

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.

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 ?

L67: but about 80,000 observations of the same quality? it is an overstatement. Need to revise

L98: the atmospheric mixing ratio data are of high accuracy? it is also an overstatement as L67.

“the accuracy is about 4 ppmv for XCO₂ “ has nothing to do with GOSAT-FTS.

L100 - L104: What about night-time GOSAT-FTS products and its use ? how to do quality control without CAI?

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.

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.

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.

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

L142: revise this sentence

L145: citation is missing

L166: this → it

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.

L218-219 “The minimum CO₂ 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.

Figure 5: caption. Need to revise.

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.

