Interactive comment on "Improvement of the retrieval algorithm for GOSAT SWIR XCO2 and XCH4 and their validation using TCCON data" by Y. Yoshida et al.
Anonymous Referee #2
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The study by Yoshida et al. collects and evaluates various development steps for the GOSAT XCO2 and XCH4 retrieval algorithm operated at the National Institute for Environmental Studies (NIES), Japan. Since the early version of the algorithm, the authors themselves and others have identified key aspects that enhance accuracy of the greenhouse gas concentrations retrieved from GOSAT. The paper under review demonstrates that these key aspects have been successfully built into the most recent version of the NIES algorithm and that performance of the algorithm is competitive with what has been reported by others [Wunch et al., 2011; Parker et al., 2011; Butz et al., 2011; Oshchepkov et al., 2012]. Thus, the study is of interest to the data user as well as the algorithm development community.
The paper is well written, the employed methods are robust, and – in most cases – due reference is given to the work of others.
Therefore, I recommend publication in AMT after considering some minor comments below:

=> Thank you for your careful reading of our paper and providing constructive comments. The followings are our reply to your comments. The revised part is marked with "double line (══: removed)" or "under bar (___: added)".

- Title, abstract: I recommend that the title and/or abstract identifies the algorithm under investigation as the "NIES algorithm" or the "official GOSAT processor" (or similar) in order to distinguish the presented effort from other GOSAT retrieval algorithms.

=> We add following sentence in the abstract.

The column-averaged dry-air mole fractions of carbon dioxide and methane (XCO_2 and XCH_4) have been retrieved from Greenhouse gases Observing SATellite (GOSAT) Short-Wavelength InfraRed (SWIR) observations <u>and released as a SWIR L2 product from the National Institute for Environmental Studies (NIES)</u>.

- p.952,l.22: of an aerosol optical depth -> of aerosol optical depth

=> Done.

- p.953,l.23: from 30 + km altitude -> from above 30 km altitude

=> Done.

- p.954,l.7: due to the very strong absorption of O2 A-band -> due to overwhelmingly strong telluric O2 absorption in the A-band range

=> Done.

- p.954,l.8: depending on the airmass -> depending on airmass

=> Done.

- section 2.2, figure 3,4,5: Is it correct that the logarithm of the aerosol optical thickness profile is retrieved? If so, this needs to be stated explicitly in section 2.2. Retrieving the logarithm of AOD makes the forward model significantly more non-linear which could result in slow convergence or the retrieval sticking to the prior values. In the view of the latter aspect, it would be helpful to show a figure comparing prior and posterior AOD (eg. by replacing the upper panels of figure 4 with the prior minus posterior difference).

=> We used a logarithm of AOD in Figs. 3 and 4 for clarity. Retrieved parameter for Cases 2-1 and 2-2 (single-layered aerosol scenario) is AOD, not a logarithm of AOD. On the other hand, a logarithm of the aerosol mass mixing ratio profile is retrieved for Case 2-3 (multiple-layered aerosol scenario). We add explanation in the text as follows, but no change is made for figures.

Three retrieval tests were conducted: (Case 2-1) AOD of aerosol with an assumed 2-km thick layer was retrieved with aerosol optical properties based on SPRINTARS V3.54 (same as the SWIR L2 V01.xx), (Case 2-2) same as the Case 2-1 but using SPRINTARS V3.84 optical properties, and (Case 2-3) a logarithm of aerosol mass mixing ratio ~~vertical~~ profile was retrieved with aerosol optical properties based on SPRINTARS V3.84.

- p.954,l.17: AOD are therefore -> AOD is therefore

=> Done.

- p.954,l.24: “However, we had planned to use : : :” It is not clear to me what was “planned” and what was actually “done”. Please rephrase.

=> The sentence was revised as follows.

However, we didn't use this vertical profile information ~~we had planned to use aerosol optical properties derived from TANSO-CAI, which has no sensitivity to the aerosol vertical profile,~~ and assumed aerosols were uniformly distributed within a 2-km layer from the surface in the SWIR L2 retrieval, because we had planned to use aerosol optical properties derived from TANSO-CAI, which has no sensitivity to the aerosol vertical profile.

- p.955,l.10: instead of AOD -> instead of integrated AOD

=> Done.

