

Earth Syst. Dynam. Discuss., referee comment RC1
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Comment on esd-2021-98

Anonymous Referee #1

Referee comment on "CO₂ surface variability: from the stratosphere or not?" by Michael J. Prather, Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-98-RC1>, 2022

This is an interesting paper addressing an oft-overlooked influence on trace gas abundances at Earth's surface. The results, while indicating no significant influence on interannual variations in atmospheric CO₂ mole fractions, is important for ensuring that interpretations of the CO₂ atmospheric record are free from significant bias. I only have a few suggestions for the author to consider that I believe, if addressed, would improve the robustness of the conclusions and the appropriateness for this work to be published in this Journal.

Isn't it true that the uniform emission scenario simulation should also result in the development of persistent trop-strat gradients in CO₂ mole fraction? As a result, the calculated surface mole fractions should include some effect of STE variability, not just tropospheric transport and mixing. So I'm surprised that the results described as investigating tropospheric transport IAV only (lines 65-70, lines 122-123), and that the results are recast as indicating only the influence of atmospheric transport of NH fossil fuel emissions into the SH (line 138-139). Consider a more accurate description of this scenario.

The sCO₂ scenario, designed to isolate the influence of interannual variability in strat-trop exchange, is set up by including a negative forcing flux at 80 km (top of the model), which then gets transported to the surface. This simulation and a rescaling of results for N₂O to make it more appropriate for CO₂ are used as an indication of the expected influence of STE on surface CO₂ mole fractions. While the sCO₂ and rescaled N₂O flux results have a similarly small amplitude (a main finding of the paper), they are not often in-phase, suggesting issues with the usefulness (accuracy?) of one or the other as tools for investigating the processes in question here. To make the conclusion more robust, the author might consider (show) whether or not the results (phasing of variability and

amplitude in the sCO₂ results at the surface) are influenced by where in the stratosphere the growth rate-related flux (-0.8 pgC/yr) is placed in the model (currently at 80km). Related results for the phasing of CFCs and other gases (STE influence at Earth's surface) has shown that it is influenced by the altitude where the loss flux occurs in the stratosphere. As a result, I think it is important to demonstrate that placing the CO₂-related loss flux into a lower model level doesn't affect the conclusions being made here (i.e., the timing of the phasing in the resulting surface mole fraction IAV for CO₂, or its amplitude).

The author also includes some conclusions on the causes of interannual variability in CO₂ over 2 to 10 year time spans. But I'm not convinced that the periods identified by the author in which the NH or SH lead are convincing or obvious. Perhaps, given the difficulty in interpreting the SMO results, as discussed, the figure 1e should only include three sites? A shortcoming of using MLO in the NH is that it too can be influenced by unusual transport regimes (like SMO). Are there other NH sites not made overly noisy by the large photosynthetic uptake signal that might also be considered here to add to the robustness of this analysis?

Finally, the analysis of seasonal amplitudes in CO₂ annual cycles is interesting, but currently lacks some indication of the goodness of fit of the linear regression. Can a figure showing the amplitudes derived from the 12-month standard deviations be shown over time? Or some appropriate metrics be cited?

Details:

Line 18-19, last sentence of abstract isn't very clear.

Line 24-25, verb missing

Line 46-48, line 101. inaccurate representation of the Ray et al. paper, which demonstrated the particular effect being discussed here for N₂O, CFC-11 and CFC-12, not just CFC-11. And that paper also focused on results from the SH.

Line 66, emissions are uniform in space *and constant over time*?

Line 85, typo

Line 139, 2019 not 2016

Line 148, unusual wording—fit was removed, but what was retained?

Lines 165-170, the unusual nature of SMO has been demonstrated for other long-lived trace gases, some citation to those studies seems warranted here to show that it isn't just an issue for CO₂.

Methods and results are included in the Figure 1b caption—likely better placed elsewhere. In fact I think most (all?) of that info already is in the main text.