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Interactive comment

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

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We thank referee #1 for the constructive comments on our manuscript and for his/her new ideas to improve this work. According to the referee's suggestions we now include some additional analysis to this paper (for details see below). Through these, we gained additional insight into the processes determining the ozone trends in the LS. Due to the new results we also reorganized the structure of the manuscript in the last sections (see new section 3.4 and 3.5). Moreover, note that we do not consider interannual correlations anymore, as we felt that not too much was learned here.

Te revised manuscript considers all questions and comments.

Major issues:

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Discussion paper

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

-> Thank you for this helpful idea. We now included the fODS simulations to Figure 8a and 8b. Note that the ozone MMM trend has slightly changed compared to the last version of Fig. 8, as we now only took the simulations into account that provide ozone for both the fGHG and as well as the fODS simulations. Moreover, we changed the structure of the text, and included a new Section (Sec. 3.5) on the forced trends and their attribution (which was previously a part of the discussion).

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.

-> Thank you for this excellent suggestion. Following your comment, we now included analysis of the intermodel correlations between local ozone trends to AoA trends, and their correlation is indeed very strong in the tropical to mid-latitude lower stratosphere (new Fig. 7a). Besides that, in order to distangle the different transport mechanism, we also include the correlation to residual circulation transit time (RCTT) and Aging by mixing trends (see Fig. 7b+c). The revised Section 3.4 describes the results and the additional insights from those analysis.

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.

-> The reviewer is right, in that the discussion here is only qualitative. However, it is not the scope of our study to investigate the role of natural variability (e.g. QBO or ENSO) for the spread in the LS ozone trends in detail – we only want to discuss our results in the light of what is known from literature. Therefore, this is part of the discussion section. To make the discussion character of section 4 clearer, we included to the text the following sentence: " While it is not easily possible to test which of the above explanations is correct, in the following we will discuss their possible contributions to the diagnosed disagreement in the light of our results and of what is known from literature." (see p. 29, line 8). Moreover, we also added in the discussion paragraph "Representation of natural variability in models", that we leave the assessment of the representation of natural variability and its effects on ozone to future studies (p.32, line1).

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). . . ...

-> Agreed. The Orbe et. al 2020 paper was published just before we submitted our draft, thus we missed to include it at several instances. It is now cited at several places.

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.

-> We agree in that the relationship between tropical and mid-latitude ozone trends is weak. Indeed, as the reviewer stated, there was a certain expectation to find a relation (which is backed up by the inter-annual correlations we showed in the previous version of the manuscript, see old Section 3.5 and in particular old Table 3 that did provide the correlation of inter-annual variability for individual models). Given our new insights into the role of different transport pathways, and the insights from the single-forcing experiments (see new Sec. 3.4 und 3.5), we realized that the expectation of anti-correlation between tropical and extratropical ozone might be misleading. Therefore, we strongly de-emphasized this point throughout the manuscript, including also the removal of old Section 3.5. Instead we focus on the trends and their correlation to transport measures (see above, new Sec. 3.4). Nevertheless, we decided to keep Fig. 3 as it nicely illustrates the mutual distribution of tropical and mid-latitude trends, but revised the text accordingly (see p. 14, lines 6-11).

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 misleading 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? ->Thank you, that's a good point – we were aware of that problem and therefore decided to follow the reviewer's suggestion to define a dynamically consistent mid-latitude box by averaging the LS ozone column from the turnaround latitudes of the BDC to 50°N . We re-calculate the LS ozone trend for this box (see Tab. 2) and moreover we indeed find a stronger inter-model correlation of LS mid-latitude ozone trends to up- and downwelling trends for this dynamical box (for details see Section 3.4 and Fig. 6).

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 but it is not clear in the present text, however.

-> See p.10, line 30: "Note that for the calculation of the MMM trend, we chose to weight all 31 simulations equally (i.e., not considering that some models have multiple ensemble members) because “the trend variations among ensemble members are as large as among the different models over this period “. However later on in Section 3.4 the MMM is now calculated as average of ensemble-means from each model, as for the longer time-periods the forced trends outweight variability, so that the above argument does not hold anymore.

-Page 7, Line 13: “dynamical linear modeling” needs to be described here.

-> We added a brief description of the “dynamical linear modeling” now – see p.9 line 1-4. -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.

-> We include Orbe et al. 2020 to the citations.

-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

-> We now provide the upwelling trends of ERA5 in Figure 2 and Figure 5 and Tab. 2. -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.

->As we restructured the text, we exclude that sentence now.

-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.

-> We revised the language to the best of our capabilities, and took all comments below into account.

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Technical Points:

-Page 1, Line 7: Please indicate a reference for CCMI -> Done.

-Page 2, Line 6: “results from” -> “result from” -> Done.

-Various paragraphs throughout are not indented which renders the formatting a bit awkward (e.g. Page 12, Line 5). Please fix. -> Done.

-Page 12, Line 3: “depending on” -> “dependent on” -> Done.

-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. -> Done.

-Page 15, Line 3: The sentence starting with “We will show. . .” is not complete. -Page 15, Line 15: “evolve” -> do you mean “simulate”? -> Done.

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