the troposphere where ozone is an air pollutant and climate change drives changes in STE and other tropospheric trends which may influence ozone levels.

The paper's main strength is the large amount of model information ingested into the study. The authors use data from three recent multi-model intercomparison projects. The analyses themselves are straightforward. I don't have any major issues with the diagnostics except that an analysis of total column ozone should exclude models with

prescribed stratospheric ozone. Also the analysis of stratospheric temperature trends should distinguish between model with and without stratospheric ozone chemistry. The latter group presumably would show less inter-model variability than the former. I'm also not sure about the paper's final conclusion that analyses of the last decade have comprehensively demonstrated that there are benefits in interactively coupling ozone and climate. These points amount to a minor revision. The paper provides the type of summary information which is likely to be of use in the 6th Assessment Report of IPCC, the Tropospheric Ozone Assessment (TOAR) and the 2018 WMO Ozone Assessment. Thus the paper needs to progress to ACP after my detailed concerns, given below, are addressed. The language is generally adequate; in a few places, there are minor grammatical or stylistic issues that further proof-reading by a native speaker would help address.

Details: P25183L19f: Are you sure HadGEM2 uses a look-up table approach which differs from UM-CAM? The models are of the same heritage. In the troposphere, HadGEM2 uses the same look-up table approach as UM-CAM (i.e. ozone column does not enter the calculation). In the stratosphere, possibly a different type of look-up table was used which depends on ozone column, pressure, temperature, and solar zenith angle. But this does not directly influence tropospheric rates. Please confirm with the HadGEM2 PIs that this is correct.

P25188L11: This is a strange formulation. The IGAC/SPARC dataset is based on observations?

P25189L11: How many ACCMIP models actually had comprehensive stratospheric chemistry? My impression was that most used prescribed or simplified ozone in the stratosphere.

P25189L16: You give a trend as "-1.64–2.45±1.2%/dec". The notation is strange. I suggest to replace this with a central estimate followed by the lower and upper (2.5% and 97.5% confidence) bounds, or for symmetric bounds the central estimate ±its

uncertainty range at the 95% confidence interval. This also applies to the notations used in various other places in the text.

P25189L17: It's no surprise that the NOCHEM models produce better agreement with obs than the CHEM models – aren't they constrained with observations? Is this a fair comparison? The unsuspecting reader might conclude that adding interactive chemistry to a model is counterproductive. . .

P25190L2: This is more than could be said for CCMVal2. I suspect that this is again because quite a few ACCMIP models prescribe ozone, hence this is as expected. You should exclude from the TCO analyses models that use prescribed stratospheric ozone.

P25190L17: The notation for the trend is strange, see above.

P25191L6: ". . . tropospheric ozone columns" (word order)

P25191L25:"the magnitude depending on region"

P25195L12: Morgenstern et al. (2010) did not consider the NAM. You mean

Morgenstern et al. (2010), Anthropogenic forcing of the Northern Annular Mode in CCMVal-2 models, JGR, 115, D00M03, doi:10.1029/2009JD013347.

P25195L18f: Slightly strange sentence structure.

P25196L6: "simulate" (plural)

P25197L12: The word "temperature" is missing.

P25198L6: The MMM is within the uncertainty estimates of the observations, so at the 95% confidence interval there is no disagreement.

P25198L17: ". . .but are not significant for the RCP8.5. . ."

P25200L18: Replace ",which" for "and"

P25200L25ff: As alluded to above, this analysis only makes sense if you restrict it to models that interactively calculate stratospheric ozone.

P25202L9: I don't think you have established "additivity" or linearity here. How about "While in the recent past both ozone depletion and increasing GHGs have favoured a strengthening of the SAM during summer, under projected ozone recovery they will drive the SAM into opposite directions" or so.

P25202L20: I don't think the leading effect is due to the representation of ozone (interactive or prescribed). My impression is that the main problem is that if ozone is prescribed, it can be inconsistent with the applied GHG and ODS forcing, which can skew the climate change signal due to changing GHGs. Your paper does not address the pure question of exactly what the differences are just due to the representation of ozone. Son et al., JGR, 2010, assess whether interactive (CCMVal-2) models behave differently from CMIP3 models, for some climate indices. The answer is, no, for these indices and models.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25175, 2015.