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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Ozone database in support of CMIP5 simulations: results and corresponding radiative forcing" *by* I. Cionni et al.

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*We thank the reviewer for the constructive comments. The reviewer has pointed out several suggestions for improving the paper. We have considered carefully each of the comments and have modified the text accordingly. Detailed answers to the reviewer's comments are given below.*

The manuscript presents a detailed description of the construction of a dataset of combined tropospheric and stratospheric ozone for use by GCMs that require an externally specified transient ozone field for CMIP5 simulations. The paper also presents a limited assessment of the present-day ozone distribution and the evolution of ozone over the recent past. Given the state of ozone specified across the CMIP3 models for AR4, such an exercise is to be commended. Further, the difficulties inherent in such an



exercise must be recognized. Only a relatively short historical record of observations of tropospheric ozone exist, with very few observations from before 1950. Even the present-day distribution of ozone can only be derived through the liberal use of interpolation of available observations. The present-day distribution of stratospheric ozone is considerably better known and there is the added advantage that extrapolations to the historical period can be made on the basis of EESC. Although, here as well, difficulties are presented by factors such as the limitation of available observations and the length of the observational record relative to the long time constants inherent in the system. Given all of the above, the construction of an ozone dataset covering the period 1850-2100 and both the tropospheric and stratospheric domains is likely to be a messy business. The manuscript clearly presents the steps undertaken to merge an observationally-derived dataset for stratospheric ozone in the past with model projections for future stratospheric ozone along with model simulations of tropospheric ozone for the entire period. The shortcomings of the approach are also presented – namely problems matching the magnitude of the observed decline in stratospheric ozone with the model-projected recovery. The estimates of radiative forcing inherent in the present ozone database are also discussed and compared with previous estimates.

Overall the article fulfills the important role of documenting the CCMVal-ACC ozone dataset being offered to the CMIP5 community. The one significant omission I find in the manuscript is a more thorough assessment of the evolution of ozone over Antarctica. The article points out that due to problems matching the ozone decrease prescribed in the observations for the past with the model projections for future recovery leads to ozone at 50 hPa over the Northern Hemisphere mid-latitudes never returning to 1980 values in the dataset, while total column ozone over the NH mid-latitudes returns to 1980 values much later than shown by any of the CCMVal models. From Table 3, ozone over the Antarctic at 50 hPa, October recovers at the very end of the time window shown by the CCMVal models (2049-2065) while October total column ozone recovers at the very beginning of the window shown by the CCMVal models (2046-2057). This raises the question of how exactly springtime ozone is projected to evolve over the Antarctic in the merged dataset and whether the process of creating the merged dataset has created peculiarities. There are several instances where the full time evolution of ozone over Antarctica is presented (Figure 3 – ozone at 50 hPa; Figure 6 – tropospheric and stratospheric columns for SON) but the analysis leaves much that is not shown. For example, Figure 10 of the annual cycle of changes in Antarctic ozone shows the largest percentage decreases extending down to 100 hPa yet the reader is left with no information on how ozone is projected to evolve over 1850-2100 at these lower levels. Given the primal significance of the evolution of Antarctic ozone to climate trends over a large part of the southern hemisphere a thorough documentation of the properties of Antarctic ozone seems warranted. I would suggest a separate subsection under the Results and evaluation section where a more thorough analysis of the temporal evolution of Antarctic ozone is presented including, perhaps, how the evolution may compare to projections from the CCMVal ensemble.

*We have changed the section titles slightly and have added more discussion on Antarctic ozone in the section on total column ozone and in the section on vertical profiles. We have also added two new figures that specifically focus on Antarctica (Figure 6 and 14 in revised manuscript)*

The other significant comment I would make is that I find the interspersing of the comparison with observations with the presentation of the longer-term evolution of ozone at different places in the Results and analysis section to be difficult to follow. Both the text and the ordering of the figures flip between presenting the long-term evolution of various quantities with a comparison of these quantities with available observations for present-day conditions. Perhaps it is just a personal preference, but I would urge the authors to consider grouping together comparisons of different quantities with observations (total column, tropospheric column, vertical profiles) and presenting these first. I feel that the reader would have a much easier time putting together the pieces if the assessment was dealt with in one go, then the long-term evolution of different quantities was presented.



