

**REVIEW OF “Stratospheric ozone change and related climate impacts over 1850–2100 as modelled by the ACCMIP ensemble” BY**

F. Iglesias-Suarez, P. J. Young, and O. Wild

*Dear Authors,*

*Thank you for submitting the revised manuscript. The authors have revised the manuscript in accordance with the referee comments. However, I have some minor/technical corrections here. Please attend these too.*

*Many thanks,*

**SPECIFIC COMMENTS**

1. The details about trend calculations are not given. Are they just linear trends? Regression models are used for these? The only hint I have here is in Page 20, line 12. However, this is not enough. Please give a short paragraph on these calculations and the uncertainty estimation of the trends. Please also state the reason for starting the trend calculations from 1980 (not from 1979) there (e.g. Hist 1980 time slice).
2. There are published trend estimates available for comparisons. Therefore, it would be good to compare your trend calculations also with those available.

*Page 25187, Lines 20-26. I think there should be discussion of the complication of calculating 1980-2000 trends from time slice experiments for 1980 and 2000 conditions.*

*Page 25188, Lines 23-24. Here it is stated that Tier 1.4 of the BDBP ozone database is based on a regression model to the original observations. Are all terms of the regression conserved when deriving the trends that are used for the comparisons shown in Figure 2?*

3. Please give the information provided in your answer file in the manuscript too for these comments.

*BDC: To explain the biases of the tropical and NH midlatitude O3 concentration, authors mentioned the importance of the BDC. Such influence could be simply illustrated by a scatter plot of tropical O3 and NH midlatitude O3 for all ACCMIP models. For example, if the modeled BDC is stronger than observation, negative relationship between the two would be stronger. Based on Fig. 5, I suspect that 50 hPa in the tropics (decreased O3 by the intensified upwelling) and 150 hPa in the extratropics (increased O3 by the enhanced downwelling) would be reasonable choice for the scatter plot. This scatter plot would also reveal the relationship between the mean biases and trends of tropical O3 and those of extratropical O3.*

4. I thought it is a good suggestion. Would it be possible to provide the figure/analysis in a supplementary material or in the answer file? This would be a good piece of information on the representation of BDC in the models.

5. In Figure 1 (answer file, replies to Referee 3), you have multiplied the ozone values with cosine of the latitude. Did you also do this for all other trend estimates presented in the article? If yes, please mention this in the text or figure caption.

### **TECHNICAL/MINOR COMMENTS**

P 1, L 13: Representative Concentration Pathways (RCP)

P3, L2: “peaked around 1998”. It depends on altitude and latitude

P15, L2: Please mention the altitude ranges for the tropospheric and stratospheric columns here.

P18, L22-24: Since this has already been noted by others, as you have stated in Introduction, it would be good to add, “as also shown by other studies” or something similar here.

P19, L5: Throughout the paper SON is used as the austral spring (e.g. P12, L4), but OND here?

P19, L23: I thought the real reason for this is the difficulty/limitation of models to reproduce the cold winters well, not “due to the cold winters” and “associated ozone loss”, as noted in lines 25—26.

P24, L10: “not captured” or “not well captured”?

Figure 3 and Figure 4 Captions: TCO cannot be negative. So please mention the normalisation in the first sentence itself.

Figure 6 caption: SON is termed as the austral spring in other sections. Line 4: a space after the full-stop.