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Atmospheric Measurement Techniques Discussions

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

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To Associate Editor and Referee #1,

We appreciate you reading our paper carefully and giving valuable comments and suggestions again. We have considered your recommendations for revisions and made the necessary changes. The major points that we deal with in the revised manuscript are as follows:

1. We have changed "V1.0" to "V1" throughout the text. The GOSAT project has released V01.00, V01.01, and V01.20 produces, but the CO2 products of all the three

versions are exactly the same data.

2. We have eliminated Figure 2 of the original manuscript not to defocus the scope of this paper that discusses UTLS CO2 data. We have described that the simultaneous retrieval of surface parameters did not affect retrieved CO2 concentrations in the UTLS regions, but could increase the number of normally retrieved CO2 data.

3. Following the recommendations of the two Referees, we have investigated the effect of considering TIR CO2 averaging kernel functions on CO2 concentrations in the UTLS regions. For this purpose, we have done the two types of analysis.

3-1. We have compared TIR and CONTRAIL CME CO2 data with and without TIR CO2 averaging kernel functions over each of the nine airports, and showed the comparison results in Figure 4 of the revised manuscript. Here, we have created CO2 vertical profiles using CME ascending/descending CO2 data below the tropopause and stratospheric CO2 concentrations taken from the Nonhydrostatic Icosahedral Atmospheric Model (NICAM)–Transport Model (TM) (Niwa et al., 2011) that introduced CONTRAIL CO2 data to the inverse model (Niwa et al., 2012), and then applied TIR CO2 averaging kernel functions to the created profiles.

3-2. Keeping the detailed evaluation of 2-1 in mind, we assumed a CO2 vertical profile on the basis of the combination of CONTRAIL CME level flight CO2 data ("CONTRAIL (raw)") and CarbonTracker CT2013B monthly-mean CO2 profiles (Peters et al., 2007) at each of the CME level flight measurement locations, applied TIR CO2 averaging kernel functions to the assumed profiles, and then compared the CO2 data with averaging kernels ("CONTRAIL (AK)") with TIR CO2 data in the UTLS regions. In Figure 5, 6, and 7 of the revised manuscript, we showed the comparison results of both the CONTRAIL (raw) and CONTRAIL (AK) data. We have explained the methods of the comparisons in Section 5, and showed the comparison results in Section 6 in the revised manuscript.

4. We have eliminated Figure 5 of the original manuscript. This is because we have evaluated the effect of considering TIR CO2 averaging kernel functions on TIR and

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CONTRAIL CME CO_2 comparisons quantitatively in the revised manuscript.

5. Following the suggestion of Referee #1, we have showed the comparison results for each latitude band, instead of showing the comparison results for each airline route, in Figure 7 of the revised manuscript. We have also modified Table 2 to show the bias values of TIR CO_2 data against CONTRAIL (AK) CO_2 data.

6. We have eliminated Figure 10 of the original manuscript to avoid speculative discussion.

Individual responses to the two Referees' comments are listed below.

Reply to Referee #1

- Any improvement in our ability to monitor CO_2 from space is important. In particular, being able to use both spectral ranges of TANSO-FTS would increase the vertical understanding in atmospheric CO_2. In that sense, it is important to validate precisely TANSO-FTS thermal infrared (TIR) CO_2 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 validation part.

Reply:

This paper has focused on UTLS CO_2 concentrations to which the thermal infrared (TIR) sensor of TANSO-FTS has highest sensitivity. In order to validate the TIR UTLS CO_2 data, we have compared them with more than 500,000 CONTRAIL CME level flight CO_2 data obtained in a wide area shown in Figure 2 of the revised manuscript. As described above, we have utilized CarbonTracker CT2013B monthly-mean CO_2 profiles to bridge the differences of vertical resolution between in-situ CONTRAIL CME and satellite TANSO-FTS TIR measurements, and compared them more quantitatively

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in the revised manuscript. As for the algorithm part, we have clearly explained the improvement of V1 algorithm from the previous algorithm of Saitoh et al. (JGR, 2009), and furthermore described the impact of the improvement regarding the simultaneous retrieval of surface parameters on the UTLS CO_2 data in Section 4 of the revised manuscript.

- 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.

Reply:

We agree with your comments. In the revised manuscript, we have used CarbonTracker CT2013B monthly-mean CO_2 profiles (Peters et al., 2007) to assume a CO_2 vertical profile at each of the CME level flight measurement locations. Then, we applied TIR CO_2 averaging kernel functions to the assumed profiles to smooth them to the vertical resolution of TANSO-FTS TIR observations, and defined them as CONTRAIL (AK). In the original and revised manuscripts, we have compared the averages of TIR CO_2 data in two or more retrieval layers (layers 9, 10, and 11, or layers 9 and 10, or layers 10 and 11), not in a single layer, with CONTRAIL CME data. In the revised manuscript, we have extracted CONTRAIL (AK) data that corresponded to the TIR retrieval layers where TIR CO_2 data were compared with original CONTRAIL CME data ("CONTRAIL (raw)"), and compared their averages with the TIR CO_2 averages in the several layers.

- 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

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impact of not taking AK into account. But no quantitative result, and too many vague statement (‘relatively 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.

