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Atmospheric
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Discussions

Interactive comment on "Preliminary validation of column-averaged volume mixing ratios of carbon dioxide and methane retrieved from GOSAT short-wavelength infrared spectra" *by* I. Morino et al.

Anonymous Referee #2

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The present manuscript aims at the validation of atmospheric CO2 and CH4 total column concentrations retrieved from solar backscatter measurements by the TANSO-FTS aboard GOSAT. The satellite retrievals are compared to ground-based observations by the TCCON network. The topic is highly relevant for researchers working on remote sensing of greenhouse gases from space and inverse modelling of the respective sources and sinks. Therefore, the study is suitable for publication in AMT.

However, the paper requires revisions – in particular a more thorough discussion of the results. In its present shape, the paper is a status report of the current official GOSAT processor. This is a valid topic for AMT in particular since GOSAT is a new, challenging, and promising satellite sensor. Unfortunately, the authors put little effort in going beyond a mere reporting of numbers. More discussion is required on why data quality is still unsatisfactory, on how data quality has been improved in the past, on how data quality can be improved in the future, and on how the validation results relate to GOSAT's overall goal to facilitate inverse estimates of sources and sinks.

Major comments

1. The number of GOSAT data used for comparison with TCCON seems very low (e.g. Fig. 4, 5, 7, and 8). For some of the validation sites, only 2 coincident GOSAT measurements are identified during ∼1 year of satellite operation.

- The authors should investigate if less stringent filtering of the satellite data or relaxed coincidence criteria can help to increase the number of GOSAT data for comparison. For example, I would consider a 30 min coincidence criterion between GOSAT overpass and TCCON measurement rather stringent given that CO2 and CH4 total column concentrations show very little variability on this timescale. The same holds for the spatial variability on ∼1 to 3 degree latitude/longitude.

- In cases with only a few data, it is useless to calculate the error statistics (e.g. Table 2, 3).

2. The paper requires significant more discussion on the results. The paper

finds, that GOSAT CO2 and CH4 retrievals are globally low-biased and that the apparent precision is 1% (as calculated from very few data, see comment 1).

- There is no discussion on why the bias varies from site to site by more than 3% (e.g. Table 2,3). Regionally varying biases are probably the most detrimental issue to inverse modelling of sources and sinks. A global low-bias, however, is not really worrisome given that also the TCCON data are globally scaled to match the in-situ standards.

- The GOSAT retrievals scatter substantially more than the TCCON data. This should be addressed in the discussion. How does the scatter compare to the noise error? Due to the scatter (and due to sparse data coverage), the Northern-hemisphere seasonal cycle of CO2 cannot be clearly identified in time series of individual GOSAT observations (figures 4,5). This is quite disappointing in my opinion.

- Potential reasons for scatter and bias are not discussed except for a very vague and short speculation in section 5. What is the impact of spectroscopy errors in the various bands (O2, CO2, CH4)? What are the instrument related uncertainties? I would expect some detailed information on instrument issues (which have certainly been discovered since launch of the satellite). What is the potential impact of residual aerosol and cirrus interference in particular since retrieved scattering properties are mostly representative for the O2A band, not for the CO2 and CH4 bands? To what extent are some validation sites “easier” than others? Garmisch for example seems a very tough site for satellite validation due to rough topography.

3. Section 4.2 covers global maps of CO2 and CH4 for two months of the year 2009. This is useless for the purpose of validation if no comparison data is shown. Moreover, similar plots have been featured in Yoshida et al., AMTD, 2010. Either remove section 4.2 or evaluate the global distributions against a validation dataset

(e.g. model data).

Further comments

- The introduction is biased toward CO2. I recommend some more introductory discussion on CH4.

- Sections 2 and 4.2 provide a basic overview about the GOSAT instruments and the retrieval method. For the latter, the paper mostly refers to Yoshida et al., AMTD, 2010. While this is a valid reference, I recommend to add some information on the aspects relevant for validation: spectroscopic databases, meteorological input, calculation of mixing ratios from total columns. Maybe a table would help here.

- From section 2, the reader might get the impression that also the thermal infrared and 2-micron CO2 bands are used for retrieval (p.5619, l 13). This should be avoided.

- Section 3.2 is quite short and could be merged into Section 3.1.

- p.5615,l.23: total warming effect -> total anthropogenic warming effect

- p.5617,l.13: several thousand square kilometers -?-> several hundred thousand square kilometers

- p.5621,l.16: the latest retrieved value was overwritten: Why are data overwritten and not averaged?

- p.5622,l.16: Figs. 4 and 5g,h -?-> Figs. 4 and 5

- p.5624,l.13: though a striking difference is seen near 50-60degree N. : I would not see a striking difference given the generally large scatter of the GOSAT data. Further, there is an at least equally ‘striking’ difference in the zonal average CO2. If this is an attempt to hint at high-latitude CH4 emission from wetlands or thawing permafrost, it first needs to be proven that the observed difference is not a retrieval artefact. In my opinion, this has not been achieved here. I recommend to remove the statement.

- Figs. 2 through 11 should be presented on the same CO2 and CH4 scales for easy comparison. The CO2 range [350, 410] ppm used in Figs. 4 and 5, for example, seems rather large and tends to mask detail. The CO2 range [360,400] ppm used in Fig. 6 seems better.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5613, 2010.