Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-185-RC2, 2021

Comment on acp-2021-185

Anonymous Referee #2

Referee comment on "Spatial distributions of X_{CO_2} seasonal cycle amplitude and phase over northern high-latitude regions" by Nicole Jacobs et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-185-RC2, 2021

Spatial distributions of XCO2 seasonal cycle amplitude and phase over northern high latitude regions by Jacobs et al., is an ambitious analysis of the CO2 dry air column-average mole fraction measured by the OCO-2 satellite. The authors analyze satellite data that has been bias-corrected based on previous work by Jacobs and complement this analysis with simulated XCO2 from GEOS-Chem and a surface contact-tracing approach. The analysis is interesting and wrangling the OCO-2 observations at high latitudes for meaningful science is an important advance. Before publication, however, there are a few major points for the authors to address.

My largest concern with the paper is that the authors are underutilized their contact tracing simulations. The contact tracing approach was not clearly explained, and results from these simulations were introduced only in the discussion section. This needs to be fleshed out more in the results section, with a mind to new or refined supporting figures. Fig. 10 could be modified to better guide the reader as to the similarities noted with Fig. 4. For example, could boxes be added in Fig. 10 around the zones whose amplitude is in the top Nth percentile in Fig. 4? Related to the contact tracer approach, it was unclear why the CarbonTracker posterior fluxes were run through GEOS-Chem rather than simply using the CarbonTracker posterior CO2 fields. Was this so the transport model was consistent for the CO2 simulation and the contact tracer simulation? If so, this should be explained in the text. However, the use of a single transport model is a major limitation to the authors' conclusion that accumulation of transported CO2 controls the seasonal cycle amplitude spatial patterns. It may be onerous to repeat the surface contact tracer analysis in another transport model, but could the authors conbine information from the CarbonTracker (TM5) posterior and their own CarbonTracker/GEOS-Chem simulations to substantiate their conclusion?

A second major issue is that the key insights from the research could be better laid out. Is it that the column seems to show earlier drawdown than inversions constrained by surface observations? The different behavior in Siberia compared to other zones? What are the implications for a correlation between amplitude and HDD in terms of processes that affect the seasonality of net exchange? Perhaps reorganization of the discussion section, with the surface tracer analysis presented in the results, will make it easier for these points to come through in the discussion.

I struggled with Section 3.2, since the section begins with a listing of material in the supplement. The authors should organize this information to provide a clear summary in the first few sentences of the paragraph/section, and then later refer to the supplementary figures for more details. In particular, the "unrealistic trop in wintertime values" might warrant a figure in the main text since the realism of the seasonal cycle fits is crucial for interpretation of the rest of the paper.

The term HDD was used before the concept was introduced or defined (L29 p7)

The authors cite a meridional gradient in Fig. 12, which I didn't see convincingly in any panel. This is brought up in the discussion (p14), and I don't think removing this sentence would affect the authors' underlying argument. If it is left in the document, more support is required.