

## Author Response to Anonymous Referee #3 Comments

Thank you for your comments. Referee comments in black and author comments/actions in red.

“This is an interesting and useful analysis, but needs to be put in context and contrasted with other recent work before it is published. The authors would also be well-advised to better qualify the limitations of their study, in particular with regard to tropospheric chemistry.”

Thanks, we agree with these comments and now have added more references to contemporary work and better pointed out the limitations of our study, particularly relating to tropospheric chemistry and the specified dynamics simulations used.

“Detailed remarks:

Page 3, lines 21-25: (and elsewhere) the authors focus on Lamarque et al. (1999), a 20-year old study. There are much more recent modeling studies that show larger net influence of STE. For example, Figure 6 of Banerjee et al. (2016) looks very much like Figure 5 of this manuscript. Doubtless similar plots for other models exist. If the authors wish to make the case for their result “that the stratospheric influence on tropospheric ozone is larger than previously thought”, they need to quote recent studies that show smaller influence.”

We agree. The statement has been toned down to reflect the important role of the stratosphere more generally. The influence of STE on tropospheric ozone is now updated here in accordance with more recent studies (including a reference to Banerjee et al., 2016). However, we keep the reference to Lamarque et al. (1999) in places to emphasise the similarity of their analyses to ours and the focus of the paper.

“Line 34: Similarly, the authors cite only one observational study, and for the Southern Ocean. There have been many such studies, e.g. Dibb et al. (1994); Elbern et al. (1997); Stohl et al. (2000); Zanis et al. (2003); Colette and Ancellet (2005); Cooper et al. (2006); Thompson et al. (2007a,b); Cristofanelli et al., (2010); Tarasick et al., (2018). Citing some of these would not only be appropriate, but would support the authors’ point, as in general they find modest influence of STE on lower tropospheric ozone levels.”

Thanks for these additional references. We now summarise and discuss this literature in a separate paragraph which immediately follows on from the discussion of stratospheric influence according to model studies.

“Page 4, lines 10-11: While I agree that there are certainly major limitations with the accuracy of retrieved tropospheric ozone from spaceborne instruments, I take issue with the statement “...scientists must instead rely on tools such as chemistry climate models (CCMs) to fill in the gaps in our understanding of the global distribution of tropospheric ozone”. NO, NO, NO! Models are sophisticated data visualization tools: they contain (at best) all that we know about the atmosphere. They allow us insights and interpretation that would not be possible without them, but they do not contain anything that we don’t, collectively, already know.”

Whilst we largely agree with this point, we would argue that quantification of the stratospheric contribution ( $O_3S$ ) to both the vertical and global distribution of tropospheric ozone is not

possible from observations alone. Additionally, specific model simulations and diagnostics can help to disentangle various feedbacks/mechanisms that could not be inferred from observations alone. We have rewritten this statement to now better explain the added scientific value we can gain from CCMs in comparison with observations.

“Page 8, lines 10-11: I thought the main problem was lack of photons that actually penetrate this far, as well as lack of contrast in the scattered spectrum, compared to a few km higher up.”

We agree that this is the main issue with the retrieval of particularly lower tropospheric ozone (with errors in the retrieval due to albedo and aerosols for instance of secondary importance). This detail has been added into the manuscript.

“Page 8, line 18: It seems odd to cite satellite papers for generic facts about the global ozonesonde network. Liu et al. (2013b) has a good discussion, with a map and table of sites. In line 26, the proper reference here is Smit et al. (2007), although the others are fine as additional references. On the next page (line 2), citations are required for the WMO & JOSIE campaigns.”

We agree with this point and omit some of this detail, referring the reader to the suggested Liu et al. (2013b) reference. Citations have been added for the WMO and JOSIE campaigns. Many thanks for these references and clarification.

“Page 9: I find the statement in lines 28-30 quite remarkable. I believe there are many studies showing that long-range transport of ozone and its precursors are the dominant source of ozone in remote areas.”

