

Atmos. Chem. Phys. Discuss., author comment AC1
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Reply on RC1

Daniel J. Ruiz and Michael J. Prather

Author comment on "From the middle stratosphere to the surface, using nitrous oxide to constrain the stratosphere–troposphere exchange of ozone" by Daniel J. Ruiz and Michael J. Prather, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-635-AC1>, 2021

We wish to thank reviewer 1 for prompt and very helpful comments on how to improve the paper.

Daniel Ruiz & Michael Prather

Response to **Anonymous Referee #1**

This study concerns developing observational constraints for the stratosphere-troposphere-exchange (STE) of ozone using N₂O and CFC-11. The authors argue that the concentrations and distribution of the latter species are well constrained with satellite and surface measurement data and exhibit useful cross-tropopause concentration gradients and relationships with ozone. Using measurements and simulations, the authors determine STE fluxes of ozone, N₂O and CFC-11 on seasonal and interannual timescales, as well as presenting how the fluxes influence the surface concentrations and how ozone fluxes in the southern hemisphere are related to the depth of Antarctic ozone hole. One particularly notable result is that their analysis points to a hemispheric symmetry of STE fluxes, in contrast to the expectation from the asymmetric strength in the Brewer-Dobson circulation.

I feel that the results and analysis presented here will be a valuable addition to the community, particularly given recent pushes to understand tropospheric ozone processes and variability through IGAC's TOAR project. Overall, I have no major reservations about the methods and analyses but I do think that the manuscript would benefit from a major overhaul in terms of its structure and presentation: I, for one, found it very hard to follow all the different threads. I expand on this comment below along with some other comments and suggestions.

Major comment

Please consider revising the structure and presentation of the manuscript. As written, the tools, data sources and, crucially, major findings are not clear to this reader. For instance,

I would encourage the authors to present the global and hemispheric ozone STE fluxes in the abstract and conclusions – these are going to be the numbers that many will want to draw from this analysis, and (like it or not!) may not have time to read through the rest of the study. Additionally, highlighting the hemispheric fluxes would enable the authors to more obviously highlight the (pre/post ozone hole) NH and SH symmetry that they find, which will interest many in the stratospheric dynamics community.

We agree that the STE fluxes should be highlighted and clearly stated in both places. We include a new Table (1) summarizing these, and have added two new paragraphs in the last Section on consistency and uncertainties. For example, we discuss the self-consistency of our results for the O₃ and N₂O fluxes with the O₃:N₂O slopes in the lowermost stratosphere.

I would also encourage the authors to consider the story that is told through the manuscript. I do not insist on a traditional structure, but I would certainly appreciate a clear distinction between data and methods and the rest of the results. As presented, the model gets described in the same section that discusses global STE results before we see new sections on interannual variability and the importance of the ozone hole, with the tracer data described later. I find all these different threads confusing and I lose how they are relevant to the bigger story about ozone STE that the title and abstract promise. There are many possible solutions to this, and I only encourage the authors to think about signposting the reader and telling a logical story.

We have pulled out the 'methods' material in a new section 2 and made it separate. We think this does indeed help the flow. In terms of the order, we find it difficult re-order without a total rewrite, and we cannot see a clear path to such a rewrite. The material is indeed a circle of connected analyses. The expanded final section may help.

Other comments

- *Can uncertainty estimates for the STE fluxes be provided? Notwithstanding model and measurement uncertainty, is there something that can be estimated from the tracer-tracer correlations?*

We should not shirk this task, and so we have developed a simple traceable best estimate, following along the lines of the original Murphy & Fahey approach. See new Table (1) and paragraphs in the final Section 6 on 'consistency' and 'uncertainty'. Thanks for the push on this.

- *Please review the clarity of the figures, particularly considering appropriate font sizes.*

Yes, Figures 1-3 need larger fonts or to be presented as larger figures, the others look OK to us.

- *I encourage the authors to proofread the manuscript as there are several typographical errors.*

Yes, we humbly agree and have been finding and fixing those.

Specific comments

P1, L15: Presumably CCMs as well as CTMs?

Yes. For the abstract we just used 'global chemistry models'.

P2, L61: Climate projections of what?

Yes, have changed that to: "projections of future warming"

P6, L18: "...well correlated ($r \sim 0.9$)..." (to be clear what the 0.9 is)

Correct, we now have defined cc as Pearson's correlation coefficient (old L166) and now use 'cc ~ 0.9 ' in the parentheses, with other clarity improvements in the paragraph noted here (old L217-255)

P8, L260: Do you mean the anomalous QBO of 2015/6, rather than "08/2010"?

No. Based on Ruiz et al, 2021 the surface impact on N₂O was highly anomalous during the 2010 QBO. We made this distinction clear in the revision.

P16, Table 1: Suggest this goes in a discussion section, and suggest full references are used so that it stands alone

This table (now Table 2) is located and discussed in the final section. The abbreviated references have been made standard as requested.