Review of
"Characteristics of Interannual Variability in Space-based XCO_2 Global Observations"
By Guan et al.
Submitted to ACP

Summary

This manuscript evaluates the interannual variability (IAV) of OCO-2 XCO2 observations over the period 9/2014-12/2020. Specifically, the IAV of the detrended, deseasonalized XCO2, which can be thought of roughly as "IAV of the XCO2 growth rate" (which is how this paper describes it). The manuscript shows that broad features of the IAV on broad zonal scales are strongly correlated with ENSO phases, and further how the XCO2 increases caused by the 2015-2016 El Nino propagate from the tropics to the northern extratropics over a period of roughly 6 months. They compare IAV from OCO-2 with that from 26 TCCON stations (individually and aggregated by latitude band) as well as boundary layer CO2 from select NOAA surface stations. They find general agreement from all three data sources, though data at individual locations are very noisy, regardless of data source.

Overall, I find the paper very well-written and generally complete. I think it will be an interesting addition to the literature, further advocating use of space-based XCO2 data in different ways. However, I do find some small deficiencies throughout the paper. Therefore, I recommend publication after dealing with the various (mostly minor) issues & questions I raise below.

General Comments

So much of the abstract seems, well, abstract. Is this a validation paper, basically validating OCO2 IAV so we can have more confidence in flux inversion results? Or to better understand the spatiotemporal scales at which we should aim our flux efforts which utilize OCO2 data? I suggest making the abstract a bit more clear about how much the manuscript is validating OCO2 IAV, versus to what degree it is doing interesting analysis with the IAV itself.

Regarding comparisons to TCCON & MBL sites: it seems like because of the low sampling associated with TCCON and the MBL sites, OCO-2 derived IAV is more powerful because of the better spatial sampling. You may wish to point this out in the abstract and/or conclusions more specifically. (I also wonder how much better future wide-swath sensors may be). Did you ever consider applying your method to GOSAT to derive IAV, to see how it compares to OCO-2? It would be especially interesting as we have 11+ years of GOSAT XCO2.

Specific Comments

Abstract: *"The amplitude of IAV variations is up to 1.2 ppm over the continents and around 0.4 ppm over the open ocean."* Please make it clear in the abstract that you are defining "amplitude of" as "standard deviation of".

Sec 2.2.1: The results of the spatial aggregation sensitivity analysis seem to show substantial differences in the IAV depending on the spatial scale of aggregation. How do you know which spatial scale is most accurate, given that the differences are not just noise, but show large-scale biases? These large-scale differences clearly matter, as you show later most of the IAV is less than 0.75 ppm. I strongly suggest you repeat your sensitivity analysis with high-resolution model data (rather than real data), sampled like OCO-2. If you use model data, you know the right answer, so you can see what you can get away with. Something like the GMAO 0.75 deg model should have sufficient resolution for this purpose.

Sec 2.2.2: It seems like using a 3rd order polynomial on a 7-year time series to remove the secular increase is a recipe for problems when trying to derive the IAV. Wouldn't this artificially remove some of the IAV? Please discuss why 3rd order is necessary in the paper. Did you test 1st or 2nd order ? If so, why were they not sufficient?

Fig 5a: Care to comment on the strong feature near the beginning of 2020 peaking at 60S latitude? That seems stronger that random variability.

Near Fig6: Because MEI/ENSO is such a heavily discussed topic in this work, a plot of the correlation coefficient of MEI with IAV timeseries in local 5x5 gridboxes may be warranted – similar to figure 6. Have you made such a plot, and does it show any interesting teleconnections? You may need to introduce a lag at the more northern latitudes when calculating correlation coefficients there (a simple 0-6 month lag as a function of latitude could work).

Figure 10: Each "point" on the plot has in fact some uncertainty on the IAV at each site, due to both retrieval errors and spatiotemporal noise. Is it possible to get an estimate of this, and use it to add x & y error bars on each point? That might give a better picture of how consistent TCCON and OCO-2 IAV are, to within their respective errors. This figure implies that they are not very consistent.

Related to the above, please check your IAV stddev calculations. I tried to reproduce your Bialystok numbers for OCO2 and TCCON. Just by eyeballing your Fig S11, I got 1.05 for OCO2 (similar to your number), but I got 0.75 for TCCON, whereas you got roughly 0.5. I wonder if some of your TCCON values are too low for some reason (in particular at the NH sites). Your values at Karlsruhe and Orleans also both seem unreasonably low (both less than 0.4 ppm).

Technical Comments

Line 72: Remove comma after "Chatterjee et al."

L76: "…is being used implicitly for flux attribution…". Please provide example references.

L85: Please add reference Baker et al., *Geosci Mod. Dev.,* https://doi.org/10.5194/gmd-15-649-2022.

L110: Replace the O'Dell et al, 2012 reference with the O'Dell et al, 2018 reference. The former applies to GOSAT; the latter applies to OCO-2 and is a much more appropriate reference.

Sec 2.1.2: Please state somewhere if you use GGG2014 or GGG2020 (I'm assuming the former).
Sec 2.1.3: If there is any kind of version number or data source website for the NOAA sampling data, please provide it.

Figure 1: Most of the TCCON sites are in completely wrong places!! It looks like the longitudes are screwed up?

L161: Please provide a reference for the NOAA monthly OLR data set, or remove the sentence about the source of the ENSO-related variables (which is probably not necessary as it will be given in the MEI documentation).

Figure 9 caption: Do you show R or R^2 in panel b? Please make the caption more clear. Currently the caption says R and the plot says R^2 on the axis label.

Line 362: Change "R" to "correlation coefficients R". Otherwise we really have to guess that you mean correlation coefficient.

Line 387: "although we note that the IAV amplitude is a factor of almost two smaller in the column average mole fraction". Relative to what? Please add "relative to boundary layer CO_2" or something similar.

Fig S1 Caption: Please give the min # of soundings per gridbox.
Fig S3 Caption: Please give the spatial gridding (5x5, etc) used, and change word "use" to "using" in the caption.

Fig S6 Caption: Suggest changing word "record" to "years" ? It took me a while to figure out what you were getting at here.