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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation of GOSAT/TANSO-FTS TIR UTLS CO_2 data (Version 1.0) using CONTRAIL measurements" *by* N. Saitoh et al.

Anonymous Referee #1

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Any improvement in our ability to monitor CO2 from space is important. In particular, being able to use both spectral ranges of TANSO-FTS would increase the vertical understanding in atmospheric CO2. In that sense, it is important to validate precisely TANSO-FTS thermal infrared (TIR) CO2 data. The authors have greatly improved the paper since its initial submission. However, the overall goal of the paper is still somewhat confuse and major revisions are needed.

The title of the paper is validation of GOSAT TIR data, but this is not what is done here. First, the paper deals with a serious update of the retrieval method itself, that has not been published before. Second, and more of a concern, the paper fails short on the

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validation part.

The major concern comes from the question of using or not averaging kernels (AK). Even if points are delivered by the retrieval process at various altitudes, the AK plotted in Fig. 1 prove that these points are in fact representative of a large and often similar part of the atmospheric column. Comparing only one retrieved point (at level 9, 10 or 11 as done here) with one aircraft measurement at the same altitude cannot be considered a validation. Even more when the authors claim that this exercise is aiming at providing the bias needed for studies of surface fluxes, since, in such studies, AK are taken into account.

In several sections, the authors do acknowledge the fact that they do not take AK into account, and part of the discussion is devoted to a small study aiming at evaluating the impact of not taking AK into account. But no quantitative result, and too many vague statement ('relativeky small', 'slightly larger', etc.) are given. I would argue that, for the paper to be accepted, the sections would need to be rearranged in order to:

i)- evaluate the variability of CO2 profiles in the part of the atmosphere the 3 UTLS levels are representative of.

ii)- evaluate the impact of taking into account or not AK, by using all CONTRAIL profiles, completed by ATM simulations of specific climatologies for the upper part.

iii)- then focus on the 3 UTLS levels considered in Section 5. In this part, I am wondering how the results differ when not only the closest GOSAT level is used to compare with CONTRAIL, but when the 3 levels are used indistinctively to perform the comparison (Section 5.2). Such a study would give an insight on how different the CO2 retrieved at each level is.

For each of these points, actual values in ppm, and not vague statement, should be given. For CO2, tenths of ppm do matter!

On another point, the tentative explanation of the biases seem unconclusive. Several

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aspects are briefly mentioned: internal calibration (but with no evidence of a correlation between the internal black body temperature and CO2 biases), choice of the state vector and spectral biases(surface parameters), and bias stemming from an improper retrieval of atmospheric temperature. The impact of adding or nor surface temperature and surface emissivity in the state vector should be the focus of one single subsection, and properly evaluated against CONTRAIL data. Also, the impact of a potential biais in retrieved CO2 stemming from a bias on the retrieved temperature should be carefully studied. In the thermal IR, the ability to decorellate temperature from CO2 is an essential part of the retrieval; this has to be checked. The retrieved temperature profiles should be compared to other temperature profiles (other L2, reanalysis), and checked for seasonal baises

Finally, the conclusions seem rather optimistic. Differences of 2 or more ppm, and latitudinal dependence biases are 'show stoppers' for any attempt at using these data for flux inversions. The authors should put in perspective the values obtained here with what is actually needed by the carbon cycle community. Also, the authors usually refer to as an improvement the fact that biases are reduced when going from the a priori to the retrieved value, but they do not discuss the change in shape of the latitudinal/longitudinal variation which is more a concern than an overall bias.

Specific comments:

A proper definition of bias, accuracy, precision should be given. The authors seem to use indistinctively one for the other.

Section 4. Retrieval algorithm:

- The actual bands or channels used in the retrieval should be given.

- AK obtained in both the tropical and extra-tropical regions should be given since DF seem to differ in both regions, and the altitude of the tropopause should substantially vary in both regions.

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- Values chosen for the emissivity are missing in Section 4.2.

- In Section 4.3, the authors state that 'The existence of a relatively large spectral bias around the CO2 15 μm absorption band in 5 TANSO-FTS TIR L1B spectra (Kataoka et al., 2014) resulted in a decrease in the number of normally retrieved CO2 profiles'. Could the authors explain why?

- The conclusions on the inclusion of surface emissivity in Section 4.3 and in Section 6 (P13013) seem reversed. Overall, does including the emissivity in the state vector matter or not? For the whole profiles, of for the UTLS part of the profile?

Concerning the figures, the captions are usually quite long and most of them are just repetition from the text. The y-scales of several of them should be more adapted to the values in order to highlight the discrepancies between the curves (for e.g., the y-axis for Fig. 6 and 7 could be 384:392). Figure 8 is particularly busy and hard to read; it could be split in 2.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 12993, 2015.