- p.955,l.10: “Possible reasons for the overestimation : : :” I suggest removing this sentence, since it is redundant to the rationale before.

=> Done.

- p.956,l.6: tendency with CO_2 -> tendency as CO_2

=> Done.

- p.956,l.28: The non-linear response creates an offset -> The non-linear response of the detector recording the interferogram creates a radiance offset in the spectral domaine

=> Done.

- p.957,l.6: no-aerosols -> no aerosols

=> According to comment by Referee #1, we revised this sentence as follows.

To focus on the non-linearity response impact, only ocean data were used and we assumed ~~there to be no-aerosols~~ aerosol free conditions for these scenarios.

- p.958,l.1: The paper should comment on the fact that other groups find different scaling factors for the O2 cross section: 1.030 [Butz et al., 2011], 1.025 [Crisp et al., 2012], surface pressure scaling of 1.004 [Cogan et al., 2012]

=> This scaling factor is utilized to get non-biased retrieved surface pressure. On the other hand, Cogan et al. (2012) normalizes the retrieved XCO2 with the ratio of retrieved surface pressure and ECMWF surface pressure. This approach differs from the scaling factor approach. Therefore, we only add Butz et al. (2011) and Crisp et al. (2012) as follows.

The O_2 absorption cross section scaling factor of 1.01 was evaluated according to the method by Butz et al. (2011) (Fig. 8). Note that different algorithms use different values of this scaling factor: i.e., 1.030 for Butz et al. (2011) and 1.025 for Crisp et al. (2012).

- section 3.1: Could you quantify the typical change in slant airmass when correcting the pointing offset?

=> We quantified the pointing anomaly as follows.

The pointing anomaly was corrected by shifting the pointing angle and this made small changes in the viewing angle (~0.5 deg. for along-track direction and ~0.3 deg. for cross-track direction) and, as a result, the airmass value (~0.01).

- p.959,l.7: are updated according to the update -> are taken from the most recent version

=> Done.

- p.959,l.25: “the chi-squared cost function” Do you refer to term 1 in equation (1) ?

=> We added the explanation as follows.

However, in this case, the chi-squared cost function of the retrieved state ($\chi^2 = J(\mathbf{x})/m$, where m indicates the number of channels used in the retrieval analysis) increased as the SNR increased, indicating that the contribution of the forward model error and/or the model parameter error became large with SNR.

- p.960,l.5: To my knowledge, an empirical noise criterion has been developed by the ACOS team. This should be acknowledged here.

=> We revised as follows.

Because it was hard to evaluate the forward model error and the model parameter error adequately, the contributions of these error components were empirically modeled as a function of SNR (Crisp et al., 2012: note that they use a different formula for empirical noise).

- p.962,l.11: “Furthermore, possible signs : : :” I suggest removing this sentence since it is speculative.

=> Done.

- P.963,l.24 “and those over ocean at present; above comparisons are made for land data using Gain-H”. This sentence reads awkward. Consider rephrasing.

=> We revised as follows.

Unfortunately, the comparison using TCCON data is restricted to land data with Gain-H, because there ~~There~~ is no suitable TCCON site for validating the retrieval results over bright surfaces, where the TANSO-FTS operates in Gain-M, and those over ocean, where the wind speed is retrieved instead of surface albedo ~~at present; above comparison are made for land data using Gain-H~~.

- Table 2, table 1 in supplement: Please add the standard deviation of biases A which is much more interesting than the mean bias.

=> Done. Also, we added Eureka and Karlsruhe TCCON sites in our comparison.

- Fig.10: The vertical bars in -> The error bars in

=> Done.

- Fig.15, upper panels: Looking at the dependence of retrieval error (difference between GOSAT and TCCON) on the difference between retrieved and a priori surface pressure, I would think that using the a priori surface pressure for calculating mixing ratios would improve accuracy substantially.

=> Some part of the bias could be removed by using the a priori surface pressure instead of the retrieved one, however, it might produce additional spatiotemporal bias because there exists strong correlation between the retrieved surface pressure, aerosol profile, and surface albedo. More precise analysis is needed to obtain the adequate empirical bias correction formula, therefore, we decided to use the retrieved surface pressure at this time.