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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 #1

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Summary: Overall, I find this paper a very enjoyable read. The authors use a new model (FOCI) to address the impact of anthropogenic drivers (increasing GHGs, ozone depletion) on SH climate, and specifically to characterize the role of interactive ozone when interactive chemistry is the only point of difference between two model constellations. The authors have done a good job communicating their story of significant differences in stratospheric and tropospheric dynamics depending on how ozone is handled. I think their methodology is sound and the results are plausible and in large parts backed up by existing literature (although few other works have presented these results with such clarity).

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Apart from a few minor issues detailed below, my only major point of criticism is that the authors should have used the simulations already presented by Haase et al. (2020) as a further line of evidence. In these simulations (only mentioned at the end of the manuscript) the ozone field is taken from a CHEM-ON simulation of FOCI and used at daily resolution to force an offline simulation with FOCI. Using these simulations would address a question that I had reading the manuscript, that various dynamical differences between the CHEM-ON and CHEM-OFF simulations might not have been caused directly by the method of treatment of ozone but rather by possibly substantial differences in the background ozone climatology. Such differences would be minimized in the above comparison; only differences to do with mismatches between the state of ozone and the state of the polar vortex would remain. (A better comparison still might be to take the ozone field from FOCI, filter out interannual variations and use it at monthly resolution, to prescribe ozone in as similar a way as it gets to the CMIP6 climatology, but with systematic ozone differences removed. That would make both ensembles of simulations comparable to the majority of CMIP6 historical simulations that have used the CMIP6 ozone climatology.)

Essentially, adding this simulation ensemble would allow the authors to decompose any differences in trends into contributions due to the background ozone climatology and due to consistency (or not) between ozone and dynamics, which are two quite different explanations that the authors cannot really distinguish between in the paper as it stands. Since the simulation(s) needed for this already exist, I feel this is not an enormously large request to make (although it might make the text longer and the figures more complex).

A further, less fundamental question relates to the treatment of radiatively active gases other than ozone in CHEM-OFF. How do you treat water vapour, methane, and nitrous oxide when chemistry is turned off, in such a way as to minimize differences between the CHEM-OFF and CHEM-ON simulations? Or is chemistry running in all model variants and just a different flavour of ozone is fed into radiation?

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And finally, you only mention CO_2 and methane as ’GHGs’ in the set-up of the ’No-ODS’ simulations. How about N_2O? Is that considered an ODS? Technically it is, but it affects ozone very differently from the halogenated ODSs (see e.g. https://doi.org/10.5194/acp-18-1091-2018). Typically ODSs are taken to be chlorinated and brominated halocarbons controlled by the Montreal Protocol. Please clarify.

Final question: Are you planning to contribute this model to CMIP6, given the large effort put into producing the PI spin-up and the historical simulations?

Minor comments: P2L29: “at destroying ozone"

P2L52: I think it’s controversial whether East Antarctica actually experienced cooling, considering the difficulties with measuring temperature there (distinguishing cloud from ice in IR-measurements, sparsity of ground-based measurements). For a “grey-literature" comment on this see http://www.realclimate.org/index.php/archives/2004/12/antarctic-cooling-global-warming/. It’s more robust to assert that the rate of warming in Antarctica exhibits large regional variations and that a large warming of much of West Antarctica has occurred.

P3L75: I find “zonally oriented asymmetries" confusing. I suggest dropping this phrase and just call them “zonal asymmetries".

P4L127: Worth noting that these were not just “different climate models" but actually two different chemistry-climate models whose results for “historical" ozone were averaged to form the CMIP6 ozone forcing dataset. You want to cite https://doi.org/10.1002/2017GL076770 for this.

P6L1: I infer from “CO_2 emissions" that this model is run using an interactive carbon cycle? Please state in the text if that is the case. Using methane “emissions" is also unusual in this context; typically methane VMRs are prescribed at the surface. “Emissions" are typically only used for tropospheric ozone and aerosol precursors. Also, please state explicitly how N_2O, HFCs, PFCs and other minor greenhouse gases are treated (see above).

P10L298: “indicates"

P13L407ff: I can see westward shifts in CHEM-ON and CHEM-OFF (figure 6), but the difference in the rates of progression is not obviously discernible to me. Could you perhaps think of a way of visualizing this better, and perhaps formalize that the trends are significantly different? You may want to cite https://doi.org/10.5194/acp-17-14075-2017 here.

P16L507: “shifted"

Figure 4 and elsewhere: My inclination is to avoid introducing scaling factors into colour bars (e.g. 10^{-2} m s^{-1}, 10^{-4} m s^{-1}) and just state unscaled units (cm/s, mm/s). This is also done elsewhere but does not improve clarity, I find. Also figure 5, panel a. Why not have the scale range from -0.36 to $+0.36$?

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