Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Analysis of recent lower stratospheric ozone trends in chemistry climate models" *by* Simone Dietmüller et al.

Anonymous Referee #1

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In this study the authors build on previous studies' examination of recent (1998-2018) trends in stratospheric ozone, in particular those spanning the middle and lower stratosphere. The main contribution from this study, which distinguishes it from previous work, is to put the results from the previous single-model and observational studies in the broader context of (several) more models. Through analysis of the ozone trends in a 31-member multi-model ensemble of free-running simulations of the recent past the authors quantify the model uncertainty in ozone trends, placing the observations in that context via construction of a "probability of disagreement" distribution, and test various sensitivities to these trends with respect to the end years of the time series and location in the stratosphere. An analysis of the convergence of the trends into the future is also made as well as a basic analysis ascertaining the extent to which modeled trends in

ozone are related to trends in upwelling. The work is very detailed and rigorous with respect to testing sensitivities, etc. The focus on the latter makes sense in the context of previous studies and given the short time series of the period of focus. At the same time, however, I have same major comments that explain why I have assigned to this study "major revisions" which are as follows:

1. The discussion of a forced signal (driven by GHG vs. ODS changes) presented in Section 4 is somewhat lacking, especially given the facility with which the authors can bring in the results not just of the fGHG simulations that they have analyzed but also the fODS simulations that were also performed for the REF-C2 scenario. Both, for example, were employed in the study by Abalos et al. (2019) cited below. By explicitly looking at these simulations, in addition to the two others considered here, the authors can more quantitatively address the relative roles of ODS vs. GHG (and the linearity between their interactions). This is a reasonable request especially given that ODS themselves can alter the stratospheric circulation (see the impacts on upwelling documented in the second study listed below) and given that these experiments have already been performed.

Abalos, Marta, Clara Orbe, Douglas E. Kinnison, David Plummer, Luke D. Oman, Patrick Jöckel, Olaf Morgenstern et al. "Future trends in stratosphere-to-troposphere transport in CCMI models." Atmospheric Chemistry and Physics 20, no. 11 (2020): 6883-6901.

Abalos, Marta, Lorenzo Polvani, Natalia Calvo, Douglas Kinnison, Felix Ploeger, William Randel, and Susan Solomon. "New Insights on the Impact of Ozone‐Depleting Substances on the Brewer‐Dobson Circulation." Journal of Geophysical Research: Atmospheres 124, no. 5 (2019): 2435-2451.

2. The discussion of the mechanism underlying the different ozone trends is a bit unsatisfying. Of course, most of this derives from focusing on a multi-model comparison for which it is (understandably) difficult to do a detailed budget analysis for each model.

However, the authors have more information than they may realize. In particular, I would strongly encourage the authors to consider analyzing the "age of air" or "e90" tracers that were also carried in these integrations as these provide a description of the actual transport circulation changes simulated in the models (which may, or may not, be directly related to changes in upwelling). The lack of any passive tracer diagnostic is a bit discouraging and I think it's incorporation would add substantially to the discussion.

3. In contrast to the previous sections, I find much of the material in Section 4 to be qualitative and speculative. For example, it is, of course, true that intermodal differences in internal variability (contributed from the QBO and ENSO) can contribute to the spread in trends among the models. However, this is never explicitly shown (only described in generalities) and I think a basic analysis needs to be done by which, for example, the authors select two models with very different ozone trends over midlatitudes and then show their ozone composites with respect to different phases of ENSO and the QBO. How does the ozone variance contributed from these two modes vary across models? Is it large? This would be an easy calculation to do and could be provided as a supplementary figure. Without a more quantitative analysis, though, it is not clear what exactly is gained from this discussion, besides raising issues that have been discussed in previous studies.

