Dear Dr. Xiong as Reviewer#1

Thank you very much for reviewing our manuscript and giving us many useful comments.
We agree your almost all of comments and suggestions, therefore the manuscript has been revised along your (and another reviewer) comments. Please check the following our reply(black) of each your comments (blue).
In addition, we are sorry for late replaying because it takes time to repair the data server which was broken down.

The major points that we deal with in the revised manuscript are as follows:

1. Figures were updated or removed.
   ① Adding 2PVU (potential velocity unit) and OLR (outgoing longwave radiation) as the indicator of deep convection to Figures 1, 2, 3 and 5 of the revised manuscript, and zonal wind to Figure 2 of the revised manuscript
   ② Figure 1 (plot of latitudinal distribution of increasing rate) in the previous manuscript was removed because Table 1 was enough information on it.
   ③ Figure 3d (time-latitude cross section for 4-year at 250 and 500hPa) of the revised manuscript was drawn by the corrected data, because the previous figure was drawn by the data without the bias correction.
   ④ Figure 5 (time-latitude cross section of inter-annual variation) in the previous manuscript was removed from the revised manuscript.
2. The discussion on $CO_2$ variation related with ENSO was moved to section of Discussion and Summary. Therefore the section name was changed to "Summary and Conclusion" to "Discussion and Summary" in the revised manuscript.
3. Many references recommended by reviewers were added to mainly the section of Introduction related with the *in situ* observational studies.

Review to "Carbon dioxide variations in the upper troposphere and lower stratosphere from GOSAT TANSO-FTS TIR profile data" (acp-2022-46)

This study investigated the CO2 mixing ratios in the UT–LS region and analyzed the CO2 trend, seasonal and intra-seasonal variation and its link to large scale circulation, ENSO and Asian summer monsoon etc. It is no doubt that such a study to the trend and variation of CO2 in UT-LS region is very important for global warming study, and our knowledge in this topic is limited due to the limited observations. Therefore, using GOSAT-FTS retrieval products is a novel approach. The method used is straightforward. With some work, it can become a good paper to publish in this journal.

Major problem:

I doubt if the first author fully realizes the limitation of satellite observation using TIR channels. The major sensitivity of TIR sensor is in the middle to upper troposphere, and its sensitivity is less in the LS and LT region. So, my major concern is: if or how much of the derived trend and seasonal variation is from the a-priori used. If it is largely from the a-priori, like using model data as a-priori that include similar trend and seasonal variation, the derived trend and seasonal variation could be wrong. Therefore, I think it is needed to add in the context: the a-priori used in the retrieval, the DOFs used to screen the data. The uncertainty/range of the derived CO2 increase rate should be added.

A: Thank you for the useful comment. Along your comments on a priori and DOF, the following

sentences were added to 2.2 sub-section of the revised manuscript. P.5, line 140-146

*"In addition, data with a higher degree of freedom (DOF), with more than two times the standard deviation from the average of each month, were used for the analysis, because we intended to use data that were not constrained by a priori information, which is taken from the NIES transport model version 5 (NIES-TM05) (Saeki et al., 2013). The retrieved CO2 data at UT fit with COTNRAIL rather than a priori data. Furthermore, the retrieved data allow the concentrations in the UT and LS to be distinguished (see figure 6b of Saitoh et al. (2016)). The magnitude of DOF highly depends on latitudes, high at low latitudes (2.25), and high latitudes (1.25) in both hemispheres., There is little seasonal change in the DOF data. The percentage of data screened by the DOF was about 2%-5%."*

On the uncertainty / range of increase rate, the standard deviation of increasing rate was shown in Table 1. The Figure 1 shown in the previous manuscript was removed because Table 1 covers the information of increasing rate at UTLS. On the other hand, the bias correction was done with considering the inter-annual variation at each latitudinal band and season by using CONTRAIL data as shown in Saitoh et al (2017).

Minor problem:

The presentation is overall pretty good, but some sentences need to revise. Below is a list of some of them, and I would encourage the authors to go through the whole manuscript.

L26: derived ?
A: corrected at p.2, l.25

L67: but about 80,000 observations of the same quality? it is an overstatement. Need to revise
A: The term "of the same quality" was removed, the sentence was revised as follow.
"*about 80,000 observations, both with and without cloud cover*" at p. 3, l. 80

L98: the atmospheric mixing ratio data are of high accuracy? it is also an overstatement as L67.
A: This sentence was removed because the description of $XCO_2$ was not needed for $CO_2$ profile data.

"the accuracy is about 4 ppmv for $XCO_2$ " has nothing to do with GOSAT-FTS.
A: We agree, this sentence was removed.

L100 - L104: What about night-time GOSAT-FTS products and its use ? how to do quality control without CAI?
A: Along the reviewer's comment, the explanation on cloud screening at the night-time are added to p.4, l.111-112.
*"The cloud screening at the night-time was based on the radiance around 900 cm-1 (around 11 μm) of the TANSO-FTS TIR band [Imasu et al., 2010]."*

The following reference was added to the list of reference section.
Imasu, R., Y. Hayashi, A. Inagoya, N. Saitoh, and K. Shiomi: Retrieval of minor constituents from thermal infrared spectra observed by GOSAT TANSO-FTS sensor, P. Soc. Photo-Opt. Inst, 7857, 785708, 2010.

