Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-705-RC2, 2020

Atmospheric Chemistry and Physics
Discussions
Open Access

Interactive comment on "Effects of prescribed CMIP6 ozone on simulating the Southern Hemisphere atmospheric circulation response to ozone depletion" *by* Ioana Ivanciu et al.

Anonymous Referee #3

Received and published: 30 November 2020

The paper reports on the impact of ozone depletion and greenhouse gases on atmospheric circulation trends in the Southern Hemisphere in the new FOCI model. In addition, the paper also documents the impact of prescribing (rather than simulating) an ozone field on the effects of ozone depletion, by comparing ensembles with interactive ozone against ensembles using the CMIP6 ozone forcing. The authors conclude that FOCI captures the effects of ozone depletion and GHGs on the circulation. In addition, they also conclude that prescribing ozone rather than simulating one interactively leads to a weaker tropospheric response to ozone depletion. Based on these results, the paper claims that climate models prescribing CMIP6 ozone will underestimate the historical ozone-induced dynamical changes in the Southern Hemisphere.

Printer-friendly version

Discussion paper

The subject of the paper is of relevance and interest for the readership at Atmospheric Chemistry and Physics. The paper is well written and the analysis is detailed and nicely discussed. However, some of the evidence provided is not convincing and as a result, some of the implications of this paper (models without interactive chemistry underestimating the effects of ozone depletion) are over-stated, especially concerning the limitations of imposing a prescribed ozone in historical simulations. The authors need to provide more convincing evidence to support some of the claims, or substantially revise some of them (perhaps tone them down!). Hence, I recommend major revisions, as detailed below.

MAJOR ISSUES:

1) This paper does not really provide a clean isolation of the 'ozone feedback'. Ozone from CMIP6 is substantially different from the ozone simulated by FOCI, as shown in Fig.7. This leads to a systematic bias in the 'CHEM OFF' experiments, as discussed in section 4.1. In addition to differences in climatological values, trends in the prescribed and interactive ozone are also different. Hence, any effects on the variability/trends are a result of these differences, rather than a missing 'ozone-radiative-chemical feedback' in the CHEM OFF ensemble (e.g., L16, L607). If the authors wish to quantify the ozone-chemical feedback, they should compare ensembles using the interactive ozone vs ensembles imposing a (time-varying) ozone, derived from the same model system (FOCI), rather than from other models (CMIP6).

2) The comparison with observations is missing. The paper claims in several instances that the FOCI model 'reliably' captures the effects of ozone depletion (e.g., in the Abstract on L17) and that in simulations with interactive chemistry, the effects of ozone depletion are stronger and closer to the observations (e.g. in the Conclusions near L610). However, this comparison with observations is lacking, as no single observational data-set is shown in the paper, using the same analysis period & statistical method. The authors should directly compare their ensembles (especially REF and CHEM ON) against observations, at least for some of the large-scale circulation met-

rics, such as zonal mean zonal wind, temperature, near-surface wind (850 hPa), to build confidence in some of their claims regarding the model's skills in capturing observed trends and regarding the increased 'realism' in the simulations with interactive chemistry. I suggest using ERA5 or some other high-quality re-analysis product for this purpose.

3) Inappropriate time-period chosen to analyze the impact of ozone depletion. The authors use the 1958-2013 period to calculate trends in their historical simulations, and they derive the impact of the ODS by taking differences between noODS and REF ensembles (REF is presumably the same as the CHEM ON...??). ODS emissions were phased out in the mid 90s and as a result, ozone depletion trends stopped near the year 2000. Since the beginning of the 21st century, we have already seen the emergence of 'healing' in the ozone layer (Solomon et al., 2016). Recently, it has also been shown that this resulted in a change in the tropospheric circulation trends in the SH (Banerjee et al., 2020). Hence, the trends calculated in this paper do not properly isolate the effects of ozone depletion, as the trends before and after 2000 are probably very different. I would strongly recommend choosing an earlier end-date in the analysis of trends (e.g. early 2000s).

