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Comment on acp-2022-46

Anonymous Referee #2

Referee comment on "Carbon dioxide variations in the upper troposphere and lower stratosphere from GOSAT TANSO-FTS TIR profile data" by Akihiro Honda et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-46-RC2>, 2022

Review of Honda et al.,

First of all apologies for my late comment, which was due to unforeseeable issues. However I'm sorry to say, that I had a hard time reading the manuscript. The data set is very interesting and clearly of interest to the community. The discussion of the link to transport and dynamics is however misleading or partly wrong and neglects many aspects of transport (e.g.. the role of the tropopause as a barrier for transport and its effect on the CO₂ cycle amplitude or phase, which has already been discussed in several papers).

The paper presents observations of CO₂ from GOSAT from 2010 to 2013, which is analyzed at pressure levels of 500, 250, 150, and 100 hPa. The authors detrend the data by applying a simple linear empirical fit to build multiannual climatologies and anomalies. They show the zonal mean distributions of CO₂ and at the levels mentioned above to conclude on the transport processes, which cause the observed CO₂ distributions, but neglect relevant literature (Boering, Andrews, Sawa, Hoor, ...). For the troposphere they state a transport time of two months from the LT (lower troposphere) to the UT (upper troposphere) and a dampening of 50% of the seasonal cycle amplitude. They relate this to the "absolute mixing ratio decreasing with altitude and to a lesser extent mixing with low CO₂ mixing ratio air mass". Further they link tropical CO₂ mixing ratios to ENSO and identify interannual variability of the CO₂ in the monsoon region to different relations of vertical transport by convection and horizontal transport "via anticyclonic circulation". The authors neglect relevant literature of CO₂ and its seasonal cycle from aircraft observations.

The paper does not include any analysis of transport via e.g. Lagrangian methods, nor it shows links to surface observations or at least comparisons to the interannual variations of emissions or surface distributions or variability (zonally, globally or regionally, e.g. monsoon).

The authors further discuss transport and mixing particular at 250hPa, but do not even mention the term "subtropical jet", mixing barrier, isentropic transport, and consequently do not discuss their roles for the propagation of the seasonal cycle. They also state that $\Theta=370$ K "indicates the physical surface of the tropopause", which is simply wrong. They fully dismiss the role of the extratropical tropopause as transport barrier, when discussing the timing of the seasonal cycle and its amplitude change at the barrier.

They state, that the role of seasonal CO₂-cycles has not been studied and neglect significant corresponding work: For the stratosphere above 100 hPa: Andrews et al., 1999, Boering et al., 1994, 1996, Strahan et al., 1998.

For the UTLS and lower stratosphere: Hoor et al., 2004, Engel et al., 2007, Boenisch et al., 2009. For upper troposphere and the monsoon: Schuck et al., 2010, Gurk et al., 2008.

All in all there are too many speculations when linking transport and CO₂ observations. I recommend to resubmit it focusing on the climatologies and the observations and carefully linking them to e.g. surface seasonal cycles from global observational network for the LT/MT data. For the UTLS there must be a correct treatment of the tropopause particularly for the 150 hPa and 250 hPa level. One could e.g. derive distinct seasonal cycles for tropospheric and stratospheric data, which can be compared to existing data sets (see references). Speculations about transport mechanisms should be removed.

Therefore I can't recommend the paper for publication in the current form.

I do highly suggest a resubmission with a different focus, since the data set as such is very valuable, but the discussion of potential links to transport and mixing is inappropriate. I encourage the authors for resubmission either sharpening the transport discussion or just focusing on the climatologies.

Minor points: line 109: What is the vertical resolution and how do averaging kernels look like?

Fig. 1: Gradients appear at the tropopause. These were not accounted for. The discussion of trends and Figure 1 illustrates an example of the coarse and insufficient discussions and speculations: The trend figure is discussed without any mentioning of the tropopause and its role for e.g. the mid-lat trend. The according table 1 provides trends for different latitude ranges, different altitudes without consideration or discussion of the tropopause. What shall one learn from this?

Fig.6 and related discussion (lines 261-268): The monsoon plays of course a role for the

observed 250 hPa CO₂ in Fig.6, but there is no discussion of potential surface emission variations, change of ENSO-related tropospheric circulation patterns, change of emission patterns, the authors state without any supporting analysis, that the observed CO₂ variability is related to variability of deep convection. How do the authors come to their conclusion? How is emission variability differentiated from large scale transport variability or convection?

Fig.2 : Concerning the bias correction, which is also mentioned in the manuscript: Which role does the isentropic CO₂ gradient at the extratropical and subtropical tropopause play for the bias correction? Did the authors consider the tropopause when calculating the bias?

Fig 2d) How do the cycles (e.g. at point Barrow and Mauna Loa) fit to the GOSAT observations at 500 hPa. Highest CO₂ at high latitudes should occur later than at low latitudes, since biological activity is delayed. How does this fit to Fig.2d? also line 165-168.

I.184: The 370K isentrope defines the physical surface of the tropopause. This statement is simply wrong. The tropopause is no way defined by isentropes (read Holton, 1995, Hoskins, 1991, Bethan, 1996...

Fig. 3: How well is the troposphere resolved in the CO₂-data (vertical resolution, kernels, degrees of freedom)?
'Caption': Replace 'vertical velocity' with 'pressure tendency' - they have different signs.

Fig.4b)c): The data at 250 hPa are affected by the tropopause location, which inhibits quasi-horizontal (quasi-isentropic) transport. The phase propagation therefore is different from 100 hPa or 500 hPa (see Sawa et al., 2008, Hoor et al., 2004). Please add the (mean) 2 PVU and 4 PVU contour (also Fig. 6a)-d) and Fig.5 a)

I.208/209: Which vertical gradient? Please calculate or plot (e.g. for different latitudes).

I.210: This statement holds for any tracer and is very unspecific - the distribution of anything in the UTLS depends on vertical and horizontal transport in the troposphere.

Fig.5: 500 hPa shows a trend of the anomaly at higher latitudes. Is this due to the (possibly wrong) linear trend estimate to derive the anomaly (eqn.1)?

Also Figure 7: Why is the CO₂ maximum related to deep convection? Why do the data not show any accumulation effect inside the anticyclone (see e.g. Baker, 2013, Schuck, 2010)?

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