

## Review of Ruiz and Prather, submitted to Atmos Chem Phys

Anonymous Referee #1

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Referee 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-RC1>, 2021

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This study concerns developing observational constraints for the stratosphere-troposphere-exchange (STE) of ozone using N<sub>2</sub>O 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, N<sub>2</sub>O 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 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.

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.

### **Other comments**

1. 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?
2. Please review the clarity of the figures, particularly considering appropriate font sizes.
3. I encourage the authors to proofread the manuscript as there are several typographical errors.

### **Specific comments**

P1, L15: Presumably CCMs as well as CTMs?

P2, L61: Climate projections of what?

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

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

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