Reply:

We appreciate your suggestions. First of all, we have compared the averages of TIR CO2 data in two and more retrieval layers (layers 9, 10, and 11, or layers 9 and 10, or layers 10 and 11) with CONTRAIL CME data, so the 1-σ values of the averages in Figures 5 and 6 of the revised manuscript show the variability of CO2 concentrations in these UTLS layers. In the revised manuscript, we have stated this point clearly. We have divided Section 6 of the revised manuscript into two parts. In the first part (ii), we have showed the comparisons between CONTRAIL (raw) and CONTRAIL (AK) data in terms of their differences of TIR CO2 data over the nine airports. In the comparison of this part, we have created CO2 vertical profiles using CONTRAIL CME ascending/descending CO2 data below the tropopause and stratospheric CO2 concentrations taken from NICAM-TM simulations (Niwa et al., 2012), and applied TIR CO2 averaging kernel functions to the created profiles. In the second part (i & iii), we have done the comparisons among the averages of TIR, CONTRAIL (raw), and CONTRAIL (AK) CO2 data with their 1-σ values. The differences of the 1-σ values between the CONTRAIL (raw) and CONTRAIL (AK) averages could be a measure of the variability of CO2 concentrations in the UTLS regions corresponding to layers 9, 10 and 11, because the CONTRAIL (raw) data were obtained at a single altitude level, but CONTRAIL (AK) data merged CO2 data in these UTLS layers. The 1-σ values of TIR CO2 data were always larger than those of CONTRAIL (AK) CO2 data, which means that TIR CO2 data had larger variability in these UTLS layers.

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

Reply:

We agree with you. In the revised manuscript, we have discussed all the results quantitatively. We appreciate your comment.

- On another points, the tentative explanation of the biases seem unconclusive. Several 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 bias 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 biases.

Reply:

We basically agree with your comments. We have regarded TANSO-FTS TIR L1B spectral uncertainty, a priori uncertainty, temperature uncertainty, and surface param-

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eter uncertainty as a candidate of the main causes of TIR L2 CO_2 bias against CONTRAIL CME CO_2 data. First, retrieving surface parameters simultaneously instead of using initial surface parameters did not affect CO_2 concentrations in the UTLS regions in the TIR V1 CO_2 retrieval. Then, we compared simultaneously retrieved temperature profiles with a priori JMA GPV temperature profiles in the UTLS region, and did not find any difference between the two which could explain the largest TIR CO_2 negative bias in the northern low and middle latitudes in spring and summer. We did not find any connection between their differences (Reference figure 1) and the magnitude of the TIR L2 CO_2 negative bias. We agree that the quality of retrieved temperature profiles should be also evaluated by other datasets. In the UTLS regions, temperature variability is relatively large, and therefore comprehensive validation analysis of both the a priori and retrieved temperature profiles should be required using reliable and independent temperature data such as radiosonde data to draw a conclusion. In the revised manuscript, we have evaluated the effect of L1B spectral bias on retrieved CO_2 concentrations, if the V161.160 spectra had the same bias as V130.130 L1B spectra reported in Kataoka et al. (2014). Please see the first three paragraphs of Section 7 of the revised manuscript for further details.

- 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.

Reply:

As described above, we have showed the comparison results for each latitude band, instead of showing the comparison results for each airline route, in Figure 7 of the revised manuscript, to show the latitudinal dependence of the bias of TIR CO_2 data against CONTRAIL CME CO_2 data. We have also modified Table 2 to show the bias of TIR CO_2 data against CONTRAIL (AK) CO_2 data, so that it should be useful for users to correct the TIR CO_2 data.

Specific comments:

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

Reply:

In the revised manuscript, we have clearly stated "TIR CO_2 bias against CONTRAIL CME data." As for the TANSO-FTS L1B spectra, we have changed "accuracies" to "biases" to show clearly that the L1B spectra have biases against radiance spectra observed with S-HIS reported in Kataoka et al. (2014).

Section 4. Retrieval algorithm:

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

Reply:

We used all the channels included in the wavelength regions of 690–750 cm-1, 790–795 cm-1, 930–990 cm-1, and 1040–1090 cm-1 in the V1 CO_2 retrieval processing. We did not adopt any channel selection. In the revised manuscript, we have clearly stated this point.

- 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.

Reply: Following your comments, we have presented the three cases of averaging kernel functions: low latitudes in summer, mid-latitudes in spring, and high latitudes in winter in Figure 1 of the revised manuscript. The degrees of freedom in the three cases were 2.22, 1.81, and 1.36, respectively.

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

Reply:

The a priori and initial values for surface emissivity were calculated by linear regression analysis using the Advanced Space-borne Thermal Emission Reflection Radiometer (ASTER) Spectral Library (Baldridge et al., 2009) using land-cover classification, vegetation, and wind speed information. We had missed this information in the original manuscript. We appreciate your comment.

- In Section 4.3, the authors state that ‘The existence of a relatively large spectral bias around the CO2 15 μm absorption band in 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?

Reply:

This is probably because TANSO-FTS L1B spectral bias in the CO2 15 μm absorption band was sometimes too large for the L2 retrieval calculation to converse in a limited iteration.

- 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?

Reply:

Simultaneous retrieval of surface emissivity did not affect retrieved CO2 concentrations in the UTLS regions, and did not contribute to increasing the number of normally retrieved CO2 data. In the V1 algorithm, including surface emissivity in the state vectors did not matter in the TIR CO2 retrieval and UTLS CO2 data.

- 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

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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.

Reply:

Following your suggestion, we have modified Figure 5 and Figure 6 of the revised manuscript (Figures 6 and 7 of the original manuscript). As described above, we have greatly modified Figure 7 of the revised manuscript (Figure 8 of the original manuscript), showing the comparison results for each latitude band, instead of showing the comparison results for each airline route. It should be easier for readers to see the differences.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C5679/2016/amtd-8-C5679-2016-supplement.pdf

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