L109: The present study defined the UT–LS region as 287.30–90.85 hPa. This is inconsistent with that in abstract and other places (maybe). Please check.

A: The UTLS is generally defined the region, 300 – 70 hPa. In this study, we use only altitudes that can correct bias using the results of comparison with aircraft observations in previous studies. In addition, along the other reviewer's suggestion the explanation of the averaging kernel was added. The text was modified as follows. P.4, l.121- p.5, l.125.

*"The averaging kernel in Figure 1 of Saitoh et al. (2016) shows the sensitivity of UT, particularly at 300 -- 200hPa at lower and middle latitudes. The present study used the level between 287.30 and 90.85 hPa as the UT–LS region. The numbers of retrieved layers vary from 9 to 14 (see table 1 of Saitoh et al. (2016)), which yield lower (upper) pressure levels of 287.30 (237.14), 237.14 (195.73), 195.73 (161.56), 161.56 (133.35), 133.35 (110.07), and 110.07 (90.85) hPa, respectively."*

L112: The algorithm has a sensitivity peak in the upper troposphere? No matter how smart an algorithm you can design, the sensor itself is the key.

A: As same answer as above, as shown in Figure 1(a,b) of Saitoh et al. (2016), the TIR band has sensitive at UT, especially between 200 and 300 hPa, in low- and middle latitudes,.

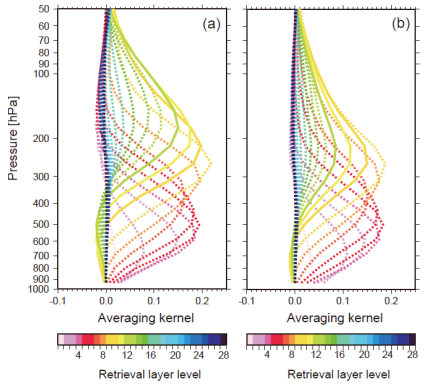


Figure S1 : Averaging Kernel functions at lower latitude in summer (a) and middle latitude in spring of TIR band, adapted from Figure 1 (a,b) of Saitoh et al. (2016).

The sentence was modified as above.

L120-121: … less than 0.5 ppmv at lower latitudes and 1.0 ppmv at middle and high latitudes? It is confused what are you talking about.

A: The sentence was modified as follow, p. 5, l.133-135.

*"\Saito et al. (2017) found that the bias of $CO_2$ data as compared with CME observations at approximately 11 km was less than 0.5 ppmv at low latitude and 1.0 ppmv at mid- and high latitudes."*

L124: conducted a bias correction at "each month"? Should not be that way.

A: The term was revised from *"each month"* to *"each season"*. P.5, l.138.

L142: revise this sentence

A: the sentence was revised as follow

*"For analysis of the inter-annual $CO_2$ variations, we used the anomaly data from the four-year monthly mean derived from equation (2), after subtracting the* ***seasonality*** *for each month and pressure and latitude."* , p.6, l.158-160

L145: citation is missing

A: Add the following reference to p.6, l.162 and the list of reference of the revised manuscript.

*Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horanyi, A., Munoz-Sabater, J., Nicolas, J., Peubey, C., Radu, R., Schepers, D., Simmons, A., Soci, C., Abdalla, S., Abellan, X., Balsamo, G., Bechtold, P., Biavati, G., Bidlot, J., Bonavita, M., De Chiara, G., Dahlgren, P., Dee, D., Diamantakis, M., Dragani, R., Flemming, J., Forbes, R., Fuentes, M., Geer, A., Haimberger, L., Healy, S., Hogan, R. J., Holm, E., Janiskova, M., Keeley, S., Laloyaux, P., Lopez, P., Lupu, C., Radnoti, G., de Rosnay, P., Rozum, I., Vamborg, F., Villaume, S., and Thepaut, J. N.: The ERA5 global reanalysis, Q. J. R. Meteorol. Soc., 146, 1999–2049, 2020.*

L166: this → it

A: corrected, p.6. l.183

L184-185: The 370 K potential temperature defines the physical surface of the tropopause. Where is this from ? also PV= 2 PVU is better to use to define the dynamic tropopause.

A: The revised manuscript uses the 2 PVU for physical tropopause and add the potential temperature of 380K along the other reviewer suggestion.

L218-219 "The minimum $CO_2$ mixing ratios were located at latitudes higher than 60°N and at around 15°N from June to November. ". It is confused and needs to revise.

A: The sentence was revised as follow,

*"The minimum $CO_2$ mixing rations were located at higher latitudes than 60N from June to October in the northern hemisphere and around 15 N from July to November."*, p.8. l.246-247.

Figure 5: caption. Need to revise.

A:This figure and its related description were removed because the other reviewer's comment pointed out that the speculative sentences and diagrams especially ENSO. The discussion related

with ENSO was moved to the section of “Discussion and Summary”.

About the link with ENSO, you can check the following paper
Corbett, A., X. Jiang, X. Xiong, A. Kao, and L. Li, 2017, Modulation of midtropospheric methane by El Niño, Earth and Space Science, 4, doi:10.1002/ 2017EA000281.

A: Thank you for giving the useful paper. In the revised manuscript the description on ENSO was moved to the section of “Discussion and summary“. The suggested reference is added to p.11, l.323-325.