4) No convincing statistics. Aside from direct comparison with observations, we need to make sure differences are really robust across ensemble members. Several studies (e.g., Seviour et al., 2017) have shown how large the variability in the SH can be, and how it can explain differences across transient simulations. Can we make sure the CHEM ON vs OFF differences are really robust in light of this large variability? I would strongly recommend comparing the response to ozone depletion (and most importantly, the CHEM ON vs OFF differences) against the inter-ensemble spread. Ideally, the authors should show the individual members of all ensembles against observations, to give more confidence in the two key statements made by this paper concerning (1) this model reliably reproducing observations in terms of the effects of ozone depletion and (2) its trends being significantly weaker with prescribed (CHEM

OFF) than interactive (CHEM ON) ozone.

5) Lacking discussion of relevant literature. The authors do not sufficiently discuss some key studies, which already looked at role of interactive chemistry in simulating the impact of ozone depletion. One of them was Eyring et al., 2013, which directly compared CMIP5 simulations from CHEM and NOCHEM models. More recently, Seviour et al. (2017) and S. Woo-Son et al. (2018) also extensively analyzed multi-model comparisons (CCMI and CMIP5) in terms of their simulated ozone depletion, and found barely any robust difference between models with and without interactive ozone. These papers should be properly cited and discussed, to provide a more critical and balanced discussion throughout the paper.

6) Unjustified claims regarding underestimation of the effects of ozone depletion in models without interactive chemistry (e.g. see L659). Several papers have shown that actually, imposing or simulating the ozone hole does not make a lot of difference. See for example S.-Woo-Son et al., 2018, as well as Eyring et al., 2013. While it's true that there inter-comparison studies do not cleanly isolate the impact of interactive chemistry alone, they do not see any systematic difference between both class of models (CHEM or NOCHEM), and they span over a wider range of uncertainty, since they look at many different models rather than a single model, as done in this paper. Hence, the implications of this study may be smaller than stated in the paper (e.g. the statement in L21-23 in the Abstract). Moreover, this paper does not properly compare any of the FOCI trends with observations (major comment 2), nor cleanly isolates the importance of interactive chemistry (major comment 1). Hence, the claims about CMIP6 models underestimating the historical ozone-induced changes in the SH are unjustified.

SPECIFIC ISSUES:

L16 "missing ozone radiative dynamical feedbacks" - see major comment 1: The CHEM ON vs OFF comparison rather quantifies the impact of a systematic bias, rather than a true feedback (which could only be quantified by comparing another CHEM OFF ensemble driven with the ozone forcing from CHEM ON).

L21-23 In light of the discussion given above (points 1, 5-6), I frankly do not find this statement very convincing.

L65 Oehrlein et al. (2020) is another recent relevant paper that studied this problem, as they compare CHEM ON vs OFF experiments strictly having the same ozone climatology. I suggest adding this paper to the reference list.

L83-85 Oehrlein et al., 2020 also explored this. They show that in time-slice simulations with constant forcing, the effect of interactive chemistry on SSWs frequency is not statistically significant. Adding this paper could help making the point about the lack of robustness across different studies on this.

L93 there were also papers showing the contrary, i.e. that models with/without interactive chemistry were very similar in their simulated trends. One paper showing this was Seok-Woo Son et al., 2018. This paper should be cited and discussed.

L114-118 Oehrlein et al., 2020 also studied this problem, using time-slice rather than transient simulations, partly confirming some of the results of Haase and Matthes (2019) but also refuting some others (e.g. the influence on SSW frequency), so I recommend citing this paper here, too.

L125-140 Another problem which is not discussed at all is the vertical interpolation. Interpolating the ozone forcing from CMIP6 which is provided on pressure levels on FOCI's own z-levels may create errors, which can be non-negligible near the tropopause. This can create problems with radiative transfer, as e.g. discussed in Hardimann et al., 2019. Have the authors tested whether this happens, too?

L139 "correctly simulate the effects of ozone depletion" - the authors do not show any observations in this paper. Hence, we cannot really determine whether the REF ensemble (which is the same as CHEM ON I guess?) is really close to observations and whether CHEM OFF is systematically off. I strongly suggest adding one such analysis. This could be, e.g. by adding trends in jet-latitude or SAM trends and show individual ensembles vs observations, as done e.g. in Seviour et al., 2017.

L160-180 if FOCI by default uses interactive chemistry in the REF experiment, then what is the difference between this ensemble and the CHEM ON ensemble?

L210-220 Is these multiple filtering really needed? Are the results for the SAM sensitive to the way the data is filtered? It would be nice if the authors could comment on this.

