

Interactive comment on “Characterising the Seasonal and Geographical Variability of Tropospheric Ozone, Stratospheric Influence and Recent Changes” by Ryan S. Williams et al.

Anonymous Referee #3

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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.

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

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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.

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.

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.

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.

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.

Page 9: I find the statement in lines 28-30 quite remarkable. I believe there are many

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studies showing that long-range transport of ozone and its precursors are the dominant source of ozone in remote areas.

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).

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

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.

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.

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

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

Minor points:

Page 5, lines 11, 12: Typographical errors.

Page 5, line 34: The solar cycle evolves?

Page 6, line 9: Typographical error.

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

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

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Page 8, line 33: Local air pollution (SO₂) is not a significant source of error in recent decades, except in unusual circumstances (volcanoes).

Page 10, line 19: Maximum, not minimum.

Page 12, line 1: “An effect. . .” Not “The effect. . .”

Page 15, line 2: Minima is plural.

Figure 3 caption: “data” is plural.

Page 23, lines 28 & 31: Not clear which model is being discussed.

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