We agree that this argument is valid but more applicable to the lower troposphere. We thus revise this sentence to refer to the free troposphere and tone down our assertion of the dominant role of STE to avoid any contradiction.

“Page 17 (top paragraph), and elsewhere: the authors put a lot of effort into explaining the effects of “vertical smearing”. Of course they need to consider OMI AKs when comparing to OMI data, but perhaps they would find it easier to use a 3D ozonesonde based dataset, like Liu et al. (2013a,b).”

We believe our paper will serve to highlight the importance of AKs for model-satellite measurement comparisons which is sometimes not fully understood or appreciated within the CCMI community. We agree that comparisons with an ozonesonde-based dataset using trajectory mapping would provide further insight but of course such products have their limitations. Such analysis we would suggest is beyond the scope of this study but we now mention such approach could be warranted to further establish and confirm the presence of such model biases we found in our model-ozonesonde comparison (Fig. 4).

“Page 22 (Figure 7a): How are these plots produced from ozonesonde data?”

This is now made clear in the opening sentence to sub-section 4.3. Ozonesonde profile measurements were aggregated for each month and for 10 degree latitude bands, which were

then subsequently averaged over all 31 years (1980-2010) over all longitudes (zonal average). Similarly to Fig. 4, measurements were interpolated and averaged between  $\pm 20$  hPa for each pressure level (350, 500 and 850 hPa).

“Page 23, line 22: The “significant difference in the strength and dominance of the shallow branch of the BDC in each model” needs more explanation. It is first mentioned here, in the Summary.”

The build-up and burden of ozone in the extratropical lowermost stratosphere is directly related to strength of the lower BDC branch (since the equatorial region is where most ozone is produced). We add in this additional detail and include citations.

“Page 31 (Summary): The authors should consider, and discuss, the differences between their results and the much smaller STE response found by Neu et al. (2014). In particular, Neu et al. claim that larger responses of tropospheric ozone to STE are found in models without comprehensive tropospheric chemistry.”

We have added a few sentences discussing our findings in relation to the earlier study by Neu et al. (2014) and note this important caveat.

“Page 32, lines 31-34: Discussing the comparison before the AKs are applied makes little sense, and should be omitted.”

Actually, we feel this finding highlights an important trade-off in applying AKs to models that have known stratospheric biases for model-measurement comparisons of tropospheric ozone. In the case of CMAM, we can infer through our analyses (Fig.2 and Fig. 4 in section 3) that the closer agreement to OMI arises due to the competing influence of the relatively simple tropospheric chemistry scheme (underproduction of in situ photchemical formation of ozone) and excessive smearing of stratospheric ozone due to a high bias in the lower stratosphere ( $\sim +20-60\%$ ). Such analyses therefore show that CMAM is more deficient in its representation of tropospheric ozone than EMAC, whereas the opposite might be inferred from Fig. 1 alone. We expand this point to argue our case and make the reader aware that in a limit number of cases, where stratospheric biases are sufficiently large, application of AKs for model-measurement comparisons of tropospheric ozone would not be advocated, particularly if the model representation of tropospheric ozone is known to be good.

“Page 33, line 26: See previous comment, page 23, line 22.”

Sentence expanded to explain this conclusion again.

“Minor points:

Page 5, lines 11, 12: Typographical errors.”

Sentence has since been removed.

“Page 5, line 34: The solar cycle evolves?”

We refer to the 11-year solar cycle which we now state explicitly for clarity.

“Page 6, line 9: Typographical error.”

Removed ‘have’.

“Page 6, line 19: “Compared with EMAC”?”

Changed to ‘In contrast to EMAC’

“Page 8, line 20: Actually the WOUDC also has data for Indian, Brewer-GDR, carboniodine and Regener sondes.”

Thanks for this clarification. We however remove such detail and direct the reader elsewhere (e.g. Liu et al., 2013b) as suggested by yourself.