*In the results section ozone is now discussed separately for total column ozone, tropospheric column ozone, surface ozone and vertical profiles. Within each section we show past and present-day development, an evaluation of the model results with observations and the long-term evolution in the future. While one could reorder the results and separate past (including evaluation) and future, we also see value in keeping it as it is. We have clarified this in the text accordingly.*

Those are my only general comments on the manuscript. Specific comments are presented below.

Page 10880, Line 13; the phrase '...unlike in the stratosphere, there is no simple model for tropospheric ozone to define its distribution.' gives the impression the reference is to the spatial distribution. The 'simple model' for stratospheric ozone, I assume, is the multiple linear regression of the temporal evolution on to EESC and other factors. The discussion should be a bit tighter.

*Revised as suggested.*

Page 10882, Lines 6-16; just a suggestion, but would section 2.1 read more easily if the derivation of the historical stratospheric ozone appeared first. Having the presentation of the stratospheric portion first would lead more naturally toward the phrase 'Furthermore, unlike in the stratosphere...'.

*Changed as suggested.*

Page 10886, Lines 2-6; While I am completely in agreement with the need to use the CCMVal REF-B2 simulations to derive the future stratospheric ozone even though this choice meant only a single GHG scenario was used for the stratospheric ozone, I don't see how the discussion of the uncertainty in 1980 return dates is relevant to the use of different scenarios for stratospheric ozone. The authors have already stated that the divergence between scenarios occurs 'mainly in the second half of the 21st century' so the effect on return dates of differences in GHG scenarios will be smaller. The authors also state that the divergence is largest over the northern midlatitudes and Arctic, which would seem to implicate changes in the residual circulation of the stratosphere and would represent a systematic difference in ozone behaviour with different GHG scenarios that would, hopefully, be captured by the ensemble mean. The discussion of uncertainty estimated by the spread between individual models from the smaller set of CCMs that ran different GHG scenarios doesn't seem relevant to the preceding discussion of possible effects arising from having only a single GHG scenario for the stratosphere. I would suggest removing the sentence beginning 'The uncertainty in the return...'.

*Changed as suggested.*

Page 10896, Lines 13-15; The statement 'The SW forcing increases when clouds are removed due to the reduced albedo (downwelling SW is unchanged, but upwelling is reduced, hence the net forcing at the tropopause is increased).' is very difficult to follow because it seems to imply the change in RF being discussed is that due to the removal of the clouds themselves, reducing the upwelling. The actual comparison is between the difference in the RF due to a change in stratospheric ozone with clouds and the difference in RF due to a change in stratospheric ozone without clouds. I agree that the change in stratospheric ozone for the two cases will have the exact same effect on downwelling SW at the tropopause. The difference is that some of the increased downwelling SW due to the change in stratospheric ozone will be reflected right back up by the clouds, but without clouds a larger fraction of the increased downwelling SW will stay in the troposphere. This explanation does not come out in the present wording.

*Sentence changed to 'The SW stratospheric O3 RF increases when clouds are removed, due to the reduced albedo (the downwelling SW is unchanged by the removal of clouds, but the upwelling SW is reduced, hence the net stratospheric O3 SW RF at the tropopause is increased).'*

Figure 2 – the solid and dashed linestyles attributed to original and adjusted ozone



timeseries seem reversed. Judging from the figure, is it not the solid line that was used for the original RCP timeseries?

*Corrected.*

Figure 3 – as shown for the stratospheric timeseries in Figure 2, it would be instructive to see the full, original CCMVal ensemble mean timeseries of ozone for the 50 hPa level.

*Corrected.*

Figure 6 – is it possible to rework the legend to simplify the long list of different colours and linestyeles? I'm thinking of noting only the colour attributed to each region, which is the largest difficult in untangling the different lines, then a separate attribution of linestyles to each RCP.

*Corrected.*

Figure 10 – Is it possible to standardize both the vertical domain and the reference periods for the calculation of the differences for the two panels?

*Corrected.*

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10875, 2011.