L231 see major comment (3) concerning the time-period. The authors should explore the sensitivity of these results to the end year chosen, and a shorter period (e.g., 1958-2000) would probably be more appropriate to explore the effects of ozone depletion.

L256 Actually, this problem has been studies in multiple papers, which looked at the upper stratospheric ozone response to large CO2 forcings in detail; e.g. Haigh and Pyle (1982), Jonsson et al. (2004), Chiodo et al. (2018). I suggest adding these papers here.

L262 This effect (GHGs –> polar cap ozone increase due to faster BDC) has been widely studied in the context of climate sensitivity experiments, imposing large CO2 forcings (2x and 4xCO2), such as e.g. Dietmuller et al. (2014); Nowack et al. (2015); Chiodo et al. (2018). Such results also apply here, although the GHG forcing studied in this paper is much smaller.

L287 "realistically capture" - I recommend adding one figure for the observations, so that the reader can appreciate how close REF (or CHEM ON) are to the observational trend. The validation paper for FOCI (Matthes et al., 2020) did not really show trends in the SH circulation, so this should be done in this paper, since the main message is that FOCI is "able to capture" the impacts of ozone depletion.

L366 "adequately simulates" - same comment as on L287

L375 I would strongly recommend changing the labeling (CHEM OFF) to something more descriptive of what is really used here (CMIP6 O3 forcing). How about CHEM

ON vs FIXO3 CMIP6?

L474 "agree better with observed trends" –> this has not really been shown here, so I am not convinced about this statement. To lend confidence on the results and statements like this throughout the paper, the authors would need to show (1) that all three ensemble members are closer to the observations than CHEM OFF and (2) that they are all significantly different from any of the members in CHEM OFF. This also applies to L610 in the conclusions section.

L494 "the feedbacks between ozone..." As stated in my major comment (1), this simulations set-up does not really cleanly isolate the feedback, as the CMIP6 ozone forcing leads to a systematically different basic state. How can we be sure that these differences are rather due to "biases" introduced by the CMIP6 ozone forcing, rather than a true "feedback"?

L511 this is a very far fetched statement, given that some studies in the past have already shown that CMIP5 models with interactive chemistry (CHEM) do not significantly differ from those that impose the historical ozone forcing in terms of the tropospheric trends (e.g. Eyring et al., 2013; Seviour et al., 2017; Seak-Woo-Son et al., 2018... just to name a few!). To show that CMIP6 is different in this sense, the authors would need to use a different set-up and/or use more models. Otherwise, this is an over-statement which is not justified by the evidence provided in this paper. This also applies to L660-662.

L552-556 see major comment on L511, and major comment (1). This also applies to the statements on L606-607.

REFERENCES

Banerjee et al., 2020; A pause in Southern Hemisphere circulation trends due to the Montreal Protocol, https://doi.org/10.1038/s41586-020-2120-4

Chiodo et al., 2018; The response of the ozone layer to quadrupled CO2 concentrations, DOI:10.1175/JCLI-D-17-0492.1

Dietmuller et al., 2014; Interactive ozone induces a negative feedback in CO2 driven climate change simulations, DOI:10.1002/2013JD020575

Haigh and Pyle, 1982; Ozone perturbation in a two dimensional circulation model, DOI: 10.1002/qj.49710845705

Hardimann et al., 2019; The Impact of Prescribed Ozone in Climate Projections Run With HadGEM3 GC3.1, DOI:10.1029/2019MS001714

Jonsson et al., 2004; Doubled CO2 induced cooling in the middle atmosphere: Photochemical analysis of the ozone radiative feedback, DOI:10.1029/2004JD005093

Nowack et al., 2015; A large ozone-circulation feedback and its implications for global warming assessments, DOI:10.1038/nclimate2451

Oehrlein et al., 2020; The effect of interactive ozone chemistry on weak and strong stratospheric polar vortex events, DOI:10.5194/acp-20-10531-2020

Seok-Woo Son et al., 2018; Tropospheric jet response to Antarctic ozone depletion: An update with Chemistry-Climate Model Initiative (CCMI) models, DOI:10.1088/1748-9326/aabf21

Seviour et al., 2017; Robustness of the Simulated Tropospheric Response to Ozone Depletion, DOI:10.1175/JCLI-D-16-0817.1

Solomon et al., 2016; Emergence of healing in the Antarctic ozone layer, DOI: 10.1126/science.aae0061

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-705, 2020.