4. It appears that one of the main results from this study is, per the conclusions, the fact that "in midlatitudes the observational trends are a rather extreme value of the models' distribution." I agree with the authors that this is an important conclusion and I think this is a nice finding from this study. However, I think the authors need to acknowledge that this was also the conclusion made in Orbe et al. (2020) (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD031631). Specifically, please find in the abstract the statement "while the free‐running model can produce negative ozone changes in the NH LS, the magnitude of these changes is significantly weaker, relative to both M2GMI and MERRA‐2; moreover, these weaker ozone

C3

decreases are consistent with weaker simulated changes in the residual circulation." That study, therefore, arrived at a very similar conclusion to the one obtained in this study, albeit using an ensemble of (single model) simulations and not a multi-model ensemble. I strongly encourage the authors, therefore, to better cite this contribution earlier in the manuscript. In particular, on page 2 in their reference to this study they can append an additional statement along the lines of "…is primarily associated with large-scale advection; furthermore, they showed that the observed changes in advection and ozone are within the range of model variability (gauged from one CCM) but on the extreme end." The conclusion made in Orbe et al. (2020), therefore, is distinct from that in Chipperfield et al. (2018) who suggested that the observed ozone trends were well within the range of simulated variability.

5. Quite a bit of attention is paid to the correlation between tropical ozone trends and midlatitude ozone trends. This is understandable, given that the two are plausibly connected, but Figure 3 does not really seem to support this. The correlation seems very small, no? I think the reader would find this relationship more convincing if the authors showed a figure showing this relationship for, say, a given model. In particular, does this relationship manifest by just considering interannual variability? What does the correlation between midlatitude and tropical ozone look like for individual years within a given model? Without a stronger case it just seems like Figure 3 is exhibiting a very weak relationship. . ..

6. Page 18, Lines 7-26: A lot of ambiguity and potential for intermodel differences is described here as stemming from differences in the latitudinal extent of upwelling/downwelling between models. I certainly agree with this comment. However, there is a very straightforward solution. One could compare w* between models in such a way that accounts for intermodel differences in the turnaround latitudes of the BDC. In particular, it is possible that the fixed lattiude boxes considered here do not span the region of mean downwelling in every model owing to differences in the meridional extent of the BDC. Not accounting for this information, therefore, would lead to the mis-

C4

leading conclusion that the models somehow underestimate downwelling but, actually, this may not be the case since the model may simply have downwelling occurring at different latitudes. What happens when you redo your analysis to be more dynamically consistent in this regard?

In addition to the major comments above I also provided these more minor points:

-Page 6, Line 20: Are different ensemble members treated the same/given the same weight as different models? Shouldn't they be weighted in such a way that distinguishes between ensemble members versus distinct models? Perhaps that is what has been done âĂŤ it is not clear in the present text, however. -Page 7, Line 13: "dynamical linear modeling" needs to be described here. -Page 8, Line 13: The Orbe et al. (2020) study also showed this discrepancy in the LS ozone trend between the observations and the models. -Figure 5: Can you add the observed trends in upwelling as well? This seems important. Of course there may be differences between reanalyses but you can add, for example, estimates from MERRA-2 and ERA-Interim. This should be easy to do as you can use the TEM residual circulation estimates from the SPARC Reanalysis Intercomparison Project (https://s-rip.ees.hokudai.ac.jp/resources/data.html). -Page 24, Line 6: It is not clear to me what the discrepancy is here that you are claiming between the GEOSCCM results presented in that study compared to the ones the authors show in Figure 1. Please explain in more detail. -The language throughout could be improved at various places. I have noted a few grammatical errors below but there are many others. I strongly encourage the main author to have all co-authors check for lingering language issues/typos.

Technical Points:

-Page 1, Line 7: Please indicate a reference for CCMI -Page 2, Line 6: "results from" -> "result from" -Various paragraphs throughout are not indented which renders the formatting a bit awkward (e.g. Page 12, Line 5). Please fix. -Page 12, Line 3: "depending on" -> "dependent on" -Page 12, Line 11: Do you need to remove "not" in front of

significant? This is confusing given that the next sentence implies that the trends are significantly related. -Page 15, Line 3: The sentence starting with "We will show. . ." is not complete. -Page 15, Line 15: "evolve" -> do you mean "simulate"?